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

Germain Gauthier*

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**Abstract**

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. I apply this novel methodology to the police records of large US cities and study the Me Too movement's effects on sex criminality. 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 persistent and positive impact on victim reporting. The increase in reporting translates into 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 deterrent effect.

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

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

*germain.jean.gauthier@gmail.com, Bocconi University, Department of Social and Political Sciences. 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, Ro'ee Levy, Martin Mattsson, Paola Profeta, Paolo Pinotti, Pauline Rossi, Arne Uhlendorff, 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 Zürich, Sciences Po, University of Warwick, University of St. Gallen, Bocconi University, Erasmus University, University Paris-Nanterre, HEC Paris, and HEC Lausanne. This research was 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 (Planty et al., 2013). In turn, a long-standing empirical challenge has been interpreting variations in reported crimes as changes in the number of offenses committed or victims' propensity to report (Quêtelet, 1831). This paper develops a 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 start by formalizing the econometric issues raised by victim underreporting. While researchers commonly regress reported crime counts on a policy indicator to estimate treatment effects, this approach is valid only if the timing of the policy is uncorrelated with reporting propensity (Levitt, 1998). This is a convenient but somewhat implausible assumption for at least two reasons. First, crime policies typically affect both actual crime rates and reporting behavior. Second, I show that if the crimes studied are reported with a delay relative to the date of the incident, the timing of the policy *necessarily* correlates with the propensity to report of victims – even absent any true policy effects. This spurious correlation arises because victims of crimes committed closer to the end of the study window have less time-to-report and are thus less likely to be observed. These results highlight the limitations of current empirical approaches, particularly for crimes with significant reporting delays. Such crimes include harassment, sexual and domestic violence, and corruption cases, for instance.

Fortunately, though delayed reports complicate the analysis of police databases, they also provide researchers with additional information on victims' propensity to report. The stock of crimes committed at a time $t$ is fixed and progressively reported to the police in subsequent periods. By construction, it cannot be affected by future policies or interventions that may change crime incidence. Thus, following a policy intervention, unusual variations in delayed reports can safely be interpreted as changes in the propensity to report crimes. Building on this intuition, I develop a mixed proportional hazards (MPH) duration model to analyze delayed reports over time. The model treats victims as entering the study at their incident date and exiting upon police reporting, while explicitly accounting for those who never report. This latter group requires specifying a researcher prior – representing the hypothetical share of permanent non-reporters for crimes committed in the initial study period. While this parameter isn't estimated from the data, one

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

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

can easily compute the range of plausible values for which treatment effects remain significantly different from zero.[3] The resulting model reconstructs the evolution of victim reporting rates over time. In turn, I can decompose the observed time series of reported crimes into its two unobserved components: crime incidence and victim reporting.

I estimate the model by maximum likelihood. The estimation is complicated by double-truncation in the data – a non-trivial sample selection scheme that arises because I only observe plaintiffs who report a crime to the police during the study window. 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 time-invariant observables and the history of interventions affecting victim reporting (e.g., #MeToo). In Monte Carlo simulations, the proposed estimator largely outperforms the naive estimator, with no apparent bias in estimates, and appropriately recovers victim reporting over time (and thus crime incidence, too).

I then take the model to the data. I use police records of New York City, Los Angeles, Cincinnati, and Seattle between 2011 and 2019. My goal is to estimate trends in crime reporting and incidence surrounding the Me Too movement's intense mediatization. Descriptive statistics underscore the prevalence of delayed reporting and its relevance for empirical analysis. Over half of all charges are filed after the incident date, with delays ranging from days to decades. The average delay between an incident and its report is 197 days. Turning to #MeToo, I find two pieces of raw empirical evidence indicating an increase in victims' propensity to report. Average reporting delays aggregated at the report date steadily increase over the decade and spike after the movement's mediatization. This suggests an increased willingness of victims to come forward to report past crimes. In line with this intuition, the visual inspection of the hazard of reporting pre-#MeToo incidents shows a sizable increase in the movement's aftermath.

I estimate my duration model to quantify this increase in reporting. In line with victimization survey estimates, I assume that 70% of the victims would not have reported incidents for 2011. Mirroring the raw empirical evidence, I find that reporting rates increased linearly between 2011-2017, before #MeToo. While this challenges the notion of #MeToo as a complete turning point, the movement did significantly accelerate this trend and appears as a structural break in the time series. Quantitatively, I estimate that the share of victims who eventually report a sex crime to the police more than doubled over the decade, reaching 72%, with #MeToo accounting for approximately 23% of this increase.

[3] In the application, I show that parameter estimates are robust to the choice of this parameter for a wide range of plausible values (see Online Appendix Figure C.1.)

Importantly, alternative values for the share of never-reporters in 2011 do not revert these trends but affect their magnitude. I show that my main results are robust to a broad range of plausible parameter values. In fact, a model that assumes that all victims eventually report to the police – and that by construction relies on the observable part of the distribution of times-to-report – still uncovers the pre-trends in reporting and the movement's impact. I also uncover two essential margins of heterogeneity. I find that #MeToo had a particularly large effect on juveniles, Blacks, and Hispanics – suggesting the movement reached some of the most vulnerable groups of the victim population. Furthermore, it had a disproportionate impact on victims of very old crime incidents (i.e., crimes reported more than five years after the incident).

I then reconstruct the time series of sex crime incidence based on estimated reporting rates. My estimates indicate that sex crime incidence decreases by approximately 35% between 2011 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 28% 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 sequentially dropping control groups, 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 8% of the decrease in sex crime incidence over the period.

Interestingly, my estimates are consistent with an alternative decomposition exercise performed at the national level and based on homicides that are sexual in nature (and which do not suffer from underreporting issues). However, they run counter to the estimates of other common approaches to disentangling crime reporting and incidence. For instance, the National Crime Victimization Survey (NCVS) suggests sex crime incidence largely increased following #MeToo's intense mediatization, and that victim reporting of such crimes remained relatively stable. Emergency department visits also suggest an increase in sex crime incidence over the period. This raises concerns regarding the reliability of these widely used data sources to disentangle both margins.

In the last part of the paper, I consider several channels that may explain my results. Unfounded allegations, changes in the legal definitions of sex crimes, or in their statutes of limitations do not account for these patterns. A first plausible explanation is a social norm narrative in which the social cost of reporting (committing) a sex crime has decreased (increased). This is consistent with the increase in sexual violence awareness that can be seen in Google searches and Twitter data. A second plausible explanation runs through a crime deterrence channel. The reporting rate has increased the probability of arrest for sex offenders, from roughly 15% in 2011 to 37% in 2019. In the data, a one percentage point increase in the probability of arrest is associated with a 0.9 percentage point decrease in sex crimes.

This paper adds to the literature on the reliability of reported crime statistics. Since the $19^{th}$ century, scholars refer to the share of crimes that are neither reported to nor recorded by law enforcement agencies as the *dark figure of crime* (Coleman and Moynihan, 1996).[4] To this day, unobserved crimes pose a serious empirical challenge for analyzing and interpreting police records. 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 incidence and reporting. A few recent contributions in economics have relied on proxy variables to disentangle both margins (Stephens-Davidowitz, 2013; Bellégo and Drouard, 2019). These frameworks implicitly or explicitly assume crimes are reported to the police in short periods or never at all. This is not the case for sex crimes, domestic violence, harassment, and corruption cases, among others. I contribute to this literature in three ways. 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. Third, I provide real-world evidence that underreporting is a serious empirical threat for practitioners and a first-order concern for the credible impact evaluation of interventions to fight crime and/or increase victim reporting.

This paper also speaks to the broad literature on gender-based violence. Underreporting is an essential aspect of gender-based violence because the monitoring and punishment of unlawful behaviors depend to a large extent on the willingness of victims to report incidents to law enforcement agencies (for theoretical perspectives, see Lee and Suen, 2020; Cheng and Hsiaw, 2022; Acemoglu and Jackson, 2017). Historically, the election of female politicians and the number of female officers in the police workforce has increased the reporting of gender-based violence in India (Iyer et al., 2012; Miller and Segal, 2019). Highly mediatized affairs (and the resulting public outrage), 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; Colagrossi et al., 2023). I contribute to this literature in two ways. First, the duration modeling approach developed in this paper is particularly well-suited to the analysis of gender violence because such crimes are frequently reported with significant delays. Second, my results highlight that social movements are important in moving away from socially undesirable equilibria where offenses are common, but their reporting is infrequent.

In doing so, I also contribute to the nascent literature that specifically studies the effects of the Me Too movement. As a global social movement, #MeToo is likely the largest public awareness campaign against sexual violence in history. Previous studies have shown that public allegations of sexual misconduct substantially impacted company valuations on financial markets (Borelli-Kjaer

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

et al., 2021) and various labor markets – including entertainment (Luo and Zhang, 2022), venture capital (Sophie Calder-Wang and Sweeney, 2021), mutual funds (Cici et al., 2021), and academia (Gertsberg, 2022), among others (Batut et al., 2021). Closely related to this paper, Levy and Mattsson (2023) provides a comprehensive analysis of the effects of the Me Too movement on reported crimes to the police in a large sample of OECD countries and document that individuals perceived sexual misconduct to be a more serious problem following the Me Too movement. Chen and Long (2024) shows that the movement's effect on sex crime reports was primarily concentrated in US regions with historically lower levels of sexism. Previous estimates of the movement's impact focused on reported crimes to the police as their primary outcome. This paper goes one step further and disentangles the relative contributions of victim reporting and crime incidence in explaining the observed increase in reported sexual crimes to the police. Both margins are shown to be empirically relevant, suggesting the movement impacted victims and offenders.

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). 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. Unobserved heterogeneity is common in practice, and modeling it can be 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. Specifically, I relax the unconditional independence assumption to a more realistic *conditional* independence assumption and extend the model to unobserved heterogeneity. The methodology I develop has many potential applications beyond police data. Double-truncation is a sampling scheme that arises in biostatistics and epidemiology (Lagakos et al., 1988; Moreira and de Una-Alvarez, 2010; Emura and Murotani, 2015), engineering (Ye and Tang, 2016), astronomy (Efron and Petrosian, 1999), and economics (Dörre, 2020).

This paper is organized as follows. Section 2 discusses the econometric issues related to victim underreporting. Section 3 outlines a general method to infer variations in crime incidence and reporting from police records with delayed reports. Section 4 presents estimates of the Me Too movement's effects on victims and offenders. Finally, Section 5 concludes.

## 2 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 formalizes the empirical issues researchers encounter when studying police data. I focus specifically on the large share of crimes that are not reported or reported with a delay and their consequences for causal inference.

## 2.1 The Canonical Problem

Consider an analyst disposing of reports recorded by the police between $\tau_1$ and $\tau_2$, 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

$$R_t = r_t \times C_t. \tag{1}$$

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

$$\log(C_t) = a + bD_t + \varepsilon_t - \log(r_t), \tag{2}$$

where $a$ is an intercept term, $b$ is the coefficient associated with $D_t$, and $\varepsilon_t$ 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. One would expect crime rates to drop but also victims to increase their reporting rate as their odds of "seeing justice served" increase.[5] Despite these obvious pitfalls, and for lack of a better alternative, regressions taking log counts of reported crimes as an outcome variable remain the default approach in the crime literature.

## 2.2 The Econometric Implications of Delayed Reports

Equation 1 assumes the absence of delayed reports. In practice, however, delayed reports are common in police data (see Section 4.1). These reports complicate the analysis of reported crimes, because 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 of their occurrence will generally

[5] In some cases, the analyst is interested in understanding the impact of the policy on the victim reporting rate $r_t$. In this case, $C_t$ acts as the omitted variable, and similar issues may arise. If a public awareness campaign encourages victims to file complaints to the police, this mechanically increases the probability of arrest of offenders, and should ultimately lower crime rates.

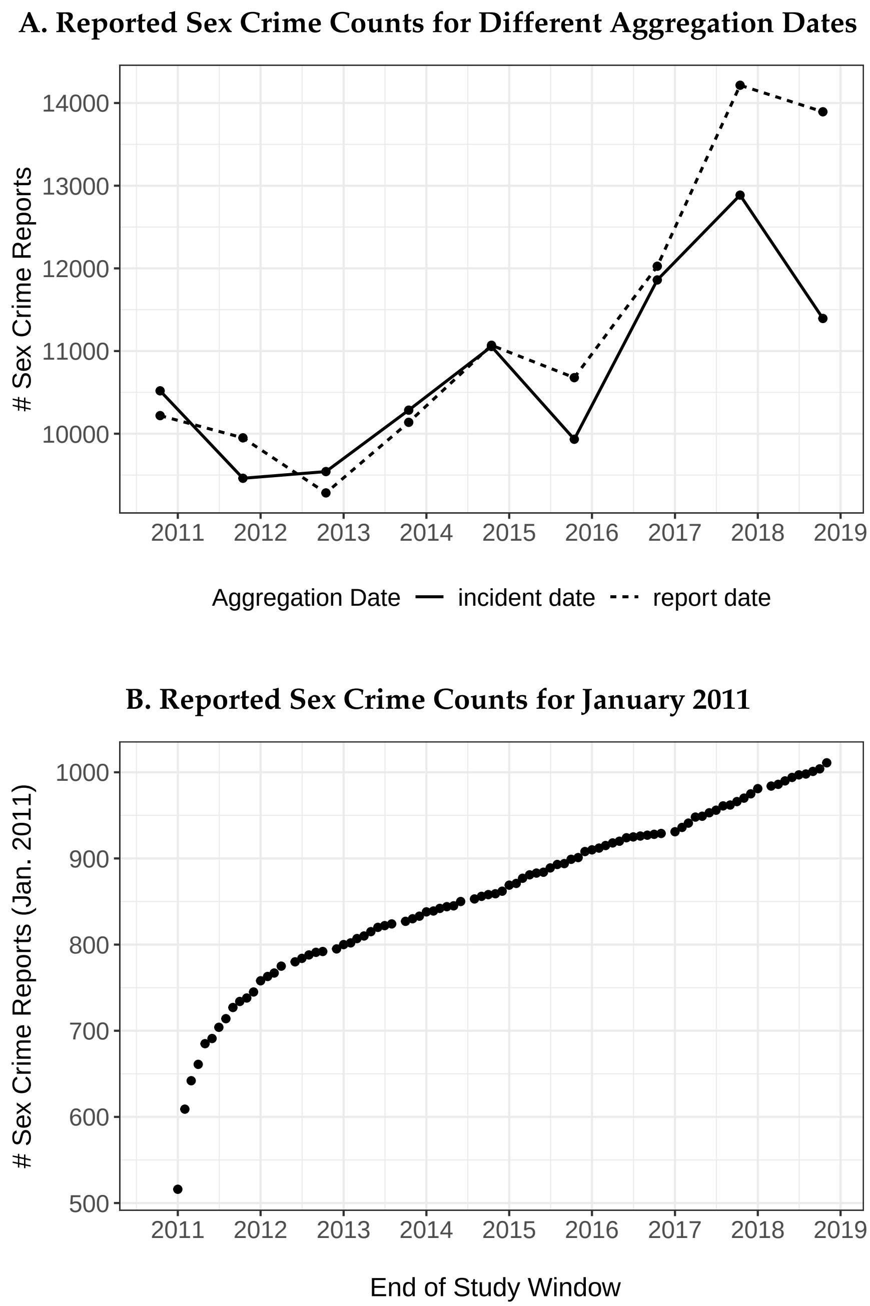



Figure 1: Delayed reports make reported crime statistics unreliable.

*Notes:* This figure demonstrates issues with police records with delayed reports. Panel A presents reported sex crime counts for different aggregation dates. The solid line aggregates reports by incident date. The dashed line aggregates reports by the report date. Overall, depending on the study window, counts may vary substantially. Panel B presents reported sex crime counts for the same incident date but different study periods. Each data point corresponds to reported sex crime counts for January 2011 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, and may report outside of the study window.

produce very different time series. Furthermore, researchers now face a sample selection issue, as crimes reported outside of the study window are unobserved (because they have not been reported yet at the time of data collection).

I briefly illustrate these empirical issues based on the police records later used to study the Me Too movement (see Section 4.1). Panel A of Figure 1 plots reported sex crimes aggregated at the incident or the report date. Counts markedly vary across measures – in some years by multiple thousands – particularly at the end of the study window. Panel B of Figure 1 plots reported sex crime counts for the same incident date (January 2011) but different cutoff dates for the end of data collection (ranging from January 2011 to January 2019). Counts are clearly dependent on the study window considered, with a downward bias on the counts of incidents occurring closer to the end of the study window. Once again, these differences are not benign: the number of incidents recorded for January 2011 varies between less than 600 and well above 1000 depending on the data collection dates.

I now turn to the econometric implications of these observations. First, 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 $Y$ and $\chi_{t_1,t_2}$ the history of interventions between dates $t_1$ and $t_2$. Then $R_t$ is a sum over all previous dates $j \leq t$, where each term is the product of the number of crimes committed in period $j$ and the probability of a victim reporting an incident $t-j$ periods later:

$$R_t = \sum_{j=0}^{t-1} f(t-j \mid \chi_{0,t-1}) \times C_j. \tag{3}$$

Turning to the linear regression analysis, we have

$$\log\Big(\sum_{j=0}^{t-1} f(t-j \mid \chi_{0,t-1}) \cdot C_j\Big) = \alpha + \beta_1 D_t + \epsilon_t.$$

There is a sum in the log transform, which makes it very difficult to measure the effect of the intervention $D_t$ on crime incidence $C_t$ with this outcome variable.

Next, consider the date of the incident for the analysis of reported crime statistics. In this case, one can extend Equation 1 to account for delayed reports. Let $R_{t,\tau_1,\tau_2}$ denote the number of crimes committed in period $t$ that were reported between $\tau_1$ and $\tau_2$, and $F$ the cumulative distribution function of times to report $Y$. $R_{t,\tau_1,\tau_2}$ satisfies

$$R_{t,\tau_1,\tau_2} = p_{t,\tau_1,\tau_2} \times C_t, \tag{4}$$

where $p_{t,\tau_1,\tau_2}$ is simply the probability of reporting a crime that occurred at date $t$ within the study period:

$$p_{t,\tau_1,\tau_2} = \begin{cases} F\left(\tau_2 - t \mid \chi_{t,\tau_2}\right) - F\left(\tau_1 - t \mid \chi_{t,\tau_1}\right) & \text{if } \tau_1 > t, \\ F\left(\tau_2 - t \mid \chi_{t,\tau_2}\right) & \text{if } t \geq \tau_1. \end{cases}$$

The omitted variable bias in the classical regression analysis remains:

$$\log(C_t) = a + bD_t + \varepsilon_t - \log(p_{t,\tau_1,\tau_2}).$$

Contrary to $R_t$ in Equation 3, $R_{t,\tau_1,\tau_2}$ depends on the study period considered, because $p_{t,\tau_1,\tau_2}$ is a function of $\tau_1$ and $\tau_2$. In fact, $p_{t,\tau_1,\tau_2}$ is mechanically correlated to $D_t$. For instance, the closer $t$ is to the end of the study period $\tau_2$, 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 $R_{t,\tau_1,\tau_2}$. As a result, in the classical regression framework, estimates of the marginal effect of $D_t$ will *necessarily* be biased. They will notably be biased even if the intervention $D_t$ has no effect on crime incidence $C_t$ or crime reporting $p_{t,\tau_1,\tau_2}$. This is clearly illustrated in Panel B of Figure 1. 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, as many victims simply have not been reported yet by the end of data collection and are thus unobserved.

# 3 Methodology

In this section, I develop a methodology to directly estimate $p_{t,\tau_1,\tau_2}$ (the probability of reporting a crime that occurred at date $t$ within the study window). The core idea is to model the time-to-report a crime to the police with a duration model, which will provide an estimate of $F$ (the cumulative distribution function of times to report $Y$), which is all that we require to estimate $p_{t,\tau_1,\tau_2}$. In turn, plugging $\widehat{p}_{t,\tau_1,\tau_2}$ into Equation 4 and rearranging terms provides a direct estimate of $C_t$ (crime incidence at time $t$).

I proceed in three steps. I first discuss the duration model and its assumptions. I then derive the model's likelihood for estimation. Finally, I assess the performance of my estimate in finite samples via Monte Carlo simulations.

In what follows, I write random variables in uppercase and their realizations in lowercase.

## 3.1 A Duration Model of Time-to-report to the Police

Let $(Y, X)$ denote a random vector where $Y \in \mathbb{R}^+$ is the time-to-report of a plaintiff and $X \in \mathbb{R}^d$ contains observed covariates. For individual $i$, victim of an incident at time $t$, I wish to model the hazard of reporting a crime to the police $y$ days after the incident occurred as a mixed proportional hazards (MPH) duration model:

$$h_{it}(y \mid \gamma_i,\, \boldsymbol{x_{ity}}) = h_0(y)\exp\left(\boldsymbol{\beta'}\boldsymbol{x_{ity}}\right)\gamma_i. \tag{5}$$

$h_0 : \mathbb{R}^+ \rightarrow \mathbb{R}^+$ is the baseline hazard and models the influence of time since the incident occurred on the probability of report. $\boldsymbol{\beta'} \in \mathbb{R}^d$ is the vector of regression coefficients and captures covariate effects on the probability of report. Note that covariates $\boldsymbol{x_{ity}}$ may be time-varying. In such cases, their values depend on the calendar incident date $t$ and the duration $y$ (e.g., if $t + y \geq$ Oct.2017, then $\text{MeToo}_{ity} = 1$, otherwise $\text{MeToo}_{ity} = 0$). $\gamma_i \in \mathbb{R}$ is a time-invariant random effect to account for potential unobserved heterogeneity across individuals.

The core parametric assumption of this model is the multiplicative decomposition between the baseline hazard, the effect of observed covariates, and the effect of individual-specific unobserved heterogeneity (Van den Berg, 2001).[6] When $\gamma_i = 1$ for all observations, the model boils down to the canonical Cox model (Cox, 1972).

In a setting where we observe all victims, it is straightforward to estimate the hazard of reporting a crime to the police with Equation 5. However, since many victims do not come forward and are thus unobserved, I account for never-reporters explicitly by enforcing the baseline hazard function as a density function and adding an intercept to the regression model:

$$h_{it}(y \mid \gamma_i,\, \boldsymbol{x_{ity}}) = f_0(y)\exp\left(\alpha + \boldsymbol{\beta'}\boldsymbol{x_{ity}}\right)\gamma_i, \tag{6}$$

where $f_0 : \mathbb{R}^+ \rightarrow \mathbb{R}^+$ is a proper density function that acts as the baseline hazard[7] and $\alpha$ accounts for the share of victims who will never report at baseline. In the simplest case of no time-varying covariates, the baseline proportion of victims who will never report a crime to the police is then[8]

$$\lim_{y\to\infty} S_i(y \mid \gamma_i, \boldsymbol{x_i}) = \exp\Big(-\gamma_i \exp(\alpha + \boldsymbol{\beta'}\boldsymbol{x_i})\Big).$$

Note that this modification is benign and does not change the underlying assumptions of the model. In fact, in a setting where we observe all victims, Equations 5 and 6 produce numerically similar estimates for $\beta$.[9] However, in a setting where we do not observe never-reporters, Equation 6 provides a convenient way to "plug-in" the share of never-reporters at baseline via $\alpha$.

[6] MPH models can be derived from economic theory if one is willing to assume myopic individuals (Van den Berg, 2001). In Online Appendix Figure C.2, I show that the hazard of reporting a crime does not spike around the maximum time-to-report a crime to the police (as defined by the statutes of limitations). This is suggestive that victims display myopic behavior.

[7] I will 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., $F, h, f, S$).

[8] See Online Appendix Section B for the extension to time-varying covariates.

[9] *Proof:* Abstracting from time-varying covariates, Equation 6 implies the following baseline survival function: $S_i(y|\gamma_i, \boldsymbol{x_i}) = \exp\left(-\gamma_i \exp(\boldsymbol{\beta'}\boldsymbol{x_i})\exp(\alpha)F_0(y)\right)$. Similarly, Equation 5 implies: $S_i(y|\gamma_i, \boldsymbol{x_i}) = \exp\left(-\gamma_i \exp(\boldsymbol{\beta'}\boldsymbol{x_i})H_0(y)\right)$. The baseline cumulative hazard $H_0$ is left unspecified, so it can also take the form $\exp(\alpha)F_0(y)$.

The model has a theoretical interpretation as a standard Poisson counting process (see Online Appendix Section B.2). It incorporates three essential features to model victim reporting: (i) a share of victims report with a delay, (ii) a share of victims will never report, (iii) covariates may influence reporting delays, and ultimately, the share of victims who will never report (e.g., indicators for policy interventions or public awareness campaigns such as #MeToo).

To build some intuition about its inner workings, consider a setting where a researcher is interested in the effect of a policy implemented in $t^*$. Let $D_{ity}$ be an indicator for the policy, which equals 1 if $t + y \geq t^*$, and 0 otherwise. She estimates the following regression:

$$h_{it}\big(y \mid \gamma_i,\, D_{ity}\big) = f_0(y) \exp\big(\alpha + \beta D_{ity}\big)\gamma_i. \tag{7}$$

Ultimately, the model is comparing $h_{it}\big(y \mid \gamma_i,\, D_{ity} = 0\big)$, the hazard of reporting $y$ days later before the policy, to $h_{it}\big(y \mid \gamma_i,\, D_{ity} = 1\big)$, the hazard of reporting $y$ days later after the policy. We do not observe the number of never-reporters before and after the policy. However, the researcher may be willing to take an educated guess on the share of never-reporters before the policy via the plug-in parameter $\alpha$. For a fixed $\alpha$, the model infers the probability of reporting a crime to the police for every time-to-report $y$ before the policy. Now consider crimes committed in period $t'$, exactly $n$ days before $t^*$. These $n$ days inform the model about the total share of victims in period $t'$, since the cumulative distribution function of times-to-report before the policy, $F(n \mid \gamma_i,\, D_{ity} = 0)$, is known. In turn, this implies that $h_{it}\big(y \mid \gamma_i,\, D_{ity} = 1\big)$ can be computed for $y \geq n$. Ultimately, conditional on a given $\alpha$, $F(n \mid \gamma_i,\, D_{ity} = 1)$ is identified for all $n \geq 2$, so that the parametric assumptions of the hazard function are only useful to extrapolate $F(1 \mid \gamma_i,\, D_{ity} = 1)$. Online Appendix Figure B.2 provides a graphical intuition of the role of $\alpha$.

### 3.2 Likelihood

I now turn to the estimation of the model. 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 (see Online Appendix B.1 for a graphical intuition). 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). The Monte Carlo simulations will make this very apparent, but I also provide an example based on real data in Online Appendix Figure B.3.

Formally, let $T$ denote the incident date, $Y$ the time-to-report to the police, $U = \tau_1 - T$ the left-truncation time, and $V = \tau_2 - T$ the right truncation time. Recall that $\tau_1$ and $\tau_2$ are, respectively, the start and end of the study period. Note also that $V = U + d$ where $d = \tau_2 - \tau_1$.[10] Finally, let

[10] In 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.

$f$ 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

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

This likelihood is complex, but under the assumption of independence between $Y$ and $U$, one can decompose it into two, somewhat more tractable conditional likelihoods:

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

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[11]

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

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.[12] If specified, random effects are integrated out.

[11]Under the nonparametric setting, Shen (2010) show that the MLE based on $L_n(f)$ and the MLE based on $L_n(f, g)$ give an equivalent estimator for $f$. This suggests that $L_n(f)$ contains sufficient information about $f$ for maximum likelihood estimation.

[12]Recently, 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 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

I estimate the models by full maximum likelihood.

For further details, see Online Appendix Section B.

### 3.3 Monte Carlo Simulations

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 package *FrailtyEM* (Balan and Putter, 2019). The package handles right-censoring and left-truncation but does not handle right-truncation. If right-truncation presents a serious empirical issue in our context, this implementation should return biased estimates.

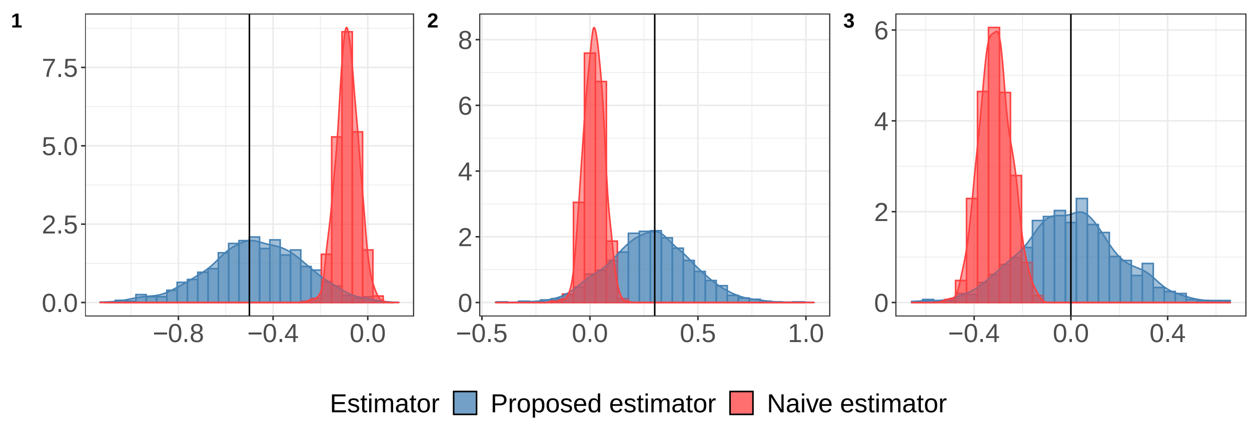



Figure 2: Densities of Estimates of Monte Carlo Simulations

*Notes:* Results of 1000 Monte Carlo simulations. 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. SPanel 1 is for the first intervention with effect -0.5. Panel 2 is for the second intervention with effect 0.3. Panel 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. The densities of estimates of the proposed estimator are centered around the true parameter value, whereas the naive estimator is biased.

I simulate many time series of crime reports over 200 periods (see Equation 6). 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

hazard. To limit the impact of this parametrization, I specify flexible, piece-wise, constant baseline hazards.

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 Figure 2. While the out-of-the-box MPH regression estimator is severely biased, my proposed estimator shows no apparent bias in estimates. Interestingly, even with relatively large amounts of unobserved heterogeneity, estimates of Equation 6 often suggest the absence of heterogeneity in the victim population. If anything, this suggests that the random effects are weakly identified in the data for reasonable numbers of observations (in the tens to hundreds of thousands). Thus, despite unobserved heterogeneity in the simulated datasets, I do not specify a random effect in the estimated models. As Figure 2 demonstrates, I find that a flexible baseline hazard often suffices to capture the treatment effect remarkably well. This is consistent with previous Monte Carlo evidence in the literature, which nuances the importance of unobserved heterogeneity when estimating duration models (Nicoletti and Rondinelli, 2010).[13]

# 4 Application to #MeToo

## 4.1 Data and Context

### 4.1.1 Data Sources

My empirical strategy requires incident-level datasets that distinguish the date of the incident and the date of its report to the police. Commonly used police datasets in the United States do not provide this information. The Uniform Crime Reporting (UCR) datasets provide yearly aggregate reports per agency but no incident-level observations. The National Incident-Based Reporting System (NIBRS) provides incident-level observations, recording either the incident date or the report date to the police, but not both. To circumvent this problem, I rely on city-level police datasets (Police Data Intiative, 2024). I choose cities that provide (i) incident-level data for the period 2011–2019, (ii) include sex crime reports, and (iii) distinguish the date of the incident and the date of its report to the police. I restrict the sample to crimes reported until December 2019 because lockdowns and other restrictions to fight the COVID pandemic may have affected sex crime incidence and reporting from 2020 onwards. 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, but the main results are qualitatively similar when including these two cities in the estimations. Ultimately, my main dataset consists of detailed incident-level police records for New York City, Los Angeles, Seattle, and Cincinnati between 2011 and 2019. 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. I also download offender-level arrest datasets for Los Ange-

[13] In general, the distributions of estimates for the proposed estimator have fatter tails than the naive estimator. This is due to the inverse probability weighting approach that may give enormous weight to some observations, inflating standard errors in the process. To trade off bias with variance, researchers could cap the maximum weight given to an observation.

les and New York City 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*. 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 New York City, I can further distinguish sex offenses between *misdemeanors* and *felonies*, and I 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.[14]

### 4.1.2 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 15$^{th}$ 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 Online Appendix Figure E.5). 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 4.3.

[14]In 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.

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 (Forbes, 2020; New York Post, 2020). Others claimed the movement failed to recognize the heightened vulnerability that women of color frequently face (New York Times, 2017a; AP News, 2021). Finally, proponents warned against the potential backlash (Harvard Business Review, 2019). 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 4.3).

## 4.2 Stylized Facts

### 4.2.1 The Prevalence of Delayed Reports

Online Appendix Table A.3 presents descriptive statistics for the database, which I briefly summarize here. Between 2011 and 2019, over 2 million crime incidents were reported. Among those, approximately 110,000 were sexual crimes.[15] 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, Blacks 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 prevalence 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). 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 average 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.[16] The standard deviation in reporting delays is almost sixteen times larger for sex crimes relative to non-sexual assaults, robberies, or burglaries. Approximately 40% of plaintiffs for sex crimes 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 experiencing higher costs to reporting. Juveniles report over more extended periods than adults. Hispanic and Black plaintiffs also display longer reporting delays than other racial categories (see Figure A.2).

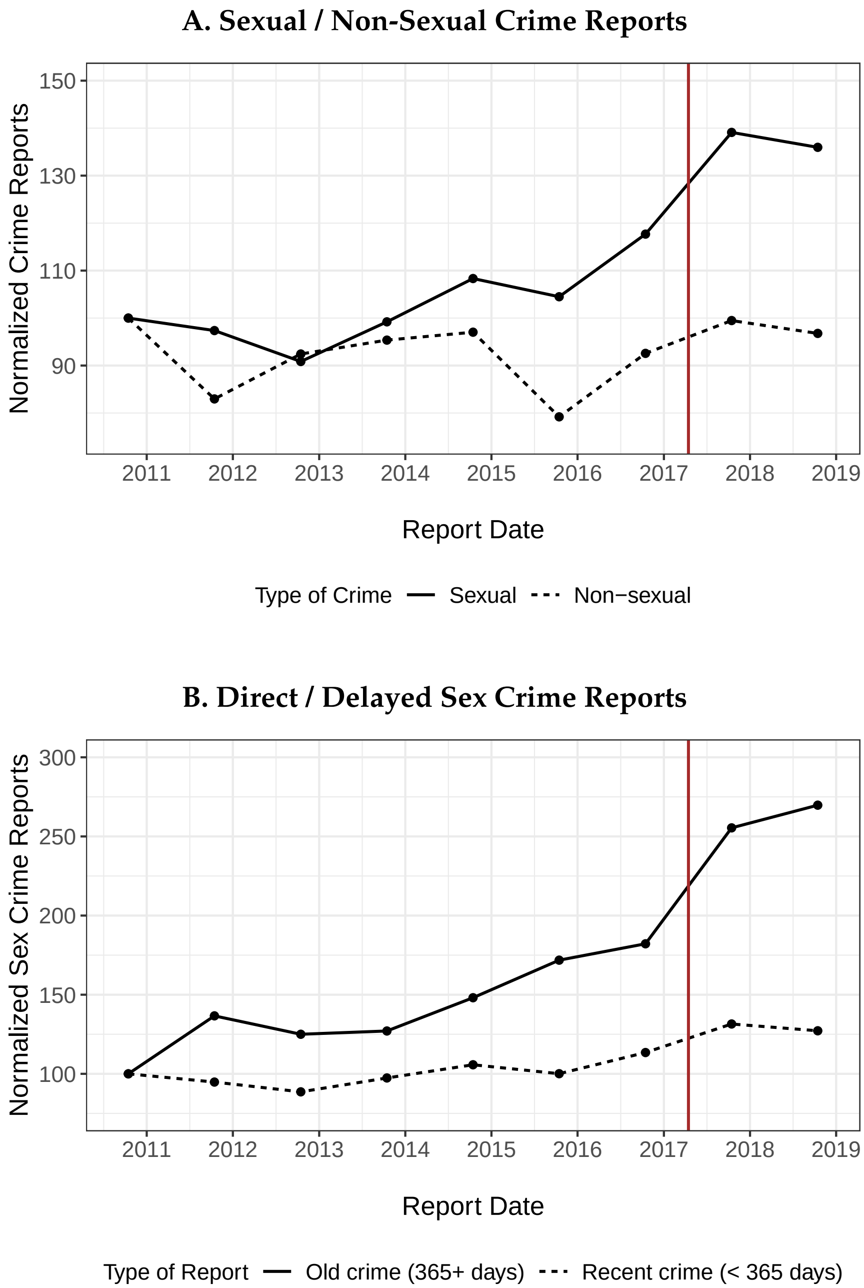
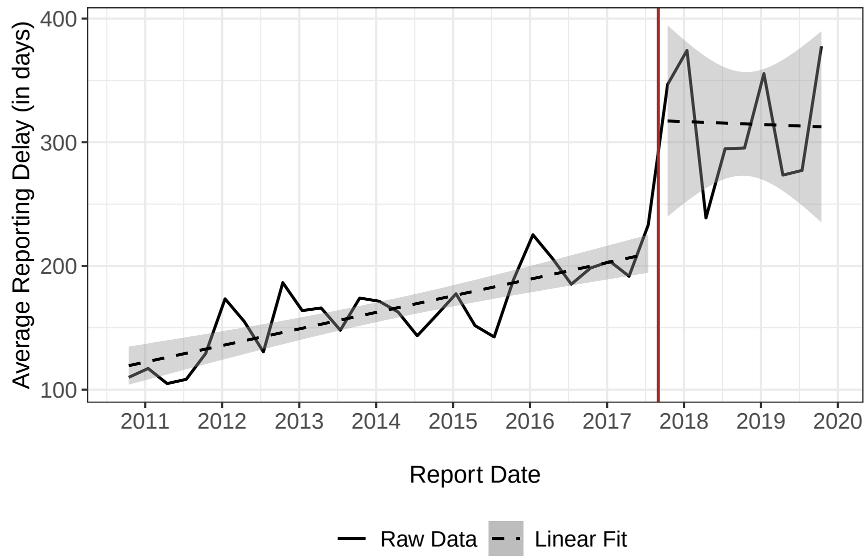



Figure 3: Trends in Reported Crimes

*Notes:* This figure presents trends in reported crimes. Crime reports are aggregated on the report date. The vertical solid line corresponds to #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 in the first month after the incident.

**A. Time Since Sex Crime Incident by Report Date**




**B. Increased Hazard of Reporting After #MeToo**

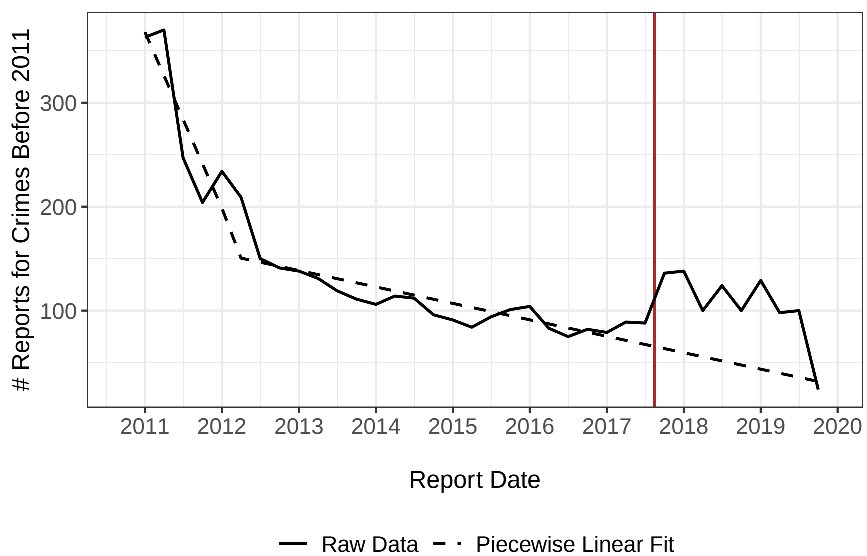



Figure 4: Raw Evidence of an Increased Propensity to Report Sex Crimes

*Notes:* Panel A displays average reporting delays (in days) for sex crimes by report date in the sample. The dashed lines are linear fits before/after #MeToo with 95% confidence intervals in grey. The increase in reporting delays suggests that victims of past crime incidents have increased their likelihood of filing a complaint over the period. Panel B presents police report counts for sex crime incidents that occurred before 2011. The stock of past sex crimes is progressively reported to the police. After #MeToo, one can observe a clear, unusual increase in reported crimes, suggesting an increase in the reporting rate of victims. The year's choice is arbitrary, and similar figures can be produced for various thresholds. The vertical solid line corresponds to #MeToo (Oct 2017).

### 4.2.2 Trends around #MeToo

I now turn to the study of trends in reported crimes surrounding the Me Too movement's mass mediatization (in October 2017). Panel A of Figure 3 indicates that sex crime reports increased by approximately 50% between 2011 and 2019, whereas non-sexual crimes remained stable over the same period. Interestingly, the surge starts before #MeToo. Following Equation 1, this can be rationalized as an increase in sex crime reporting or an increase in sex crime incidence, if not variations in both latent variables.

Several pieces of evidence point to an increase in the reporting rate of victims. Panel B of Figure 3 distinguishes delayed and direct reports for sex crimes. Delayed reports increased twice as much as direct reports between 2011 and 2019 and drove a sizable share of the increase in reported sex crimes. This stylized fact is consistent with the depletion of a large stock of unreported sex crimes being progressively reported to the police.

To better understand this increase in delayed reports, Figure 4 analyzes average reporting delays and probabilities of reporting a sex crime over time. If victims become increasingly likely to report sex crimes to the police over time, then Equation 3 predicts that we should observe an increase in average reporting delays for a given report date. Panel A clearly shows that average reporting delays more than tripled during this period. The increase is particularly large after #MeToo went viral on social media.

A complementary way of looking at this is to visually inspect the probability of reporting crimes committed at time $t$ in subsequent periods. Panel B plots complaints for sex crimes that occurred before 2011 that were filed between 2011 and 2019. The number of reports progressively decreases over time as the stock of unreported crimes depletes itself. However, one can observe an unusual increase in reports after #MeToo, that a simple piecewise linear fit would not predict. Given that #MeToo cannot impact the number of crimes committed before 2011, this suggests the victim reporting rate increased after #MeToo.

To summarize, we have strong reasons to believe the reporting rate increased, and particularly so after #MeToo. This raw empirical evidence alone, however, cannot tell us *by how much* sex crime reporting and incidence evolved over time. In what follows, I rely on the methodology developed in Section 3 to decompose the time series of reported sex crimes to the police in the two margins of victim reporting and crime incidence.

## 4.3 Main Results

This section studies sex crime incidence and reporting between 2011 and 2019, with a focus on the Me Too movement's impact. I first investigate the reporting rate of victims (see Equation 6). This allows me to compute estimates of sex crime incidence over the period (see Equation 4). Finally, I

[15] For comparison, note that the National Crime Victimization Survey (NCVS) estimates rates of sexual violence based on less than a hundred self-declared cases per year.

[16] Murders have long reporting delays when the police record them with a delay which correspond to cold cases.

isolate the effect of #MeToo on sex crime incidence.

### 4.3.1 Did #MeToo increase victim reporting?

I estimate the victim reporting hazard between 2011 and 2019. The dependent variable is the number of days elapsed between a sex crime being committed and its report to the police. I specify a piece-wise constant baseline hazard with breaks set after 1, 30, 90, 180, and 365 days.[17] Consistent with estimates of the NCVS, I set $\alpha$ so that the share of never-reporters in 2011 is equal to 70%. I pool all cities and specify the hazard as follows:

$$h_{it}(y) = f_0(y) \exp\left(\alpha + \sum_{k=\text{Oct.15,2010}}^{\text{Oct.15,2019}} \beta_k \mathbb{1}(t + y \geq k)\right) \tag{8}$$

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

Figure 5 decomposes reported sex crimes to the police by incident date into estimates of sex crime incidence and reporting. For each year, I report the hypothetical share of victims who would have eventually reported for that year absent future changes in reporting behaviors (in green). For year $J$, this corresponds to:

$$\lim_{y\to\infty} F_{it}(y) = 1 - \exp\left(-\exp\left(\alpha + \sum_{k=\text{Oct.15,2010}}^{\text{Oct.15},J} \beta_k \mathbb{1}(t + y \geq k)\right)\right). \tag{9}$$

For simplicity, I refer to this as the reporting rate of victims. My estimates indicate that the reporting rate more than doubled during the decade. It increases from 30% in 2011 to 55% before October 2017. The Me Too movement coincides with a reinforcement of these broader trends as the reporting rate reaches 75% in 2019.

Given the duration model, I can also estimate $p_{t,\tau_1,\tau_2}$ for each incident date $t$. Using Equation 4, this provides me with direct estimates of sex crime incidence (in red). I find a large decrease in sex crime incidence over the same period. Increasing reporting rates and decreasing sex crime incidence partly cancel each other out in the time series of reported crimes and translate into an increase in reported sex crimes by incident date of approximately 50%.

These results have clear implications. In terms of methodology, they 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. Regarding the Me Too movement, my results underscore that it did not appear in a vacuum. Though

[17] The breaks are chosen by inspecting estimates of a piecewise constant hazard with over 50 breaks (see Online Appendix Figure C.2). The reporting hazard sharply decreases in the first year and remains flat for longer delays.

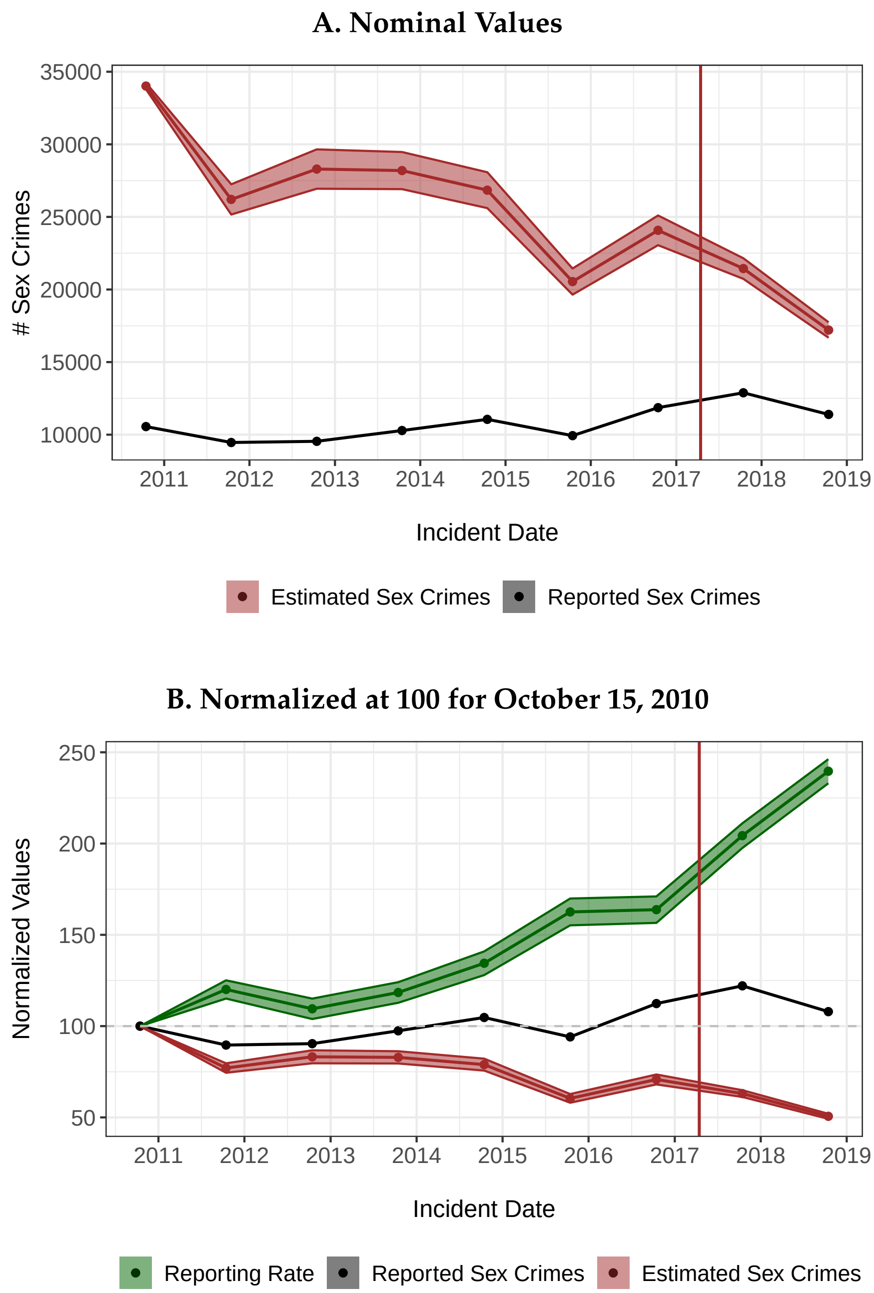



Figure 5: Trends in Sex Crime Reporting and Incidence

*Notes:* Panel A presents estimates of sex crime incidence (in red) compared to reported sex crimes to the police by incident date (in black) between 2011 and 2019. Panel B decomposes reported sex crimes by incident date (in black) into crime reporting (in green) and crime incidence (in red) based on Equations 6 and 9. I assume 30% of victims would have eventually reported sex crimes committed in 2011. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. There is no unobserved heterogeneity. 95% confidence intervals are constructed with a bootstrap procedure and 500 iterations. The vertical solid red line corresponds to the Me Too movement's mediatization.

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.

I conduct several additional exercises in the Online Appendix. First, given the importance of the hypothesized share of never-reporters in 2011, I present estimated trends for different values of this hyperparameter. The model rescales the observable part of the distribution of times to report $Y$. The choice of $\alpha$ thus changes the magnitude of the effects uncovered but not the underlying trends. I show that a broad range of reasonable parameter values for $\alpha$ – ranging from 60 to 80% of never-reporters at baseline – leaves the main decomposition result qualitatively unchanged (see Online Appendix Figure C.1).[18] As I show later, my results on the movement's deterrent effect are also robust to these alternative shares of never-reporters.

Second, to remove entirely the influence of the plug-in parameter $\alpha$, I show estimates for a model focusing on the observable part of the distribution of times-to-report (see Online Appendix Table C.1). That is, the model assumes all victims eventually report to the police. I still find a large and positive effect of #MeToo on the hazard of filing a complaint to the police, robust across a battery of specifications adding control variables, linear and quadratic calendar time trends, a gamma-distributed heterogeneity, and restricting the sample in various ways. Turning to heterogeneity analysis, my estimates indicate that juvenile, Hispanic, and Black victims were more responsive to the movement's sudden mediatization (see Online Appendix Figure C.4). Thus, evidence points towards #MeToo having a larger, positive effect on the most vulnerable groups of the victim population.

Third, Equation 8 assumes that the marginal effect of covariates on the baseline hazard $h_0$ is independent of the time-to-report $y$. In practice, the Me Too movement may have differentially impacted victims of old crimes relative to recent crimes. This would be consistent with a "generational catch-up" narrative, for instance, where older generations are very responsive to the Me Too movement's message, but younger generations are less so because they already adhere to a social norm enforcing a high reporting rate.

Investigating time-dependent effects is challenging for two reasons. The first issue relates to statistical power. Recall that the effect of #MeToo is identified by incidents that occurred before but were reported after #MeToo. Consider an extreme case: If I am interested in the hazard of reporting on the second day after the incident following #MeToo, then only crimes committed on the $14^{th}$ of October 2017 and reported on the $15^{th}$ could be used to identify the movement's impact on the hazard. The second issue relates to dynamic effects. To say anything about the effect of #MeToo on the hazard of reporting two days after the incident date, I also require the movement to affect reporting behaviors immediately. Figure 5 shows the movement's effect was, in fact, persistent and increasing over time (see also Online Appendix Figure C.3 for quarterly estimates of the movement's dynamic effects).

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

Despite these caveats, I decompose the effect of a #MeToo indicator on the hazard of reporting across bins of reporting delays. I consider three bins: crimes reported in less than five years, crimes reported between 5 and 10 years, and crimes reported in more than 10 years. Results are presented in Online Appendix Figure C.5. Victims are more likely to report to the police across the entire spectrum of reporting delays. However, victims of very old crimes are particularly responsive to the Me Too movement. Victims of crime incidents committed less than five years ago see their hazard increase by 10% following #MeToo. In comparison, the hazard of victims of crimes committed 5 to 10 years ago – and similarly for those victimized more than 10 years ago – increases by approximately 300%. Given this heterogeneity, victims of very old crime incidents may inflate the movement's impact on the reporting rate of victims of recent crimes in my main results. To investigate this concern, I reestimate Equation 6 on a sample restricted to incidents reported in less than five years. As expected, the resulting decomposition uncovers very similar trends in crime incidence and reporting to my main results, but of a slightly smaller magnitude (see Online Appendix Figure C.6). This is expected, as very old crimes represent a very small fraction of all sex crime reports and ultimately play a negligible role in the point estimate based on the full sample of crime reports. Furthermore, as I show later, my results on the movement's deterrent effect are also robust to this restricted sample.

#### 4.3.2 Did #MeToo have a deterrent effect?

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 (e.g., 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:

$$\log(\text{Crimes})_{itc} = \beta \text{MeToo}_{it} + \delta_{ic} + \delta_t + \varepsilon_{itc}. \tag{10}$$

$\delta_{ic}$ and $\delta_t$ are respectively crime-city and time fixed effects. $\text{MeToo}_{it}$ is a dummy variable that takes value one for sex crimes after October 2017 and thus $\beta$ is the marginal effect of #MeToo on sex crimes.

In this context, the difference-in-differences estimates may be interpreted causally under two assumptions. First, I assume 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 6). Second, 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. My preferred specification defines non-sexual assaults as the control group. Non-sexual assaults are the closest crime category to sexual assaults and are thus a natural choice. To assess the robustness of my results, I also consider specifications with murders, robberies, and burglaries as part of the control group.

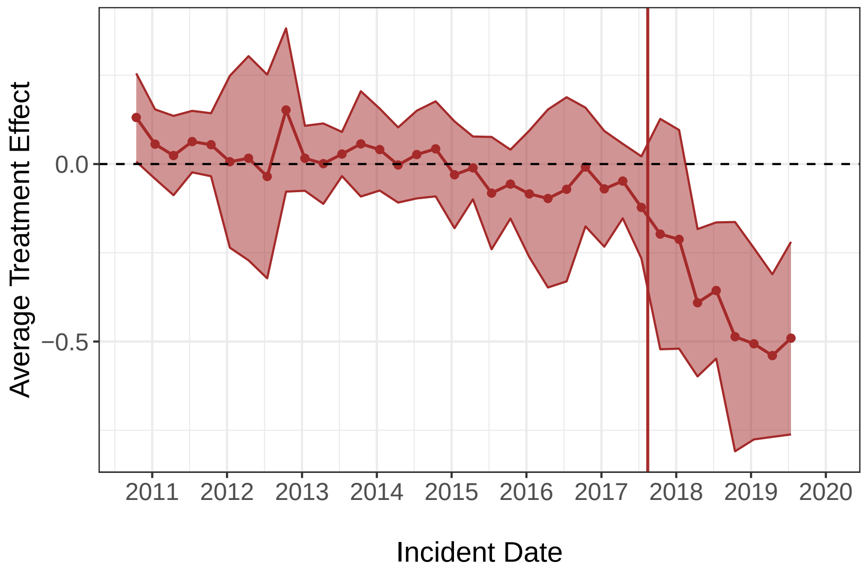



Figure 6: #MeToo Effect on Sex Crime Incidence

*Notes:* Quarterly estimates of the average treatment effect. The control group is reported non-sexual assaults. The main results are presented in Table 1. I present here the results from a standard two-way fixed effects event-study. 95% confidence intervals are constructed with the jackknife method (as is recommended when the number of treated units is small, see Liu et al., 2022). The vertical solid red line corresponds to the Me Too movement's mediatization. For other counterfactual models that relax the parallel trends assumption, see Online Appendix Figure D.1.

Table 1 presents the main results. Across specifications, I find a strong, negative, statistically significant decrease in sex crime incidence after #MeToo (≈ -28%, see Column 1). I compare my results to estimates 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 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 suggesting an increase in the reporting rate of victims (Levy and Mattsson, 2023). This warrants two 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 its report). In our empirical context, the size of the effect and its statistical significance vary substantially. Second, 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.

I conduct a series of robustness exercises. First, as presented in Table 1, my results are robust

Table 1: #MeToo Effect on Sex Crime Incidence

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Proposed Approach | | | Usual Approach | | Placebos | | | |
| | Estimated Sex Crimes (in logs) | | | Reported Sex Crimes (in logs) | | Reported Crimes (in logs) | | | |
| | | | | Incident Date | Report Date | Murders | Assaults | Robberies | Burglaries |
| **After #MeToo (indicator)** | -0.28 | -0.21 | -0.24 | 0.20 | 0.32 | 0.16 | 0.15 | -0.17 | -0.15 |
| | (0.12) | (0.07) | (0.10) | (0.11) | (0.11) | (0.16) | (0.14) | (0.20) | (0.19) |
| **Model** | DID | IFE | MC | DID | DID | DID | DID | DID | DID |
| **Fixed Effects** | | | | | | | | | |
| City-Crime Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Time Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **Control Groups** | | | | | | | | | |
| Murders | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| Assaults | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ |
| Robberies | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ |
| Burglaries | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | |
| Standard Errors | Clustered | Jackknife | Jackknife | Clustered | Clustered | Clustered | Clustered | Clustered | Clustered |
| N Observations | 740 | 740 | 740 | 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. Dependent variables are on the log scale. The panel data is aggregated at the quarterly level. Standard errors are in parentheses. In most specifications, standard errors are computed analytically and clustered at the city-crime level. For the Matrix Completion and Interactive Fixed Effects methods, I rely on jackknife standard errors (as is recommended when the number of treated units is small, see Liu et al., 2022). 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 assumption of parallel trends. Column 4 presents the observed effect on reported crimes to the police (aggregated at the incident date). Column 5 does the same exercise but aggregates crime reports at the report date. Columns 6 to 9 sequentially use reported non-sexual crimes as the treated unit and are interpreted as placebo tests.

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). Both counterfactuals display a much better fit in the pre-treatment period (see Online Appendix Figure D.1). In practice, however, point estimates of the movement's effects on sex crimes remain qualitatively unchanged (see Columns 2 and 3). Second, 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 6 to 9).

The Online Appendix provides additional robustness exercises. Table D.1 shows that my estimates remain robust when key model assumptions are relaxed. In columns 1 to 3, I vary the baseline share of never-reporters from 60% to 80%. Columns 4 and 5 restrict the sample to crimes reported within five years of the incident date for the decomposition analysis. Table D.2 further confirms that the results are not driven by a single city or crime category in the control group. I sequentially exclude each non-sexual crime used in the counterfactual analysis (columns 2 to 5) and each city in the sample (columns 6 to 9). In every case, the estimated effect on sex crime incidence remains strong, negative, and statistically significant. The effect size ranges from -0.22%

to -0.47%, providing a broad range of plausible causal estimates that all point in the direction of a deterrent effect.

## 4.4 Discussion and Mechanisms

### 4.4.1 Comparison to Alternative Approaches to Measuring Crime Incidence and Reporting

I first show trends for sex crime incidence and reporting at the national level using an alternative empirical strategy based on homicides (see Online Appendix E.1). Homicides are extremely likely to be recorded by law enforcement agencies and are thus the only crime category with a reporting rate of virtually 100%. The critical assumption of this alternative empirical strategy is that the ratio of sexual (non-sexual) homicides to sexual (non-sexual) violent crimes is constant over time. Another clear limitation of this empirical strategy is that homicides are much rarer than other crimes. I find qualitatively similar trends at the national level. My estimates suggest a substantial (five to six-fold) increase in sexual crime reporting and a substantial (70 to 80%) decrease in sexual crime incidence between 2011 and 2019, while the reporting rate and incidence of non-sexual crimes remain stable over the same period.

I now turn to victimization surveys for direct estimates of sex crime incidence and reporting, as those are often used in applied research on crime. Survey evidence indicates that increased sex crime incidence mainly drives the rise in sex crime reports (+300% over the decade; see Online Appendix Figure E.2). The increase is particularly large after #MeToo. Alternatively, another common approach is to rely on emergency records to proxy sexual violence (Aizer, 2010). Emergency department visits indicate that consultations for sexual assaults increased by 40% over the period, whereas consultations for non-sexual assaults decreased by 16% (see Online Appendix Figure E.3). The increase is particularly large for the years 2017 and 2018.[19] Thus, both approaches suggest an increase in sex crime incidence that may be understood as a backlash effect following the Me Too movement's mediatization, and are widely inconsistent with my main results. Of course, all these exercises are performed at the national level, whereas my main results are obtained in a smaller sample of cities. Nonetheless, they should at least raise eyebrows regarding the reliability of these alternative data sources.

### 4.4.2 Recording Guidelines and Practices

An alternative interpretation of my results is that the recording guidelines of police officers changed over the period. The legal definitions of sex crimes did not change between 2011 and 2019 in my sample. However, the FBI changed its definition of rape in 2013.[20] City-level police records rely

[19] Note that seeing a doctor and declaring a sexual assault are partly the result of a victim's decision. As for reported crime statistics, emergency records reflect variations in victim decision-making, recording practices and guidelines of doctors, and crime incidence.

[20] 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*

on a different categorization of offenses, which should not affect my results. To remove doubts, the #MeToo effect remains when I restrict the sample to crimes reported after 2014 (see Column 9 of Online Appendix Table C.1). Similarly, police officers could have become more likely to accept and record plaintiffs' complaints for sex crimes. This is plausible and difficult to assess with existing data. A closer inspection of arrest rates suggests that, if anything, arrest rates slightly decreased over the period (see Online Appendix Figure E.7).

#### 4.4.3 Unfounded Allegations

A remaining concern is that my main results assume truthful and founded crime reports. In practice, false allegations of a crime are a prevalent 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. 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 (through larger financial compensations or higher probabilities of sentencing, for instance). 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. When performing the crime reporting and incidence decomposition exercise for this restricted sample. I find qualitatively similar trends to my main results for Los Angeles (see Online Appendix Figure E.4). Thus, to the extent that there are unfounded allegations, they are unlikely to drive my results.

#### 4.4.4 Attitudes Toward Sexual Violence

The recurring public debate over the incidence of false allegations is a reminder that social norms and beliefs also influence the decision to report a crime to the police. 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.[21] Second, social conformity concerns may also influence a victim's decision. If victims care about what other victims do, or about what they think society expects from them, then a repeated coordination game may easily result in persistent, sub-optimal equilibria. Unfortunately, there is no systematic

*person, without the consent of the victim."'*

[21]Given the considerable uncertainty on the matter, these beliefs need not be accurate or unbiased.

empirical evidence of the discourse surrounding sex crimes, and the attitudes of the broader public on the matter.[22] To proxy attitudes towards sexual violence, I collect tweets mentioning sex crimes between 2011 and 2019. Online Appendix Figure E.5 shows the time series of tweet counts (in logs). 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. In fact, the topic was already discussed on Twitter, and its share of the total number of tweets has been increasing since 2011. Similar observations are made with Google Trends data (see Figure E.6). This suggestive evidence is consistent with the pre-trends in crime reporting uncovered in Section 4.3.

#### 4.4.5 Odds of Punishment for Sex Offenders

Higher odds of reporting crimes to the police mechanically increase the probability of arrest of sex offenders (unless arrest rates are zero). Thus, a plausible mechanism for the estimated decrease in sex crime incidence between 2011 and 2019 is that of a deterrent effect. Between 2011 and 2019, arrest rates in Los Angeles and New York City remain relatively stable, with a slight decrease in sexual offenses towards the end of the decade (see Online Appendix Figure E.7). Combining arrest rates with my estimates of the victim reporting rate, I compute the unconditional probability of arrest for committing a sex crime. Using my baseline specification estimates, I find that the probability of arrest increased from 16% to 37% between 2011 and 2019 (see Online Appendix Figure E.8). The decrease in sex crime incidence is thus consistent with a Beckerian argument. Between 2011 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. Though one cannot conclude causation, I understand these findings as suggestive evidence 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.

## 5 Conclusion

Underreporting has been a major empirical challenge in making sense of reported crime statistics in the past two centuries. I proposed a methodology to separate 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 studied sex crime incidence and reporting surrounding the Me Too movement's mediatization. Three key results emerged. First, #MeToo largely and persistently increased the propensity to report of victims. The effect is larger for juveniles and racial minorities, as well as victims of past crime incidents. Second, according to my estimates of sex crime incidence over the period, the

[22] To the best of my knowledge, the Views of the Electorate Research (VOTER) survey is the only national survey with explicit questions on attitudes towards sexual violence. Levy and Mattsson (2023) find inconclusive empirical evidence of a change in attitudes towards sexual violence among respondents. Note that the questions that were asked before and after #MeToo investigate sexual harassment in the workplace, which is not the focus of this paper.

movement also had a deterrent effect on sexual offenders. Third, it 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. Instead, I presented 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 public awareness campaigns and social movements 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 Me Too movement's broader legal, political, and economic consequences remain largely unknown and represent an opportunity for future research.

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# Online Appendix for Measuring Crime Reporting and Incidence: Method and Application to #MeToo

# A Data Sources

## A.1 City-level Police Records

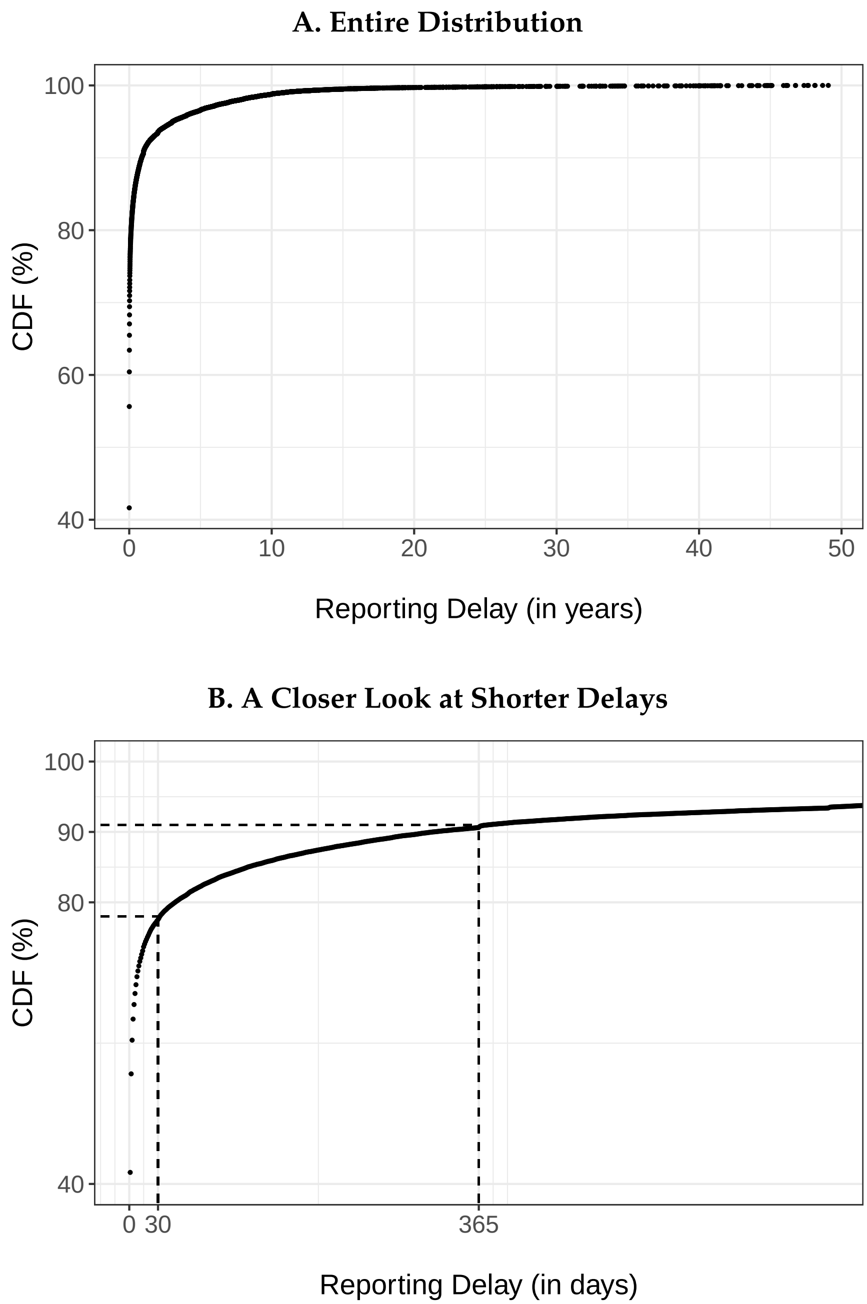



Figure A.1: Distribution of Reporting Delays for Sex Crimes

*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.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, beastiality, 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 (adultry,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 (adultry,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*.

Table A.3: Descriptive Statistics on Police Records

| | Crime Type: | | | | |
|---|---|---|---|---|---|
| | **Sex Crime** | **Murder** | **Assault** | **Robbery** | **Burglary** |
| **Number of Observations** | 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 |
| Standard Deviation | 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 2011 and 2019.

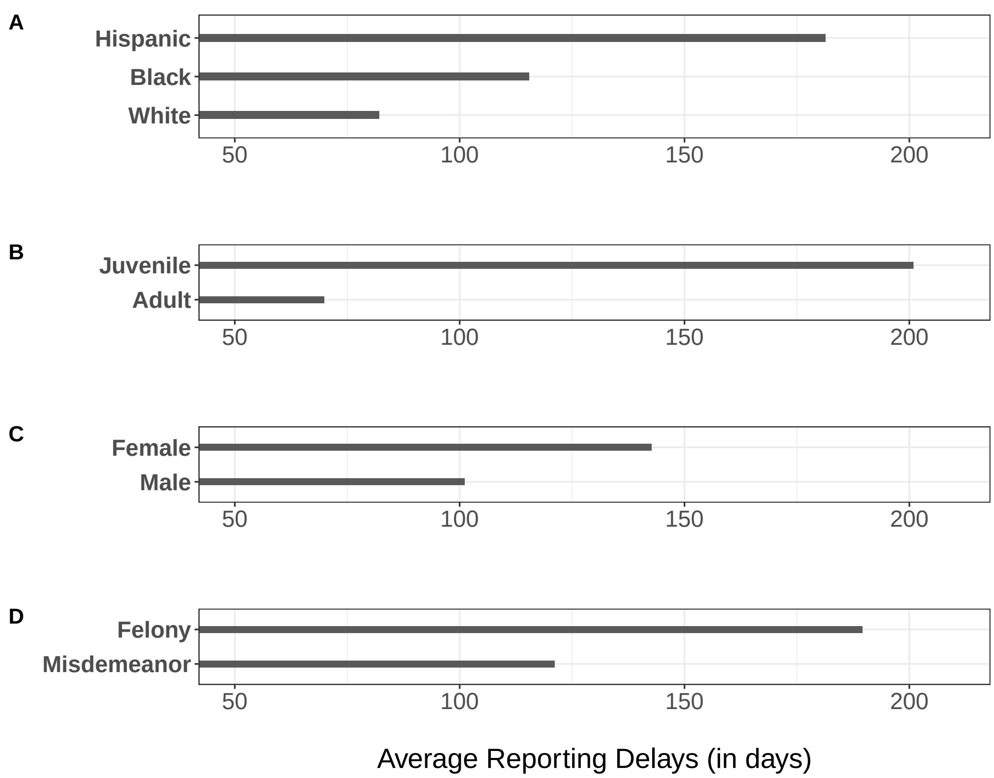



Figure A.2: Reporting Delays per Incident and Victim Characteristics

*Notes:* average reporting delays for sex crime incidents per incident characteristics based on the New York City Police Department database (2011 – 2019). I restrict the sample to incidents reported in less than ten years (to avoid inflating the means with outliers). Socio-demographic groups and incidents with lower reporting rates are usually associated with longer average reporting delays.

# B Additional Details on the Duration Model

## B.1 The Structure of Crime Reports

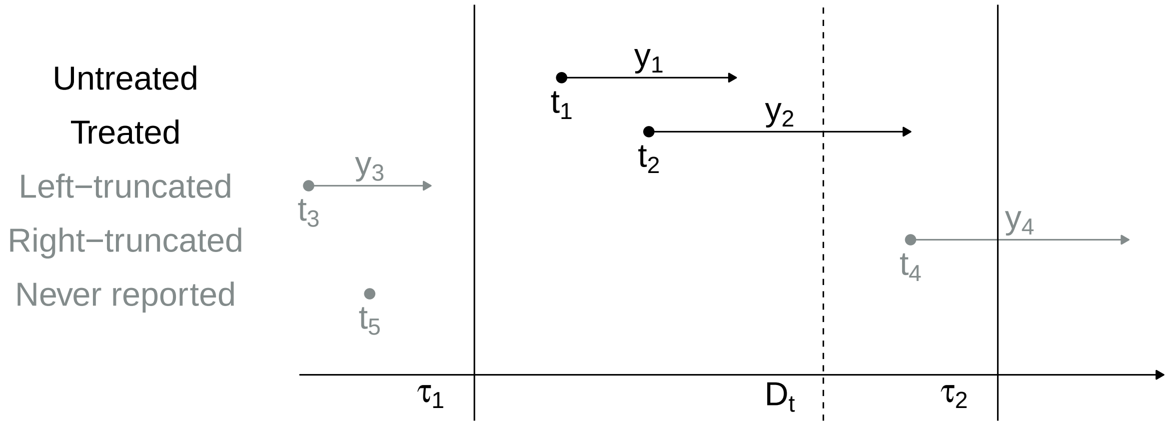



Figure B.1: The Structure of (Delayed) Crime Reports

*Notes:* A graphical depiction of time to report data based on police records. The study window goes from $\tau_1$ to $\tau_2$ (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).

In this section I focus on the data structure of police records. Some of its peculiarities are relevant for survival analysis. Figure B.1 presents a graphical summary of police data as duration data. The study window goes from $\tau_1$ to $\tau_2$ (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 the main paper's Section 3 and Online Appendix Section B.3.

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 duration model that accounts for this stylized fact, which I explain in greater detail in the main paper's Section 3 and Online Appendix B.2.

## B.2 The Duration Model

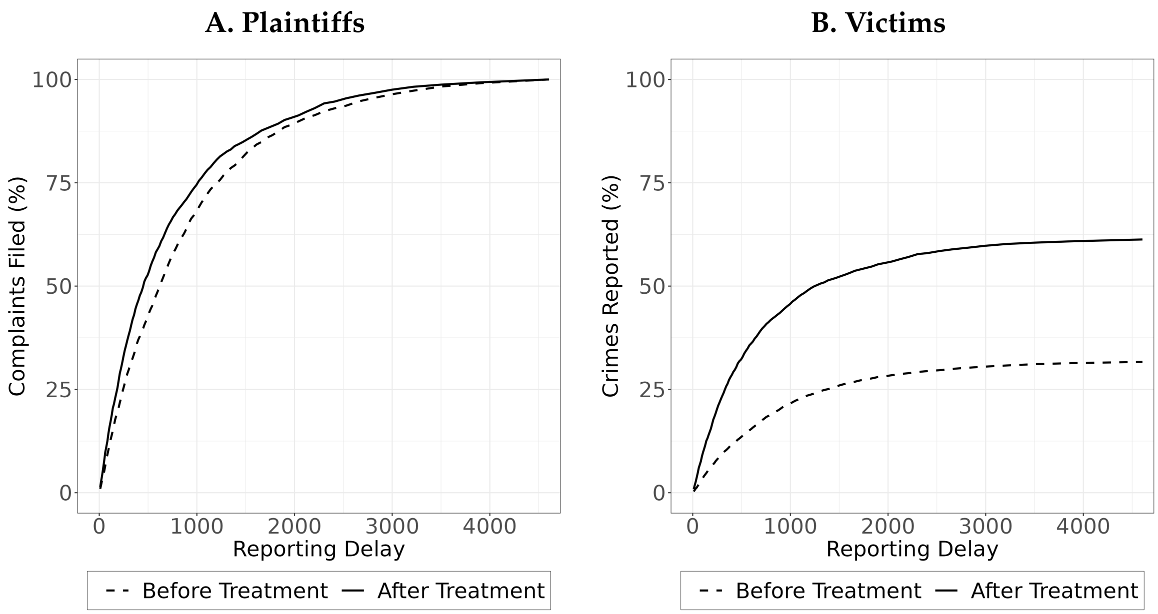



Figure B.2: From Observed Delays to Victim Reporting Rates

*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 observed reporting delays in police records. Panel B plots the CDF of the entire victim population and corresponds to Equation 6. 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 4.3).

For individual $i$, victim of an incident at time $t$, I model the hazard of reporting a crime to the police $y$ days after the incident occurred as a mixed proportional hazards (MPH) duration model. I account for never-reporters explicitly by enforcing the baseline hazard function as a density function and adding an intercept to the regression model:

$$h_{it}(y \mid \gamma_i, \boldsymbol{x_{ity}}) = f_0(y) \exp\left(\alpha + \boldsymbol{\beta'} \boldsymbol{x_{ity}}\right) \gamma_i,$$

where $f_0 : \mathbb{R}^+ \to \mathbb{R}^+$ is a proper density function that acts as the baseline hazard, $\alpha$ accounts for the share of victims who will never report at baseline, $\boldsymbol{\beta'} \in \mathbb{R}^d$ is the vector of regression coefficients and captures covariate effects on the probability of report, and $\gamma_i \in \mathbb{R}$ is a time-invariant random effect to account for potential unobserved heterogeneity across individuals. Figure B.2 provides intuition for how this model rescales the observable distribution of reported delays to account for never-reporters.

I now provide a brief theoretical justification for the 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 reporting 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:[23]

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

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 $F_0$.[24] It is then straight-forward to show that the hazard and survival functions of times to report of victims are respectively

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

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:

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

[23]An 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).

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

**Formal Proof**

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

## B.3 Derivation of the Likelihood

Figure B.3 highlights how right-truncation bias may lead researchers to seriously overestimate the impact of an intervention against crime. Clearly, the estimates of a naive Cox model that does not account for right-truncation are very dependent on the study window. The closer the intervention to the end of the study window (in this case, #MeToo), the larger the estimate. This is obviously a spurious result as right-truncation leads to an oversampling of shorter durations as we move closer the end of the study window. In what follows, I derive likelihoods that appropriately account for double-truncation in the data.

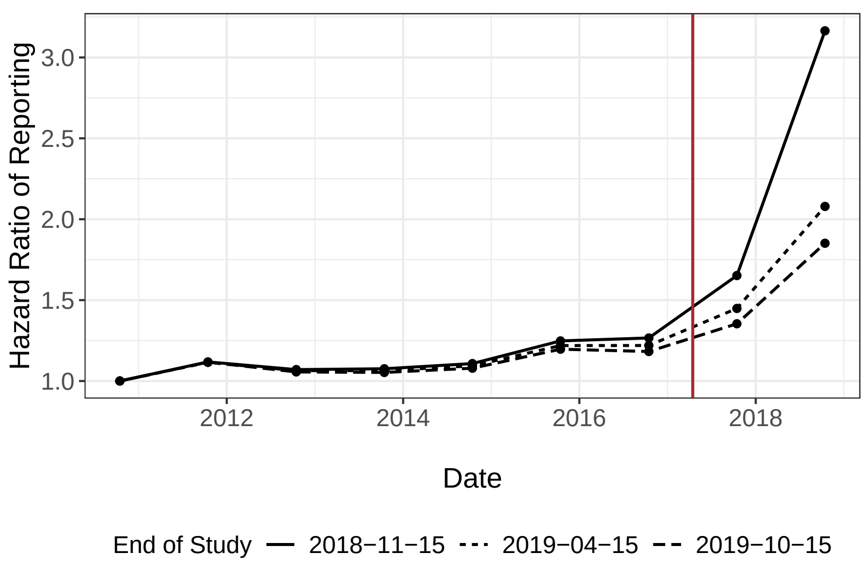



Figure B.3: An Example of Right-truncation Bias

*Notes:* Yearly estimates of the plaintiff 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. There is no unobserved heterogeneity. 95% confidence intervals. Without an appropriate correction for right-truncation, estimated hazard ratios are heavily dependent on the end of the study period $\tau_2$.

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 $\Theta$ 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 $\{(x_i, y_i)\}_{i\in\{1,...,n\}}$ for inference. Under the assumption that $Y$ and $U$ are independent conditional on observed covariates $X$, we have

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

which gives

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

after some factorization, we obtain

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

**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 $y_{i1}, ..., y_{iJ_{i-1}} \in \mathbb{R}^{J_{i-1}}$. Among those, the first $J'_i + 1$ are observed variations, whereas the remaining are *counterfactual* values for the right-truncation time[25], such that: $y_{i0} = u_i \leq y_{i1} \leq ... \leq y_{iJ'_i} = y_i \leq ... \leq y_{iJ_i} = v_i \leq \infty$, yielding the sequence of covariate vectors: $\chi_i = \{x_{i1}(y_{i0}), ..., x_{iJ_i}(y_{iJ_{i-1}})\} \in \mathbb{R}^{J_i}$. For $j = 1, ..., J_i$, $y_{ij} - y_{ij-1}$ 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

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

[25]For 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.

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-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 $\gamma$. 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

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

Applying Bayes rule, we have

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

Replacing expressions with the model parameters gives $L(\Theta \mid \gamma, x)$ as

$$\prod_{i=1}^{n} \frac{\mathbb{E}_{\gamma}\Bigg[\gamma \exp\big(\beta' x_i\big) h_0(y_i) \exp\Big(-\gamma \exp\big(\beta' x_i\big) H_0(y_i)\Big)\Bigg]}{\mathbb{E}_{\gamma}\Bigg[\Big\{\exp\Big(-\gamma \exp\big(\beta' x_i\big) H_0(u_i)\Big) - \exp\Big(-\gamma \exp\big(\beta' x_i\big) H_0(u_i + d)\Big)\Big\}\Bigg]}.$$

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 $\mathcal{L}_{\gamma}$ and its first derivative $\mathcal{L}_{\gamma}^{(1)}$:

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

I assume unobserved heterogeneity is gamma distributed with variance $\Sigma$ (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 $\{\gamma_k\}_{k\in\{1,\ldots,K\}}$ and respective shares within the population $\{s_k\}_{k\in\{1,\ldots,K\}}$. Further, I impose that all parameters are strictly positive and that the sum of their shares is one. Just like the piece-wise constant function to model baseline hazards, this formulation is a general specification of unobserved heterogeneity, which can account for many distributions. $L(\Theta \mid \gamma, x)$ is then

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

This last approach is the most flexible. However, it is computationally demanding, prone to converging to local minima, and requires large numbers of observations to estimate the random effect precisely.

**Code Implementation** For models with parametric (or without) unobserved heterogeneity, I use the BFGS algorithm (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 R package *rgenoud* implements the evolutionary algorithm.

# C Robustness – Did #MeToo increase victim reporting?

## C.1 Estimates for Different Values of $\alpha$

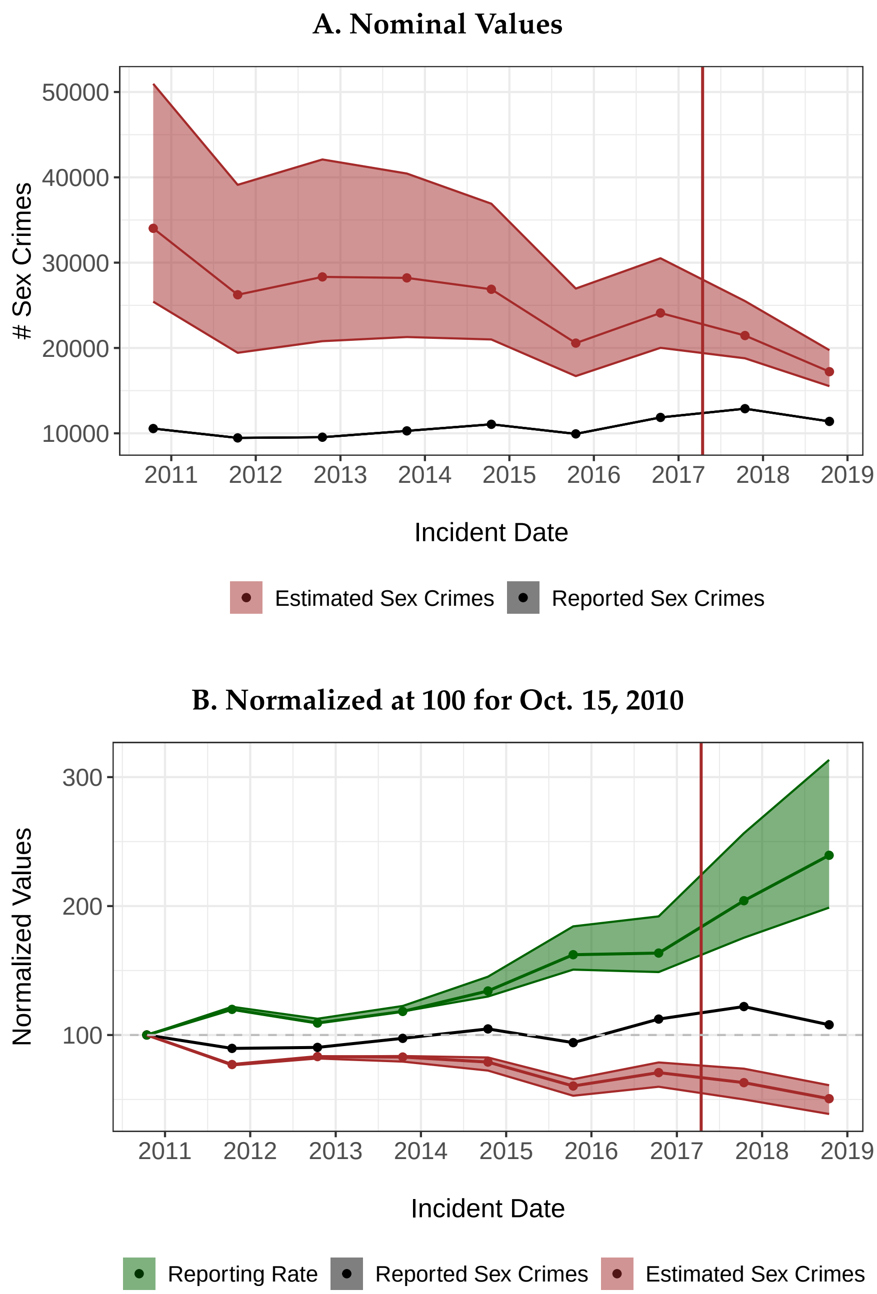



Figure C.1: Robustness of Estimates to Alternative Values of $\alpha$

*Notes:* Estimates of sex crime incidence and victim reporting for different values of $\alpha$. 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. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. There is no unobserved heterogeneity. 95% confidence intervals are constructed with a bootstrap procedure and 1000 iterations. The vertical solid red line corresponds to the Me Too movement's mediatization.

## C.2 Estimates Based on Observed Times-to-report

In what follows, I abstract from never-reporters and focus on the observable part of the distribution of times to report $Y$. That is, I assume that all victims eventually report to the police (though potentially over long periods). The dependent variable is the number of days elapsed between a sex crime being committed and its report to the police.

**Shape of the baseline hazard** I first investigate the shape of the baseline reporting hazard. In a model without covariates and without unobserved heterogeneity, I estimate a very granular piece-wise hazard, with breaks set at 1, 30, 90, 180, and 365 days and for each additional year afterward. The resulting estimates are presented in Figure C.2. Two findings emerge. First, the decline in the hazard is very steep in the first years – particularly in the first 365 days. Second, there is no clear evidence of an increase in the hazard for the statutes of limitations (the most common limitations are between 2 and 5 years). This suggests victims may be myopic in their decision to report to the police.

**#MeToo Effect Across Samples and Specifications** I then investigate the marginal effect of the Me Too movement's mediatization in October 2017. My baseline specification is

$$h_{itc}(y) = h_0(y)\exp\left(\delta_c + \beta \text{MeToo}_{ity} + \boldsymbol{\zeta' X_i}\right)\gamma_i. \tag{11}$$

$\delta_c$ is a city fixed effect that accounts for variations in reporting delays across cities. $\gamma_i$ is a gamma-distributed unobserved heterogeneity term. $X_i$ 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 $15^{th}$ of October 2017. Thus, $\text{MeToo}_{ity}$ is a dummy variable equal to one if $t + y \geq 15^{th}$ of October 2017, and 0 otherwise. Table C.1 presents results on the log-scale. Column 1 shows the baseline effect of #MeToo in a model without unobserved heterogeneity. Column 2 adds a gamma-distributed random effect. Column 3 adds a linear time-trend for calendar years between 2011 and 2019. Column 4 considers a quadratic time-trend. Columns 5 to 8 progressively add more breaks to the piece-wise constant baseline hazard. Since the FBI's definition of rape changed in 2013, Column 9 restricts the sample to crimes reported between 2014 and 2019. Columns 10 to 11 restrict the sample to New York City. Column 10 presents the baseline #MeToo effect in a model without controls. Column 11 adds the crime category, as well as victim and suspect characteristics as controls. Columns 12 and 13 restrict the sample to Los Angeles. Column 12 presents the baseline #MeToo effect in the full sample of incidents reported in Los Angeles. Column 13 presents an estimate for a sample restricted to reports that resulted in an adult arrest. Overall, the effect size is robust to these various specifications and samples.

**Dynamic Effects** To better understand the dynamics of the #MeToo effect on the hazard, I estimate the following model on crimes committed between 2017 and 2019:

$$h_{it}(y) = h_0(y) \exp\left(\delta_c + \sum_{k=\text{Oct.15,2010}}^{\text{Oct.15,2019}} \beta_k \mathbb{1}(t + y \geq k)\right)\gamma_i, \tag{12}$$

with quarterly calendar period indicators. Results are presented in Figure C.3. Consistent with the main results of the paper, I find that the movement had a persistent and increasing effect on the reporting hazard over time.

**Heterogeneity Analysis** I also perform a 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 C.1). My specification on this restricted sample is

$$h_{itc}(y) = h_0(y) \exp\left(\delta_c + \boldsymbol{\zeta'X_i} + \phi \text{MeToo}_{ity} + \boldsymbol{\Omega'X_i} \times \text{MeToo}_{ity}\right)\gamma_i. \tag{13}$$

Figure C.4 presents estimates of $\exp(\Omega)$, which may directly be interpreted as hazard ratios. The comparison group is composed of white women plaintiffs, filing a complaint against a white suspect for a sexual felony. Relative to these women, the reporting hazard increases more following #MeToo for juvenile, Black, and Hispanic victims. I find no differential effect of #MeToo for misdemeanors relative to felonies.

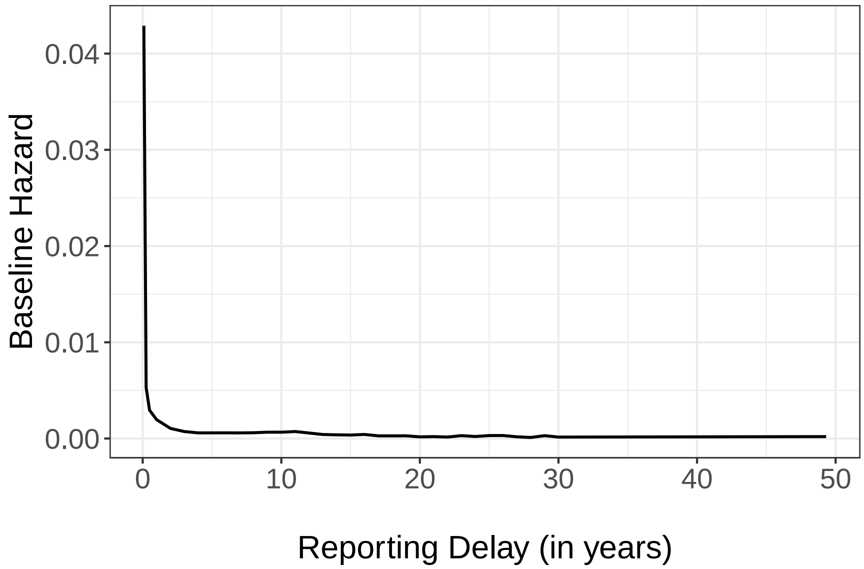



Figure C.2: A Granular Estimate of the Baseline Hazard

*Notes:* This figure shows a very granular estimation of the baseline hazard of reporting a sex crime to the police for crimes reported between 2011 and 2019. I Breaks are set at 1, 30, 90, 180, and 365 days and for each additional year afterward. I omit the first coefficient for days 1 to 30 because it is well above 40% and compresses the scale of the y-axis. There is no clear evidence of an increase in the hazard for the statutes of limitations (the most common limitations are between 2 and 5 years).

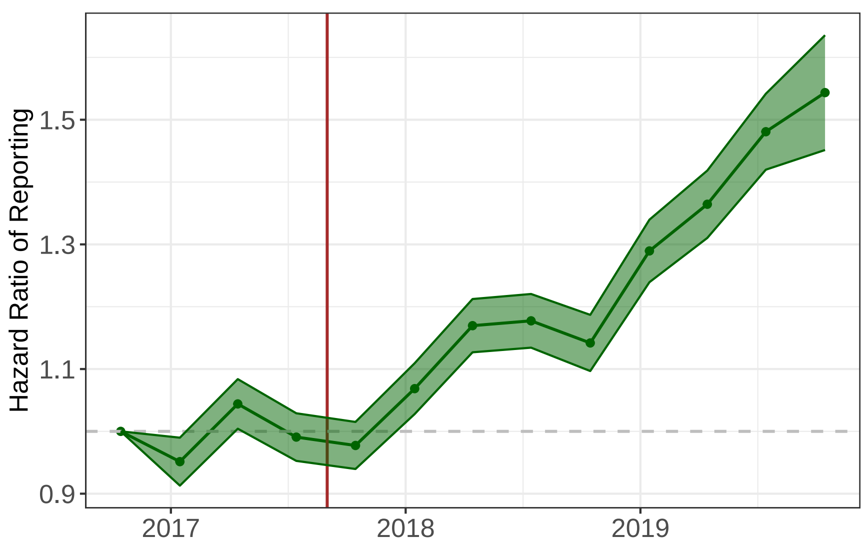



Figure C.3: Quarterly Dynamic Effects After #MeToo

*Notes:* Quartely estimates of the hazard of reporting before and after #MeToo based on Equation 12. Breaks are set at 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. Unobserved heterogeneity is assumed gamma-distributed. 95% confidence intervals. The solid vertical line separates periods pre- and post-#MeToo.

Table C.1: #MeToo Effects on the Hazard of Filing a Complaint

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Dependent Variable: Hazard of Reporting a Sex Crime** | | | | | | | | | | | | |
| **#MeToo (Indicator)** | 0.154 | 0.169 | 0.13 | 0.10 | 0.224 | 0.170 | 0.166 | 0.166 | 0.135 | 0.196 | 0.196 | 0.249 | 0.29 |
| | (0.009) | (0.011) | (0.012) | (0.017) | (0.012) | (0.011) | (0.011) | (0.011) | (0.013) | (0.020) | (0.019) | (0.026) | (0.075) |
| **Baseline Hazard** | | | | | | | | | | | | | |
| Day 0+ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Day 1+ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Day 30+ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Day 90+ | ✓ | ✓ | ✓ | ✓ | | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Day 180+ | ✓ | ✓ | ✓ | ✓ | | | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Day 365+ | ✓ | ✓ | ✓ | ✓ | | | | | ✓ | ✓ | ✓ | ✓ | ✓ |
| **Controls** | | | | | | | | | | | | | |
| Crime Category | | | | | | | | | | | ✓ | | |
| Victim Characteristics | | | | | | | | | | | ✓ | | |
| Suspect Characteristics | | | | | | | | | | | ✓ | | |
| City Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | | | |
| **Time-Trends** | | | | | | | | | | | | | |
| Linear | | | ✓ | ✓ | | | | | | | | | |
| Quadratic | | | | ✓ | | | | | | | | | |
| **Unobserved Heterogeneity** | | Gamma | Gamma | Gamma | Gamma | Gamma | Gamma | Gamma | Gamma | Gamma | Gamma | Gamma | Gamma |
| Start of the study $\tau_1$ | 2011 | 2011 | 2011 | 2011 | 2011 | 2011 | 2011 | 2011 | 2014 | 2011 | 2011 | 2011 | 2011 |
| End of the study $\tau_2$ | 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 | LA | LA |
| Number of Observations | 111869 | 111869 | 111869 | 111869 | 111869 | 111869 | 111869 | 111869 | 72730 | 32442 | 32442 | 26709 | 2717 |

*Notes:* Marginal effect of the Me Too movement's sudden mediatization in Oct. 2017 on the plaintiff reporting hazard (see Equation11) for various specifications and samples. Estimates are presented on the log scale. I focus on plaintiffs and abstract from never-reporters. Standard errors are in parentheses. Column 1 presents the baseline estimate. Column 2 adds a gamma-distributed random effect in the estimation to account for time-invariant unobserved heterogeneity. Columns 3 and 4 respectively account for linear and quadratic time-trends in reporting. Columns 5 to 8 sequentially increase the number of breaks in the baseline hazard. Column 9 restricts the study period to 2014–2019. Column 10 presents estimates for New York City. Column 11 controls for incident-level characteristics for New York City. Column 12 reports the baseline estimate for Los Angeles. Column 13 reports the estimate for Los Angeles when restricting the sample to complaints that lead to an adult arrest. Overall, the effect size is very stable across specifications.

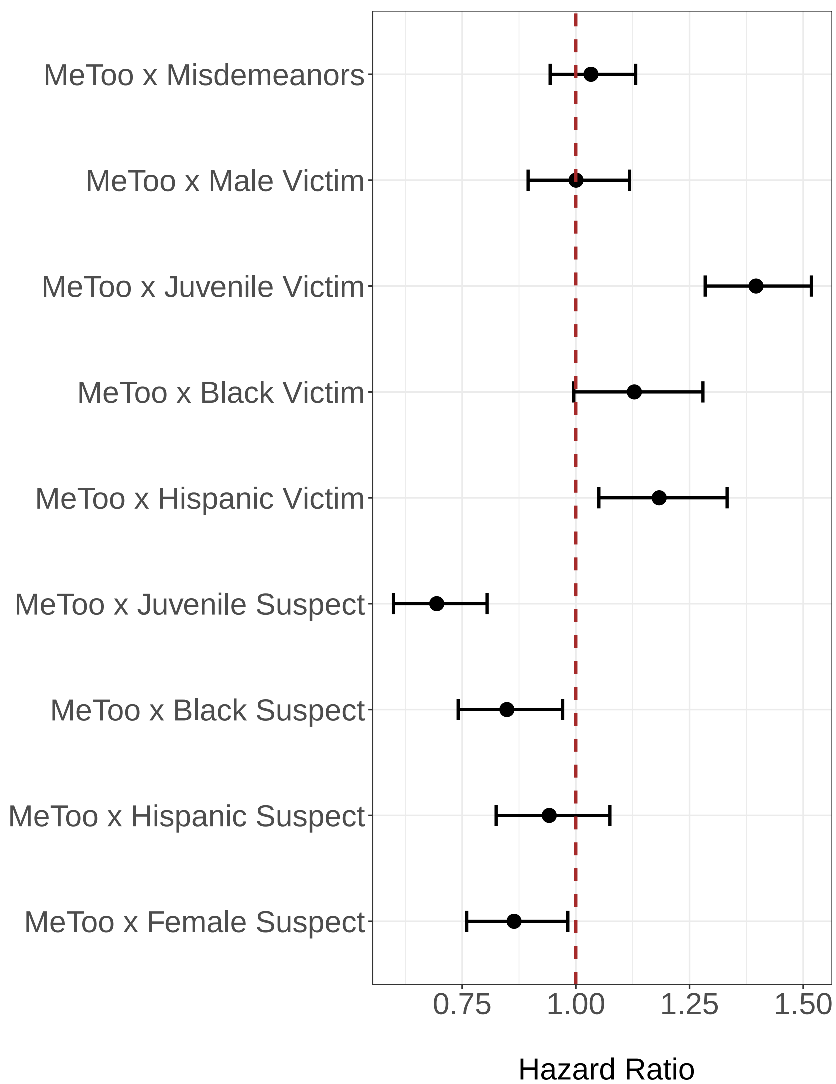



Figure C.4: #MeToo Effects on Crime Reporting – Heterogeneity Analysis

*Notes:* Estimates of $\exp(\mathbf{\Omega})$ based on Equation 13. The baseline #MeToo effect is for white women plaintiffs, filing a complaint against a white male suspect, for a sexual felony. The vertical dashed red 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. 95% confidence intervals.

## C.3 Analysis of Time-dependent Effects

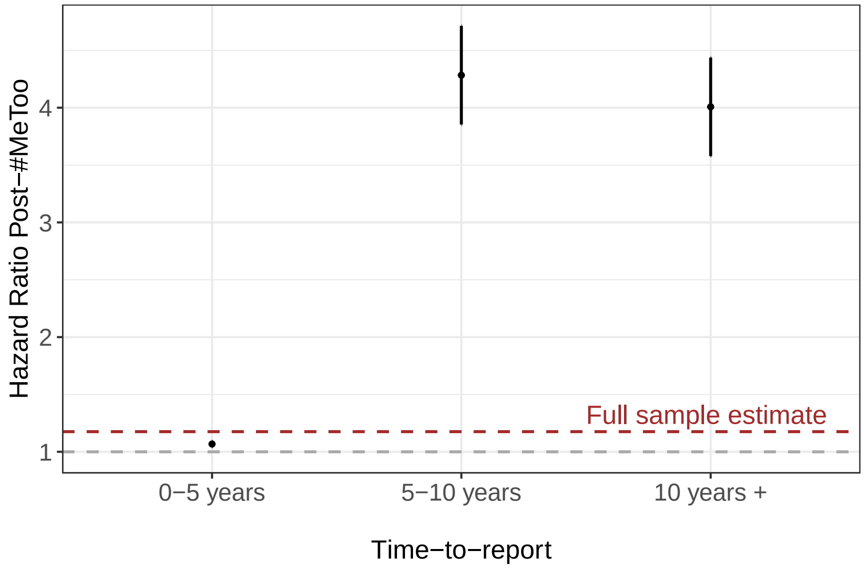



Figure C.5: Marginal Effect of #MeToo for Different Times-to-report

*Notes:* Estimates of the marginal effect of #MeToo for per bins of reporting delays (less than five years, 5 to 10 years, and more than 10 years). The red horizontal dashed line corresponds to the full sample estimate assuming time-independent effects. I abstract from never-reporters and focus on the observable part of the distribution of times-to-report. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. There is no unobserved heterogeneity. 95% confidence intervals.

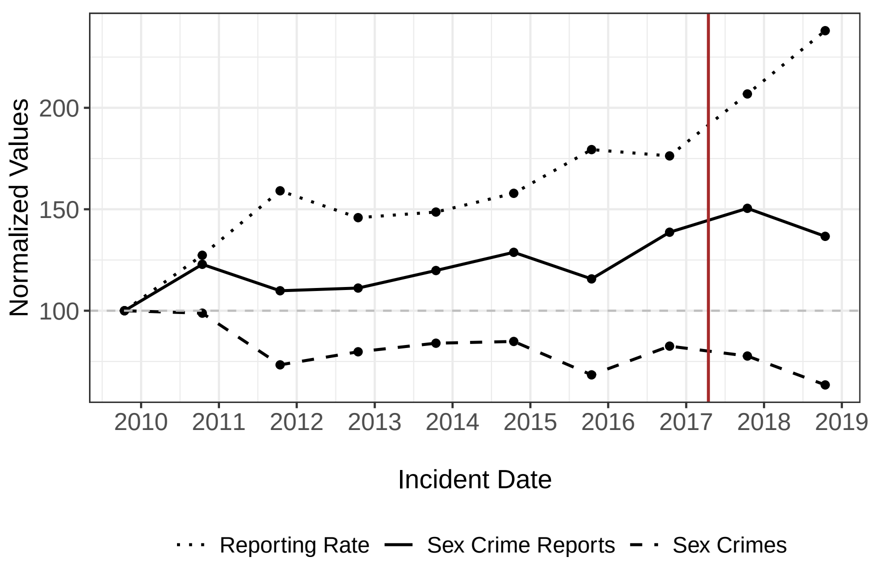


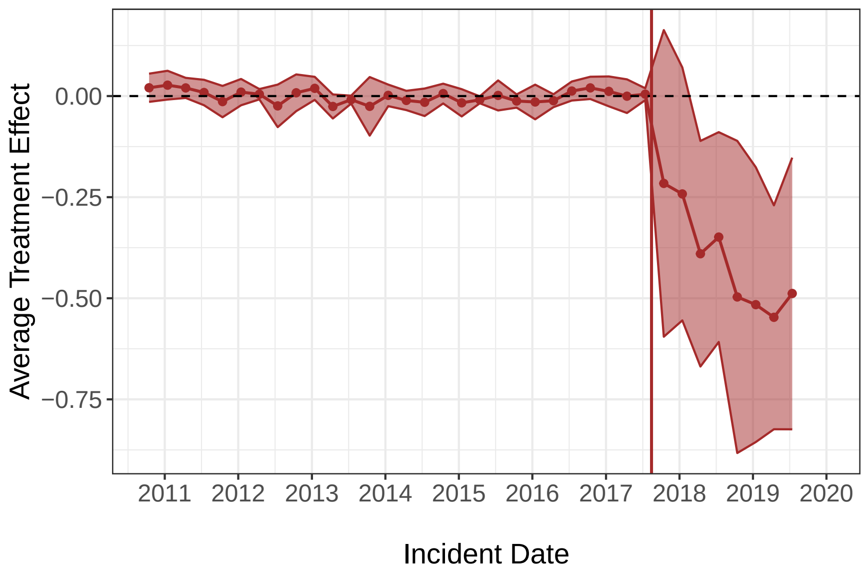

Figure C.6: Trends in Crime Reporting and Incidence (Sample Restricted to Recent Crimes)

*Notes:* Estimates of sex crime incidence and victim reporting based on a sample of crime reports restricted to crimes reported in less than five years. I assume 30% of victims would have eventually reported sex crimes committed in 2011. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. There is no unobserved heterogeneity. 95% confidence intervals are constructed with a bootstrap procedure and 1000 iterations. The vertical solid red line corresponds to the Me Too movement's mediatization.

# D Robustness – Did #MeToo have a deterrent effect?

## D.1 Alternative Counterfactual Models

**A. Interactive Fixed Effects (IFE)**




**B. Matrix Completion**

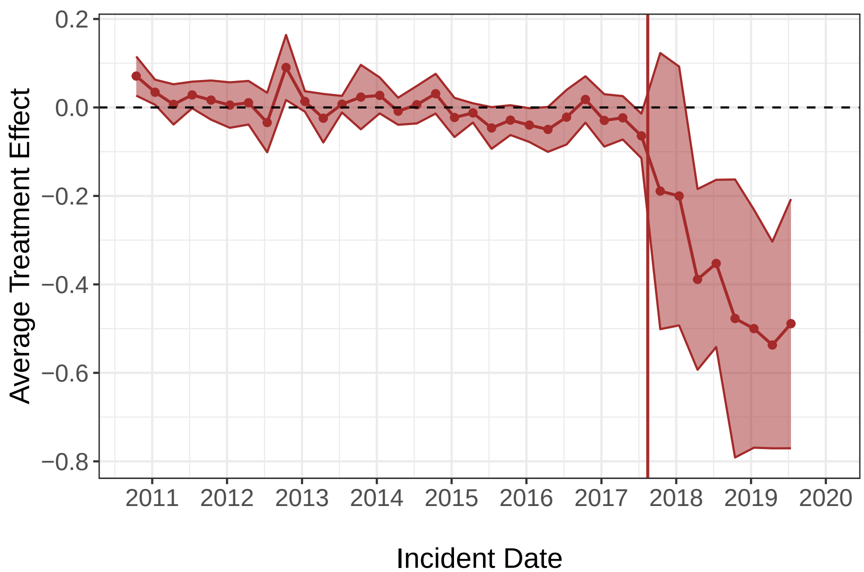



Figure D.1: Alternative Counterfactual Models of Sex Crime Incidence

*Notes:* Quarterly estimates of the Average Treatment Effect for alternative counterfactual models of sex crime incidence. The control group is reported non-sexual assaults. Panel A uses an interactive fixed effects (IFE) model with three additional factors (Xu, 2017). Panel B relies on the Matrix Completion method (Athey et al., 2021). 95% confidence intervals are constructed with the jackknife method (Liu et al., 2022). The vertical solid red line corresponds to the Me Too movement's mediatization.

## D.2 Robustness to Alternative Specifications of the Duration Model

Table D.1 presents estimates of the average treatment effect of #MeToo on sex crime incidence for alternative specifications of the duration model.

Table D.1: Robustness – #MeToo Effect on Sex Crime Incidence

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | **Dependent Variable: Estimated Sex Crimes (in logs)** | | | | |
| **After #MeToo (indicator)** | -0.28 | -0.26 | -0.47 | -0.22 | -0.34 |
| | (0.12) | (0.14) | (0.12) | (0.12) | (0.14) |
| **Counterfactual Model** | DID | DID | DID | DID | DID |
| **Fixed Effects** | | | | | |
| City-crime Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Time Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| **Control Groups** | | | | | |
| Murders | ✓ | ✓ | ✓ | ✓ | |
| Assaults | ✓ | ✓ | ✓ | ✓ | ✓ |
| Robberies | ✓ | ✓ | ✓ | ✓ | |
| Burglaries | ✓ | ✓ | ✓ | ✓ | |
| **Duration Model** | | | | | |
| Never-reporters at baseline | 70% | 60% | 80% | 70% | 70% |
| Sample of reports | Full sample | Full sample | Full sample | Recent | Recent |
| **Standard Errors** | Clustered | Clustered | Clustered | Clustered | Clustered |
| **N Observations** | 740 | 740 | 740 | 740 | 288 |

*Notes:* Estimates of the average treatment effect of #MeToo on sex crime incidence (see Equation 10) for alternative specifications of the duration model. "Recent" reports are crimes reported within 5 years of the incident date.

## D.3 Sensitivity Analysis to Dropping Cities and Crimes

Table D.2 presents estimates of the average treatment effect of #MeToo on sex crime incidence when dropping sequentially cities and crimes used for the control group.

Table D.2: #MeToo Effect on Sex Crime Incidence – Sensitivity Analysis

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | **Dependent Variable: Estimated Sex Crimes (in logs)** | | | | | | | | |
| **After #MeToo (indicator)** | -0.28 | -0.24 | -0.24 | -0.32 | -0.32 | -0.41 | -0.33 | -0.46 | -0.40 |
| | (0.12) | (0.13) | (0.13) | (0.12) | (0.12) | (0.18) | (0.15) | (0.16) | (0.15) |
| **Counterfactual Model** | DID | DID | DID | DID | DID | DID | DID | DID | DID |
| **Fixed Effects** | | | | | | | | | |
| City-Crime Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Time Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **Control Groups** | | | | | | | | | |
| Murders | ✓ | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Assaults | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Robberies | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ | ✓ |
| Burglaries | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ |
| **Cities** | | | | | | | | | |
| New York City | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| Los Angeles | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ |
| Seattle | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | | ✓ |
| Cincinnati | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | |
| Standard Errors | Clustered | Clustered | Clustered | Clustered | Clustered | Clustered | Clustered | Clustered | Clustered |
| N Observations | 740 | 592 | 592 | 592 | 592 | 592 | 592 | 592 | 592 |

*Notes:* This table presents the robustness of the proposed estimation approach to excluding individual control crimes or cities. The dependent variable is the estimated sex crime incidence (log scale), and the panel is aggregated quarterly. Column 1 is the baseline. Columns 2–5 sequentially exclude one control crime. Columns 6–9 sequentially drop one city from the sample. Standard errors are clustered at the city-crime level.

# E Mechanisms and Discussion

## E.1 Alternative Measures of Sex Crime Incidence and Reporting

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 download official consolidated crime databases between 2011 and 2019. Reported crimes are harmonized into the Uniform Crime Reporting Summary Reporting System (SRS) (Kaplan, 2021). The SRS is a crime report database aggregated by month, agency, and crime category. It is the most comprehensive database on offenses known and clearances by arrest in the United States.[26] The UCR also provides supplementary reports on homicides (Kaplan, 2019). This allows me to compute sexual and non-sexual homicide rates.

Homicides are extremely likely to be recorded by law enforcement agencies and are thus the only crime category with a reporting rate of virtually 100%. Under the assumption that violent crimes are proportionally related to violent crimes which end up being homicides, one can recover variations in the reporting rate over time. More formally, let $H_t$ denote the number of homicides, $C_t$ the number of violent crimes, and $R_t$ the number of violent crimes reported to the police, and $r_t$ the reporting rate of victims. If we assume that $H_t = aC_t$ for some unknown constant value $a$, and that $R_t = r_t C_t$, then we have

$$r_t = a \times \frac{R_t}{H_t}. \tag{14}$$

That is, the ratio of reported crimes to homicides provides an alternative estimate of variations in victim reporting over time. The critical assumption of this empirical strategy is that the ratio of homicides to violent crimes is assumed to be constant over time. Another clear limitation of this empirical strategy is that homicides are much rarer than other crimes. For instance, in the United States, the average number of sexual homicides per year over the decade was 18. This makes such an approach unsuitable for studying smaller geographical areas such as cities and counties (contrary to the duration modeling approach that relies on many observations per geographical unit). Furthermore, we expect larger uncertainty in estimates.

Relying on Equation 14, Figure E.1 presents the results of this alternative decomposition exercise. Note that the FBI's definition of rape changed in 2013 and led to a mechanical increase in the number of reported sex crimes. From 2014 to 2016, the FBI provided estimates using the old and the new definition. On average, the old definition accounted for approximately 72% of sex crimes reported in these three years. To make trends comparable over time, I rescale reported sex crime reports from 2013 to 2019 for each US state by the average share of sex crime reports that were accounted for by the old definition in these three years. Panel A suggests a substantial (five to six-fold) increase in sexual crime reporting and a substantial (70 to 80%) decrease in sexual crime

[26] The 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. 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.

incidence between 2011 and 2019, while Panel B indicates that this is not the case for non-sexual crimes. Overall, the uncovered trends are consistent with the main analysis at the city level.

For comparison, I turn to estimates of crime incidence and reporting based on the National Crime Victimization Survey (NCVS). The NCVS is a comprehensive, nationally representative survey conducted by the U.S. Bureau of Justice Statistics to gather detailed information about nonfatal criminal victimization in the United States. It collects data on both personal crimes, such as assault, robbery, and sexual violence, and property crimes, including burglary, theft, and motor vehicle theft. Using a rotating panel design, the survey interviews individuals aged 12 and older in selected households over several waves. It provides insights into the frequency, characteristics, and consequences of victimization, as well as victims' interactions with law enforcement and their reasons for reporting or not reporting crimes. Figure E.2 presents official NCVS estimates normalized at 100 for 2011. It indicates a large increase in sexual violence following #MeToo. If anything, the reporting rate of victims decreased over the period. This is very much at odds with my main results as well as my estimates based on homicide data.

I also consider emergency department visits as a proxy for sexual violence (Aizer, 2010). I rely on the National Electronic Injury Surveillance System All Injury Program (NEISS-AIP), which collects data on all types and causes of injuries treated in a representative sample of U.S. hospitals with emergency departments (EDs). Hospital personnel declare confirmed and suspected assaults, and further distinguish between sexual and non-sexual assaults. Thus, I can compute the number of emergency department visits in the United States for sexual assaults, non-sexual assaults, and other causes. Figure E.3 presents national estimates of emergency department visits between 2011 and 2019 for sexual assaults (solid line), non-sexual assaults (dashed line), and other conditions (dotted line). Observations are weighted using the official weights provided by the NEISS-AIP dataset. It indicates that ED visits flagged as sexual assaults increase by approximately 40% between 2011 and 2018. Once again, if we were to interpret this increase as an increase in sexual violence, these results would be at odds with my main results as well as my estimates based on homicide data.

In practice, both ED visits and victimization surveys are not fundamentally different from police records. They require the victim to be willing to report the crime and/or a practitioner to be willing to record it as a crime. One might expect these probabilities of reporting and recording to increase following #MeToo. Ultimately, unless one is willing to make very strong assumptions (i.e., fully transparent reporting in victimization surveys or perfect recording of sexual assaults during ED visits), it remains unclear to what extent such alternative data sources allow researchers to disentangle variations in crime reporting and incidence.

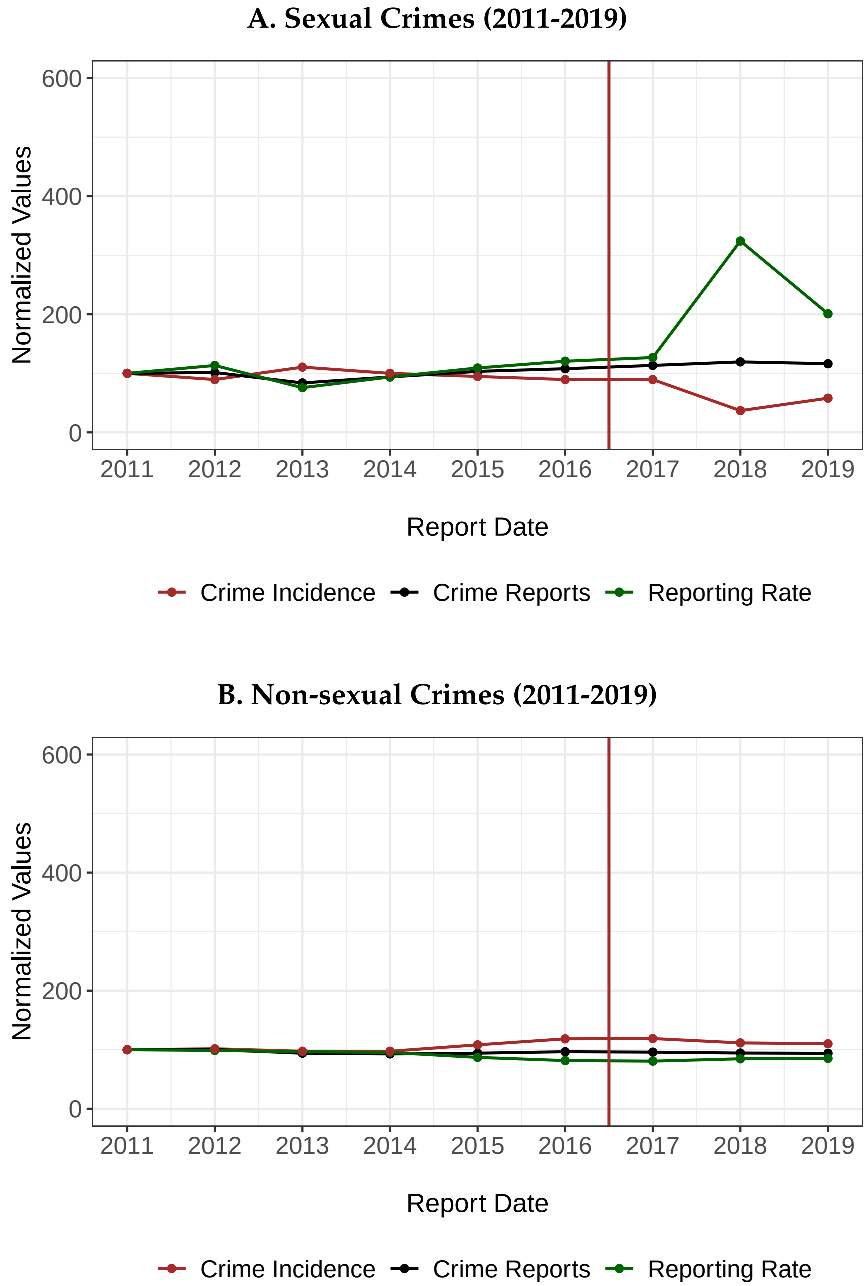



Figure E.1: National Trends in Crime Reporting and Incidence Based on Homicides

*Notes:* This figure presents national trends in crime reporting and incidence using homicides as a proxy for crime incidence based on Equation 14. The analysis relies on the FBI's Uniform Crime Reporting Summary Reporting System and its supplementary reports on homicides between 2011 and 2019 (Kaplan, 2019, 2021). Panel A plots trends for sexual crimes, and Panel B trends for non-sexual crimes. The solid vertical line separates years pre- and post-#MeToo.

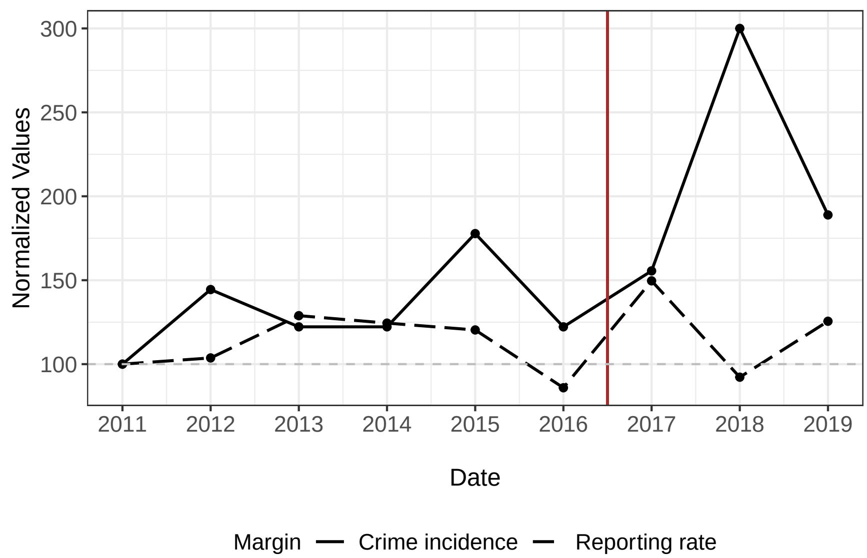



Figure E.2: National Crime Victimization Survey – Official Sexual Violence Estimates

*Notes:* Official national estimates of sex crime reporting and incidence produced by the Bureau of Justice based on the National Crime Victimization Survey (NCVS). The solid black line is for crime incidence. The dashed black line is for the reporting rate. Values are normalized at 100 for 2011. The solid vertical line separates years pre- and post-#MeToo.

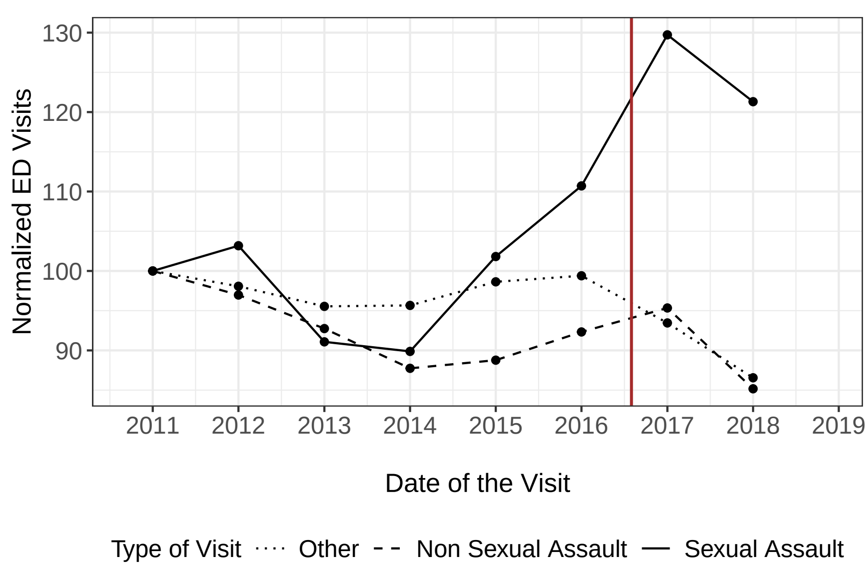
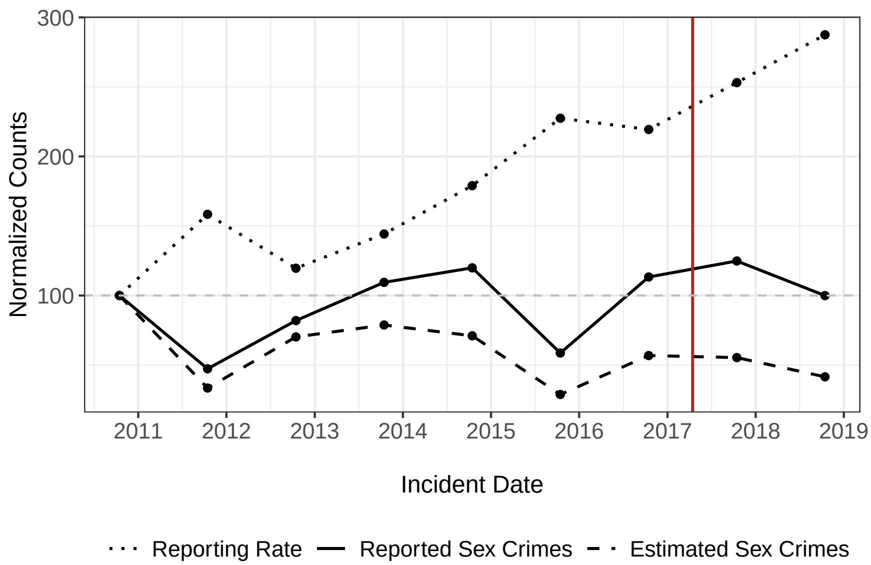



Figure E.3: Emergency Department Visits by Type of Visit

*Notes:* National estimates based on the National Electronic Injury Surveillance System All Injury Program (NEISS-AIP). Observations are weighted using the official weights provided by the NEISS-AIP dataset. I distinguish between visits for sexual assaults (solid line), non-sexual assaults (dashed line), and other reasons (dotted line). Values are normalized at 100 for 2011. The solid vertical line separates years pre- and post-#MeToo.

## E.2 Robustness to Unfounded Allegations

**A. Trends for Los Angeles – Full Sample**




**B. Trends for Los Angeles – Sample Restricted to Adult Arrests**

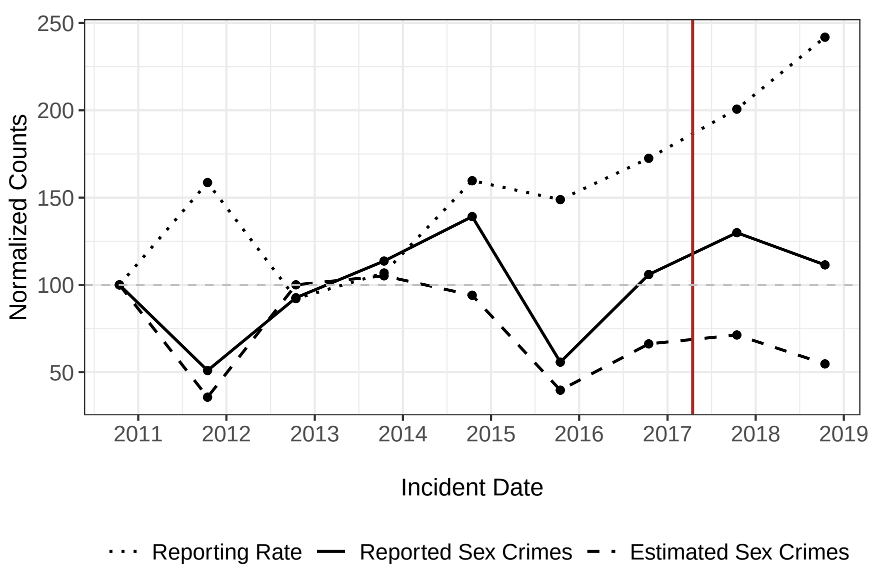
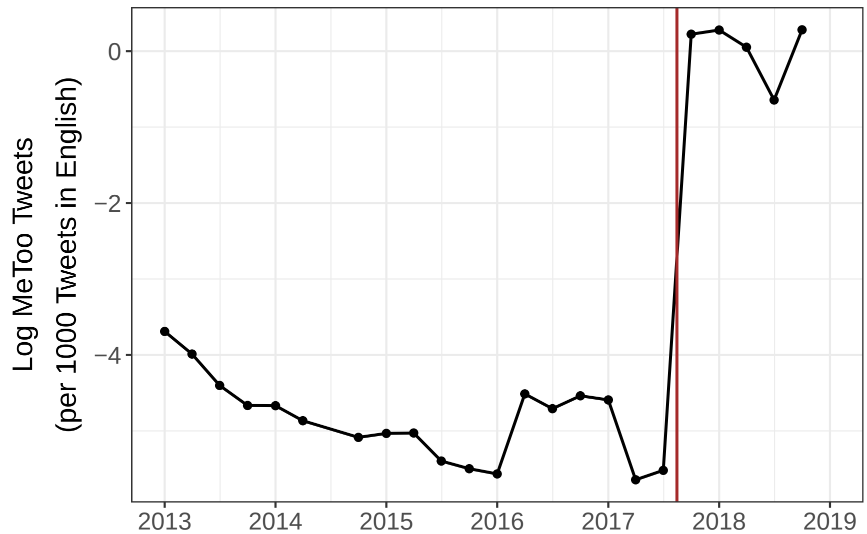



Figure E.4: Robustness to Unfounded Allegations

*Notes:* I assume 70% of never-reporters at baseline as in the main text. Breaks in the baseline hazard are set after 1, 30, 90, 180, and 365 days. The likelihood appropriately corrects for double-truncation. There is no unobserved heterogeneity. The vertical solid red line corresponds to the Me Too movement's mediatization. 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.

## E.3 Sexual Violence Awareness on Google and Twitter

**A. Me Too Tweets**




**B. Sex Crime Tweets**

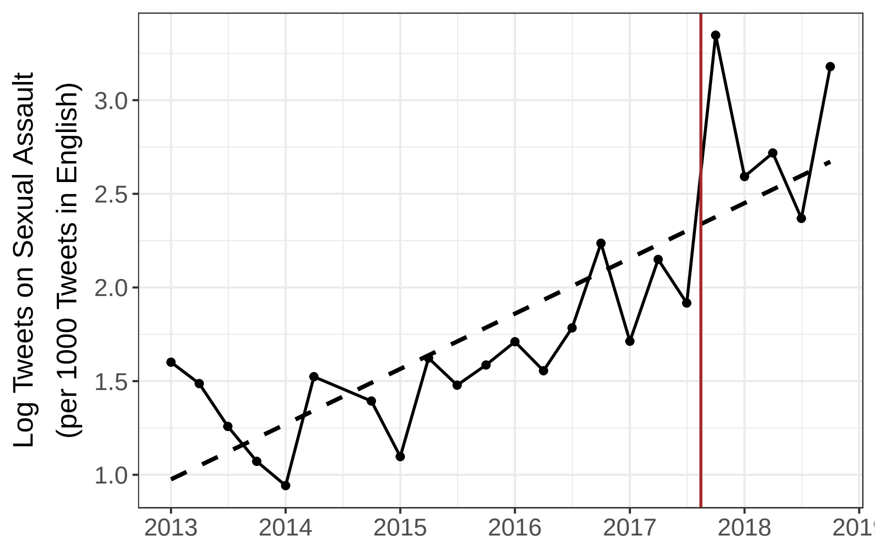



Figure E.5: Sexual Violence Awareness on Twitter

*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).

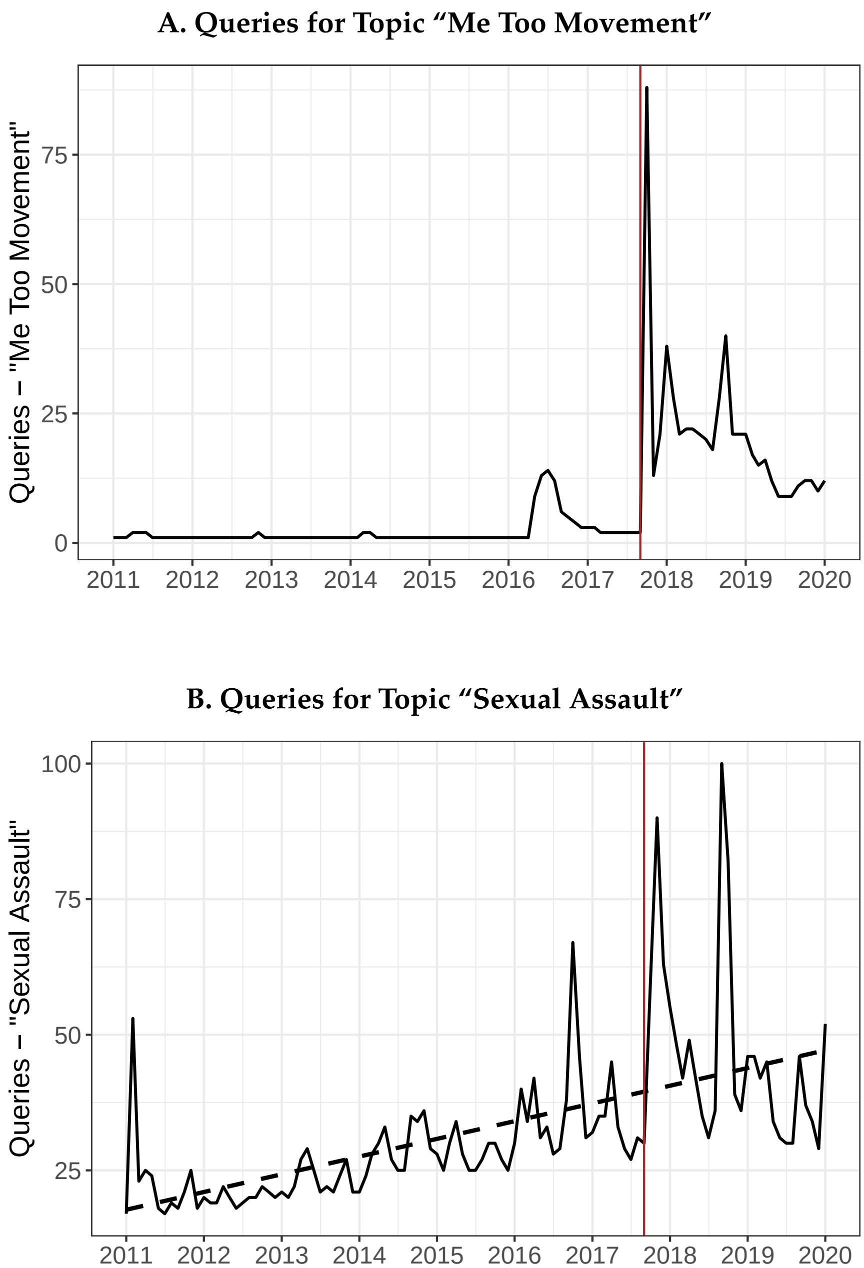



Figure E.6: Sexual Violence Awareness on Google

*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 solid vertical line separates months pre- and post-#MeToo. 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.

## E.4 Arrest Rates and Probabilities of Arrest

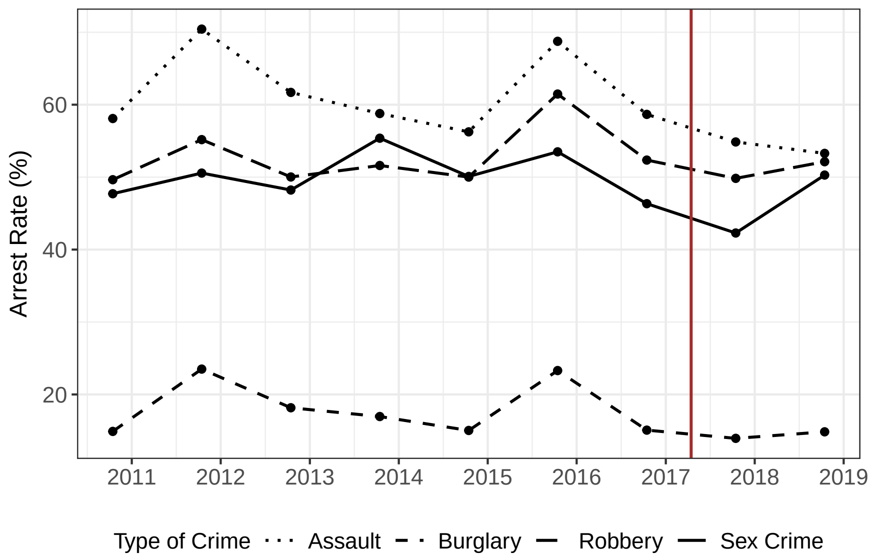



Figure E.7: Arrest Rates Over Time

*Notes:* Probabilities of arrest conditional on a complaint being filed (i.e., arrest rates) across crime categories for New York City and Los Angeles. The vertical solid red line corresponds to the Me Too movement's mediatization.

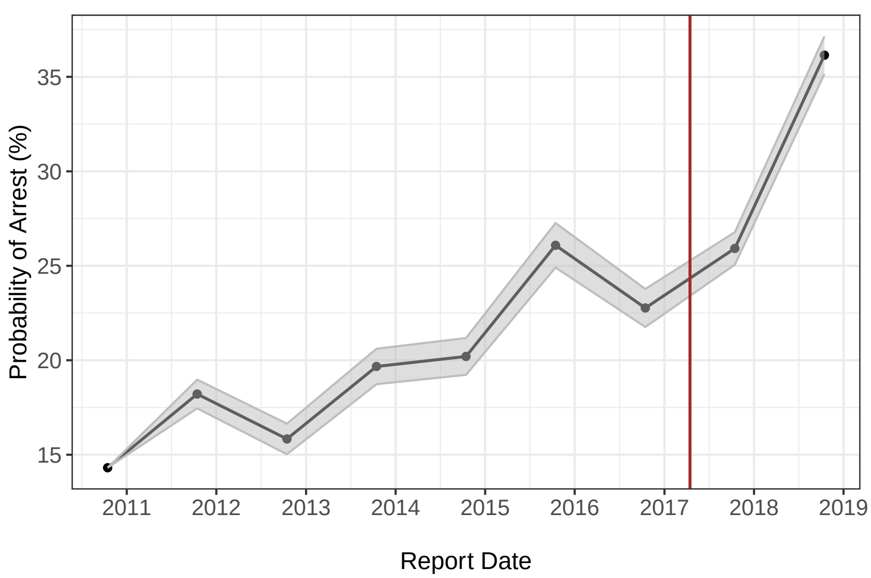



Figure E.8: Yearly Estimates of the Probability of Arrest for Sex Offenders

*Notes:* Yearly estimates of the probability of arrest conditional on committing a sex crime 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 500 iterations. The vertical solid red line corresponds to the Me Too movement's mediatization.

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