Accusing and Believing in Equilibrium: Evidence from #MeToo*

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Abstract

Many social movements encourage reporting of wrongdoing, but critics argue this increases false accusations. We study how #MeToo – a social movement that encouraged reporting of sexual harassment – changed the truthfulness of sexual harassment accusations and adjudicators' willingness to believe them. We estimate that #MeToo increased the probability of winning a sexual harassment complaint by 10.1 pp. This increase can reflect stronger complaints filed (selection) or more favorable adjudicators (direct treatment). We develop a framework to separate these effects and find evidence for both channels. Exploiting complaints filed before but resolved after #MeToo to estimate the direct treatment effect, we show that adjudicators became more likely to rule in favor of complainants, particularly male complainants. Newly induced complaints became more likely to be substantiated, with women's complaints positively selected (more credible) and men's negatively selected (less credible).

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1 Introduction

The goal of many social movements and campaigns is to increase the reporting of wrongdoing. For example, the "See Something, Say Something" campaign encourages reporting of terrorism-related activity, Black Lives Matter the reporting of police misconduct, and Environmental, Social, and Governance (ESG) initiatives the disclosure of harmful corporate behavior. But there is debate about whether more reporting exposes legitimate wrongdoing or generates excessive accusations. We study this debate in the context of the #MeToo movement, one of the largest social movements of the twenty-first century, which marked a turning point in public attitude towards sexual harassment. Starting in 2017, the movement spurred a flurry of sexual harassment and sexual assault accusations against high-profile individuals in media, politics, and academia. The goal was to decrease the costs of reporting sexual harassment (by decreasing stigma) and increase the benefits (by believing complainants more).

Public opinion diverged after #MeToo, with some believing it encouraged unsubstantiated accusations. For example, the majority of respondents in a 2018 survey[1] thought #MeToo created accountability for perpetrators, but 40% thought it went too far:

> "respondents cited a rush to judgment, the prospect of unproven accusations ruining peoples' careers or reputations, and a bandwagon effect that may prompt some to claim sexual misconduct for behavior that doesn't quite rise to that level."

There are perhaps two chiefly relevant ways to measure the "credibility" of accusations made in the wake of the movement: First, are they more likely to be true according to an objective metric?[2] Second, are those adjudicating accusations more likely to believe them? In this paper, we investigate both of these questions jointly, as they are potentially interdependent in equilibrium. If adjudicators are more likely to believe accusations, then more individuals may make accusations, and these induced reporters may or may not be selected toward truthfulness. On the other hand, if many more true (false) reports are made, then adjudicators may be more (less) likely to believe them. To

[1] See https://www.npr.org/2018/10/31/662178315/on-metoo-americans-more-divided-by-party-than-gender

[2] Throughout, we define "true" accusations as those that accurately reflect the actions taken by another individual and that meet the legal definition of sexual harassment. "False" accusations can stem from either not meeting the legal standard or a misrepresentation of events. We discuss the distinction between false and true accusations when we present our formal model.

answer these questions, we first estimate the impact of #MeToo on the probability that different populations of complainants win their cases. We then use a theoretical model to decompose these empirical estimates into two effects: a *direct treatment* effect ("Is the adjudicator more or less willing to believe a given complaint after #MeToo?") and a *selection into reporting* effect ("Are complaints induced by #MeToo more or less likely to be true than prior complaints?").

We focus on sexual harassment cases — such as inappropriate comments, requests for sexual favors, or failure to promote based on gender — brought by complainants to anti-discrimination government agencies. These charges are civil as opposed to criminal, meaning that the burden of proof the complainant must reach in order to be "believed" is a preponderance of the evidence: a winning complaint must be assessed as more likely than not to be true. State anti-discrimination agencies serve as neutral third-party arbiters, offering a standardized process to resolve discrimination disputes. Following a complaint, a representative of the anti-discrimination agency conducts an investigation into the merits of the claim, which may include visiting the employer site, conducting interviews, or reviewing personnel files. The agency then rules on whether there is probable cause to believe discrimination occurred.

Sexual harassment complaints are an ideal setting to study the joint impacts of the direct treatment and selection into reporting effects, providing insight not only into the #MeToo movement but also into social movements that encourage reporting more broadly. Three aspects make this an ideal setting. First, because of the low evidentiary threshold, it may be plausible for a non-trivial fraction of accusations to be false; with a higher burden of proof, false accusations that lack solid evidence may not have a chance of success, and so will not be made in equilibrium. Second, sexual harassment cases are frequently of a "he-said-she-said" nature and lack the forensic evidence often available in criminal cases. Given the burden of proof, small changes in how adjudicators weigh evidence can cause large changes in the overall win rate of complaints. Third, our setting lacks formal court procedures (e.g., rules on the consideration of character evidence) and therefore may give more leeway for adjudicators' beliefs to guide the final ruling.[3]

[3] Consider the case of Harvey Weinstein, a Hollywood producer who was accused of sexual assault—a criminal charge—by many women. In 2024, an appeals court overturned earlier convictions. The central disagreement was over the role of testimonial evidence in the original trial and whether it should count for his conviction. In a dissenting opinion (see https://www.nycourts.gov/ctapps/Decisions/2024/Apr24/24opn24-Decision.pdf), Judge Singas argued that the majority fails to give accurate weight to women's accusations, thereby advantaging Weinstein. She stated, "by whitewashing the facts to conform to a he-said/she-said narrative...this Court has continued a disturbing trend of

In the first part of the paper, we study the average impact of #MeToo on the outcomes of workplace sexual harassment complaints. To do so, we compile a new dataset of employment discrimination complaints filed with anti-discrimination agencies across 17 states between 2010 and 2022, for a total of 192,000 complaints. We observe the basis of discrimination alleged, filing and resolution dates, the outcome, and in some states the complainant's name and gender.[4]

We use difference-in-differences to estimate how #MeToo changed the outcomes of sexual harassment complaints as compared to a control group of other discrimination complaints. Age, disability, nationality, race, religion, and retaliation discrimination complaints form our control group. Our main outcome is the probability that the complainant wins (i.e., there is probable cause to believe discrimination occurred). The win rate is a function of both the quality of evidence presented in the complaint as well as how favorable adjudicators are, conditional on a certain evidence level. An increase in the win rate can therefore reflect complaints filed with better evidence, more favorable adjudicators, or both. Since the average impact of #MeToo combines the selection and direct treatment effects, we refer to it as the *combined effect*. We find that #MeToo had a combined effect of 10.1 percentage points, increasing the probability that sexual harassment complainants win by 66% over the control mean. We also use a triple difference specification to examine effects by gender and find that #MeToo's combined effect on women's win rate is not statistically different from its combined effect on men's win rate. While this finding may be surprising given #MeToo mainly focused on female victims, it potentially masks substantial gender heterogeneity in both selection into reporting and direct treatment.

The second part of our paper develops a framework for comparing the credibility of reports before and after a policy change, even when the evidence presented in any particular report is not directly observable in the data. We present a simple model where true complaints are assumed to typically generate more evidence than false ones. Adjudicators base their rulings on the amount of evidence present. Because true cases tend to generate more evidence, in a large enough sample, true cases will win more often on average. This provides the link between average win rates in the data and the underlying composition of true versus false complaints: all else equal, a sample with

overturning juries' guilty verdicts in cases involving sexual violence." The cases we study do not have this constraint, allowing for testimonial evidence to play a strong role in determining final outcomes.

[4]The defendant is typically listed as an employer rather than an individual being accused, so we cannot consider heterogeneity by defendant gender.

a higher win rate should contain a larger share of true complaints.

Combining our model with two empirical assumptions allows us to decompose the combined effect. The key intuition behind our decomposition is that the post-period includes two types of reporters: *always reporters* (a subset of reporters who are as likely to make a true accusation as reporters pre-#MeToo) and *induced reporters* (the remaining reporters, who may be more or less likely to file true complaints). Importantly, because always reporters are neither positively nor negatively selected by definition, the impact of #MeToo on the win rate for always reporters reflects only the direct treatment effect on adjudicators. We prove that, under our assumptions, the combined effect can be written as a weighted average of the average treatment effects for these two groups, where the weights correspond to their shares in the post period.

To estimate the effect for always reporters, we exploit *overlap complaints* — cases filed before but resolved after #MeToo — which, by construction, could not have been filed in response to the movement. Since overlap complaints are not subject to concerns about selection into reporting, the impact of #MeToo on these cases is the impact of #MeToo on always reporters (and therefore the direct treatment effect). Comparing this overlap effect to the combined effect reveals the direction of selection for induced reporters. Intuitively, if the overlap effect is smaller than the combined effect, induced reporters must be positively selected to make up the difference; if it is larger, they must be negatively selected.

We find that the effect on overlap complaints is 8.7 percentage points, smaller than the combined effect of 10.1 percentage points. This leads to two findings of economic interest. First, the effect on overlap complaints is positive, and, through the lens of our model, this implies that adjudicators were more likely to believe accusations of sexual harassment post-#MeToo, holding the characteristics of the complaint fixed. Second, since the effect without selection is smaller than the combined effect, the effect on induced reporters must be positive and large to make up the difference. In fact, we estimate that the treatment effect on induced reporters is larger than the treatment effect on always reporters for every possible share of these groups in the data. This implies that complaints induced to report by #MeToo are *positively* selected in terms of their truthfulness.

However, these aggregate findings disguise interesting gender heterogeneity: the positive selection in new cases is driven by women. The effect for women in overlap complaints is 8.6 percentage points (men is 15.3 pp), while the overall effect for women is 12.3 percentage points (men is 11.9 pp). This reveals a striking pattern: female induced reporters after #MeToo are positively selected across all possible group shares, whereas male induced reporters are *negatively* selected. In other words, men induced to report by #MeToo are more likely to make a false accusation than are male always reporters. Moreover, for our preferred calibrations, the selection-into-reporting effect for men is so large that it dominates the treatment effect of adjudicators; male induced reporters are actually less likely to win their case as a result of #MeToo than were male reporters prior to #MeToo.

Overall, our analysis suggests that adjudicators were more likely to believe sexual harassment claims by both men and women after #MeToo. This effect is particularly large for men – men who were already filing sexual harassment complaints before #MeToo were now more likely to be believed. But the new reporters exhibit a different pattern. Contrary to criticism that #MeToo encouraged women to report "bogus" cases, we find women started reporting sexual harassment complaints that were, on average, more likely to be true. Men, on the other hand, started reporting sexual harassment complaints that were less likely to be true.

1.1 Related Literature

There is a growing empirical literature studying the effects of the #MeToo movement. The closest study to ours is Levy and Mattsson (2025), which uses crime data from the United States and other OECD countries to estimate the average impact of #MeToo on sex crime reporting and arrests. They document a 10% increase in reported sex crimes and a corresponding rise in arrests for sexual assault, with larger effects for female victims and in politically liberal U.S. counties. They interpret these findings as evidence of increased reporting, rather than increased incidence, of sexual violence.

Levy and Mattsson (2025) provide careful empirical estimates of how #MeToo shifted public responsiveness to sexual crimes and offer an important international perspective. Our study builds on this work in two key ways. First, we focus on civil administrative cases, specifically complaints filed with state anti-discrimination agencies. Our data capture harassment that is arguably more representative of the workplace incidents that #MeToo most directly sought to address, rather than severe misconduct found in crime data. The milder nature of these complaints makes the question of substantiation particularly salient. While it is encouraging that #MeToo increased accountability for severe offenses, our data suggest an even larger response—complainants are 66% more likely to win their case— perhaps reflecting a greater margin for change in the assessment of lower severity cases that we study. Second, and more centrally, our contribution goes beyond estimating aggregate effects. We provide a framework to decompose the overall treatment effect, allowing us to distinguish between changes in adjudicator belief and changes in the composition of complaints. This approach sheds light on the types of individuals induced to report by social movements and on whether those newly reported cases are more or less credible.

This paper also contributes to the growing empirical literature on sexual harassment. Folke and Rickne (2022) use Swedish data to document how sexual harassment varies systematically by workplace, with women reporting the most sexual harassment in male-dominated workplaces where wages are high. Our paper studies what happens downstream once sexual harassment is reported and how changes in social attitudes impact who is believed. Adams-Prassl, Huttunen, Nix, and Zhang (2022) use linked Finnish data to examine the labor market effects of violence against women in the workplace. They find a decline in female employees in the wake of an assault, but only for male-managed firms. Boudreau, Chassang, González-Torres, and Heath (2024) show that there are costs of reporting sexual harassment, which are based at least in part on a fear of retaliation and are endogenous to the reporting and punishment institutions in use.

Our modeling approach also relates to the literature on the design of punishment mechanisms started by Becker (1968). Specifically, our theoretical framework nests modern modeling techniques presented in Kaplow (2011), whereby an adjudicator uses a threshold rule on the amount of evidence necessary for "believing" an accusation, where the threshold optimally trades off type I error (believing a false allegation) and type II error (not believing a true allegation). We make no attempt to explicitly model or measure the adjudicator's preferences over these types of errors, but allow for #MeToo to change the evidentiary threshold by raising the cost of a type II error and/or reducing the cost of a type I error. Importantly, this discussion clarifies that our model allows for accusations to be false or fabricated, and we use our model to measure the effect of #MeToo on the truthfulness of induced reports. Therefore, our model differs from other models of the #MeToo movement, such as Cheng and Hsiaw (2022); Boudreau et al. (2024), who assume no falsification of allegations is possible. Cheng and Hsiaw (2022) study the impact of coordination failures when multiple accusations of the same defendant are potentially necessary for the claims to be believed; our model differs by investigating reporting agents independently, although a decrease (increase) in the evidentiary threshold post #MeToo in our model can serve as a reduced form for #MeToo increasing (decreasing) the probability of corroborating accusations by others. Our model is also flexible enough to allow for changes in the cost of reporting sexual harassment after #MeToo, perhaps by reducing the risk of retaliation (Boudreau et al., 2024).

Finally, the direct treatment effects we estimate reflect changes in adjudicator behavior rather than changes in legal regimes. In this capacity, our paper speaks to literature studying the effect of non-legal factors on judge behavior, which has found that judges are most lenient after a lunch break (Danziger, Levav, and Avnaim-Pesso, 2011), football team performance impacts juvenile court decisions (Eren and Mocan, 2018), and higher outside temperature makes judges less favorable to immigration applicants (Heyes and Saberian, 2019). But unlike these examples of personal or fleeting factors affecting judges, the #MeToo movement represented a society-wide, durable shift in attitudes towards sexual harassment. In this regard, our paper relates to work on how judges' worldviews affect legal decisions, such as Ash, Chen, and Naidu (2022), which shows how economics training made judges more conservative in their rulings.

The remainder of the paper is organized as follows: Section 2 describes the #MeToo movement and institutional context, Section 3 describes our data, Section 4 presents our empirical strategy, Section 5 presents our aggregate estimates, Section 6 presents a model of the institutional setting and adecomposition of the combined effect into objects of interest, Section 7 presents the results of the decomposition, and Section 8 concludes. Proofs, additional tables and figures, and additional data details are housed in the Appendix.

2 Background

2.1 The #MeToo movement

Over half of women and around a fourth of men in the US report experiencing sexual harassment in their lifetime.[5] On October 15, 2017, the #MeToo movement gained widespread attention when actress Alyssa Milano tweeted asking people who had experienced sexual harassment to respond with the phrase "me too." Figure 1 shows that Google searches for #MeToo were roughly zero before October 2017 but spiked immediately thereafter.

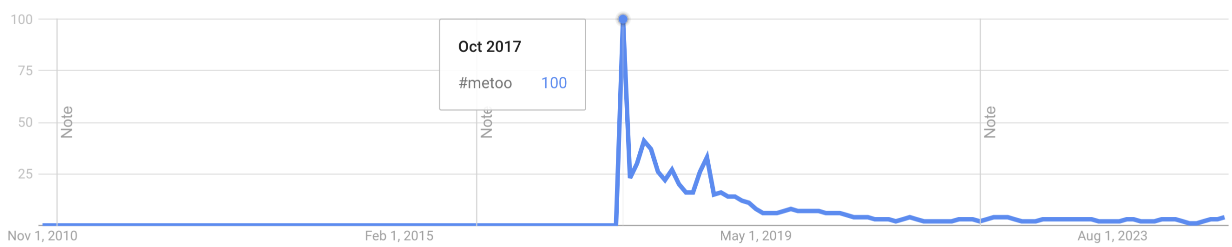


Figure 1: Google Trends timeseries of #MeToo searches from 2010 to 2024 showing a peak in October 2017. The y-axis is Google's measure of the relative popularity of the search term over time, with 100 being peak popularity.

The movement increased the salience of sexual harassment as a societal issue. #MeToo was used over 19 million times on Twitter in the first year alone (Anderson and Toor, 2018). In December 2017, Time magazine named #MeToo activists as their Person of the Year.

2.2 Legal protections against sexual harassment

Sexual harassment in employment is considered a form of sex-based discrimination under Title VII of the Civil Rights Act of 1964. The Equal Employment Opportunity Commission (EEOC) – the federal agency responsible for enforcing federal laws that protect against discrimination in employment – defines sexual harassment as "unwelcome sexual advances, requests for sexual favors, and other verbal or physical conduct of a sexual nature" that is made explicitly or implicitly a condition of employment, used as a basis of dismissal from employment, or interferes with the

[5] In a 2018 Pew survey, 59% of women and 27% of men reported experiencing unwanted sexual advances or harassment (Pew Research Center, 2018). A 2019 poll revealed even higher numbers, with 81% of women and 42% of men reporting lifetime experiences of sexual harassment (Stop Street Harassment, 2019).

individual's work (Legal Information Institute).[6] State definitions of sexual harassment do not depart significantly from the EEOC definition; in fact, many states default to the EEOC definition. This homogeneity in definitions matters because it ensures that states classify similar incidents as harassment, allowing us to aggregate treatment effects meaningfully across states.[7]

2.3 Filing a sexual harassment complaint

Sexual harassment is typically treated as a civil offense and addressed through administrative complaints or civil lawsuits. However, serious instances of sexual harassment, such as rape, physical assault, or stalking, can rise to the level of a criminal offense or may be classified as a different type of sex crime. This paper focuses on civil complaints.

In civil cases, an individual ("complainant") can file a complaint against a respondent with an anti-discrimination agency at the state or federal level. We do not observe complaints filed with firms' Human Resources (HR) departments, nor do we observe whether an individual previously filed with these other outlets before filing with a state agency. According to officials who work at these state agencies, complainants may not report sexual harassment to their employer for one of the following reasons: they fear retaliation, they do not trust the HR department, a member of the HR department conducted the alleged discrimination, they want legal remedies for the situation, or their company does not have an HR department.

3 Data

To obtain data on discrimination complaints, we filed Freedom of Information Act (FOIA) requests with all U.S. states that have relevant agencies (Arkansas and Mississippi do not). Our request sought all closed employment, public accommodation, housing, and education discrimination complaints filed between June 1, 2010, and June 1, 2023. We requested details including the respondent's name, filing and resolution dates, alleged basis and issue of discrimination, the agency's decision, and any compensation awarded. Table A1 summarizes the FOIA results by state,

[6] Figure A1 provides specific examples of sexual harassment.

[7] Some states have expanded protections beyond federal regulations. These expansions include covering smaller employers, mandating specific training, allowing personal liability for harassers (whereas federal law only allows employers to be named in lawsuits), and explicitly protecting sexual orientation (Justia; Vigilant; Higgins Law; P.C.; Maine Legislature).

with additional process details in Appendix D.

Our analysis focuses on employment discrimination. We harmonize the alleged bases of discrimination with the EEOC categories: race, religion, sex (including sexual harassment), nationality, age, disability, and retaliation (punishment for filing a discrimination complaint).[8]

Our treatment group consists of sexual harassment complaints, and other types of discrimination form our control group. We drop a small number of sexual harassment cases not classified as sex discrimination, as well as sex discrimination that is not sexual harassment (e.g., pregnancy discrimination). Retaliation complaints that reference sex anywhere in the basis are treated as part of the treatment group, as they reflect backlash against sex-based allegations.

We exclude complaints lasting over 764 days (more than two standard deviations above the mean of 262 days). Our final sample contains 191,985 employment discrimination complaints. We use complaints from public accommodation, housing, and education sectors as a robustness check, and this larger sample contains 345,000 complaints.

After filing, a discrimination complaint can be closed in multiple ways. A complaint may be dismissed, be adjudicated in court, be settled by the parties (where no determination of liability is rendered but the complainant can receive compensation), or proceed to investigation to determine whether or not there was probable cause that discrimination occurred. The investigation is conducted by agency officials and may include interviews with relevant parties, evidence gathering, or site visits. Figure 2 presents a flowchart of the complaint process.

Our main outcome of interest is the outcome of the investigation stage, as the investigation finding offers a clear signal as to the level of evidence supporting the complainant. We consider a complainant to have "won" if there is a finding of reasonable or probable cause during the investigation. We consider the complainant to have "lost" if no probable cause is found. If the complainant wins, potential remedies include the defendant paying compensation to the victim, the employer implementing new policies, or other arrangements to compensate the complainant. Since these are civil proceedings, no criminal charges or jail time are involved. Data on remedies

[8] Cases whose bases do not fit into the main categories, such as genetic, police, veteran, drug, and other types of miscellaneous discrimination comprise 2% of the data and are excluded from the analysis. For discrimination complaints that have multiple bases, we take the first basis if the state does not provide priority ordering. https://www.eeoc.gov/employers/small-business/3-who-protected-employment-discrimination

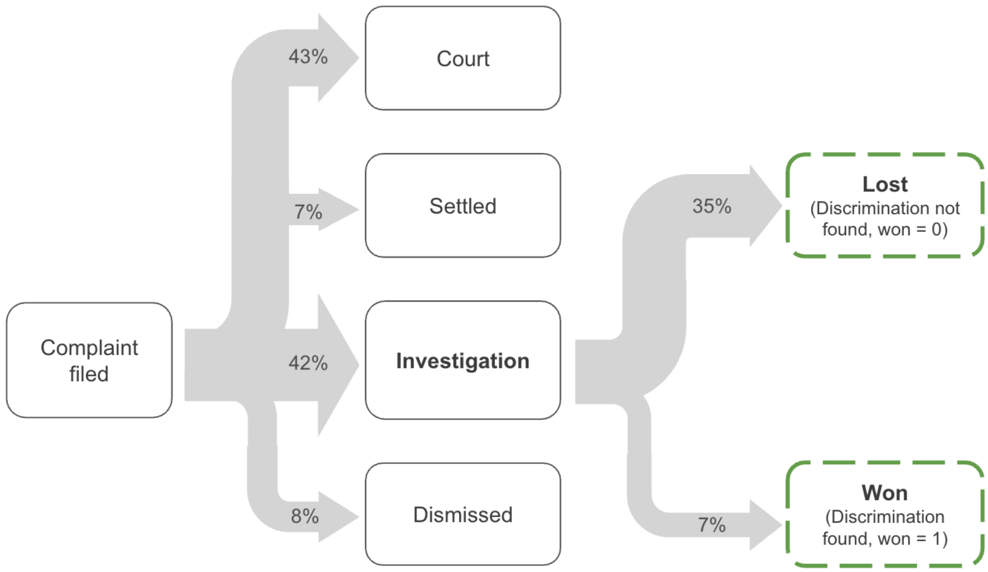


Figure 2: Complaint flow with outcomes in boxes with green dashed lines. All terminal boxes add to 100%.

was sparsely provided by states, and we therefore do not consider it as an outcome in our analysis.

We also study other outcomes in the Appendix, which are qualitatively different because they do not terminate following an investigation at the agency level. We consider a case "Settled" when the raw data indicates a settlement, mediation, or withdrawal with adjustment. While the outcomes of settlements generally are not observable to the public, since the goal of a settlement is to privately agree on redress without admitting blame, we have data on the compensation paid to the complainant in 96% of settlements. A case ends in "Court" if the case is removed to a court proceeding overseen by a judge or magistrate. We do not observe the outcomes of complaints that were removed to court. We define a case as "Dismissed" if it is administratively closed, withdrawn (without settlement), or labeled as "Dismissed" in the raw data. Administrative closures include cases with logistical issues, such as being out of the agency's jurisdiction, being filed in an untimely manner, or being unable to contact the complainant. No restitution is made in such cases, as they exit prior to a thorough investigation.[9] We provide additional details on mapping the raw data into case outcomes in Appendix D.

[9]Complaints are often dismissed for administrative reasons, including when the complainant cannot be located despite assiduous efforts, when the complainant or defendant has declared bankruptcy or passed away, when the complainant has filed an equivalent federal complaint, or when the complainant refuses to cooperate in the resolution process. Some complaints are dismissed for insufficient evidence, but the evidentiary threshold is likely low given the small percentage of cases that are dismissed outright.

Approximately 7% of cases settle, 8% are dismissed outright, and 43% go to court, where we do not observe their outcome. Of the remaining 42% of cases that go to investigation at the anti-discrimination office, 83% (.35 / .42) lose. Table 1 provides summary statistics of interest, and provides a comparison of differences in means of complaints before and after #MeToo. Figure A2 plots the increase in the number of sexual harassment complaints filed after #MeToo.

4 Empirical strategy

We estimate the effect of #MeToo on sexual harassment complaints using difference-in-differences. Treatment timing is the start of the #MeToo movement on October 15, 2017 and is constant across all states. Our main estimating equation is a two-way fixed effects specification within a state:

$$Y_{ist} = \beta^{CE}(\text{SH} \times \text{Post})_{it} + (\alpha_i \times \phi_s) + (\tau_t \times \phi_s) + \epsilon_{ist} \tag{1a}$$

where $i \in \{\text{sexual harassment, age, disability, nationality, race, religion, retaliation}\}$ indexes complaint type, t indexes the year-month of when the complaint was resolved, and s indexes state. $(\text{SH} \times \text{Post})_{it} = 1$ if i is a sexual harassment resolved after #MeToo. Y_{ist} is an indicator for whether the complainant wins their case (versus loses). We cluster standard errors at the complaint type level throughout.

β^{CE} ("combined effect") is our coefficient of interest. It represents the average treatment effect on the treated (ATT) under a parallel trends assumption. We assume that in the absence of #MeToo, the outcomes of sexual harassment and control complaints would have trended in parallel within a state. Let $Y_{ist}(1)$ be the potential outcome if a complaint had received treatment and $Y_{ist}(0)$ if a complaint had not received treatment. Let D_{ist} indicate actual treatment status, and $t = 0$ be the post period and $t = -1$ be the pre-period in a two period model. Then, this assumption is $E[Y_{is0}(0) - Y_{is-1}(0)|D_{is0} = 0] = E[Y_{is0}(0) - Y_{is-1}(0)|D_{is0} = 1]$, and we refer to this as the *standard parallel trends assumption* throughout.

We call β^{CE} the combined effect because it captures the effect of changes in the probability of winning, holding fixed case characteristics, as well as the effect of changes in the composition of cases reported. Ex ante the sign of β^{CE} is unclear. β^{CE} may be positive (negative) if #MeToo induces

TABLE 1 — COMPLAINT CHARACTERISTICS

	Sample Statistics		Mean Difference (Post-Pre) MeToo	
	All	SH only	All	SH only
Complaint Characteristics				
Sexual harassment	0.171	1.000	-0.096***	.
	[0.38]	[0.00]	(.0019)	.
Complainant is female	0.499	0.741	-0.027***	0.041***
	[0.50]	[0.44]	(.004)	(.0098)
Filed after MeToo	0.724	0.837	.	.
	[0.45]	[0.37]	.	.
Duration (days)	162.583	117.125	65.204***	150.072***
	[176.71]	[167.06]	(.89)	(2.4)
Discrimination basis				
Age	0.074	.	0.049***	.
	[0.26]	.	(.0013)	.
Disability	0.337	.	-0.038***	.
	[0.47]	.	(.0024)	.
Nationality	0.030	.	0.024***	.
	[0.17]	.	(.00088)	.
Race	0.254	.	0.098***	.
	[0.44]	.	(.0022)	.
Religion	0.027	.	-0.008***	.
	[0.16]	.	(.00082)	.
Retaliation	0.110	0.006	-0.026***	0.019***
	[0.31]	[0.08]	(.0016)	(.0012)
Sex	0.168	0.986	-0.098***	-0.030***
	[0.37]	[0.12]	(.0019)	(.0018)
Outcomes				
Settled	0.068	0.059	0.043***	0.082***
	[0.25]	[0.24]	(.0013)	(.0035)
Won	0.066	0.082	0.092***	0.216***
	[0.25]	[0.27]	(.0013)	(.0039)
Lost	0.355	0.184	0.240***	0.268***
	[0.48]	[0.39]	(.0024)	(.0056)
Dismissed	0.074	0.063	0.047***	0.094***
	[0.26]	[0.24]	(.0014)	(.0036)
Went to court	0.438	0.609	-0.418***	-0.657***
	[0.50]	[0.49]	(.0024)	(.0063)
Observations	191,985	32,737	191,985	32,737

Table 1: SH is sexual harassment. Columns 1 and 2 present the means and standard deviations in parentheses of variables in the entire sample and the sample of sexual harassment complaints. Columns 3 and 4 present the difference in means of each variable after minus before #MeToo, with the standard deviation of the t-test in parentheses. Sexual harassment can be classified as Retaliation in the data, so Retaliation is non-zero in the "SH only" column.

adjudicators to believe more (fewer) accusations. Or it may be positive (negative) if #MeToo leads to an influx of true (false) complaints where discrimination is more (less) likely to be found.

We include interactions with state dummies ϕ_s to account for differences in how complaints are adjudicated between states. $(\alpha_i \times \phi_s)$ account for constant state policies that differentially affect sexual harassment and non-sexual harassment outcomes. $(\tau_t \times \phi_s)$ account for time-varying state policies that affect all cases the same. For example, in the six years following the start of #MeToo, half of all states enacted additional anti-harassment laws to further protect workers (National Women's Law Center, 2023). Since these laws applied to all discrimination cases, time-by-state fixed effects control for the impact of these policies. What is captured in β^{CE} are any policies instituted after #MeToo that may have *differentially* impacted sexual harassment and non-sexual harassment complaints, changes in adjudicator behavior, and compositional changes in complaints, all of which we consider part of the treatment of #MeToo.

For our event studies, we estimate a dynamic version of equation 1a, where k indexes yearly leads and lags around #MeToo, $t = -1$ is the 12 month period before October 15, 2017, and $\text{SH}_i = 1$ if i is a sexual harassment complaint:

$$Y_{ist} = \sum_{k \neq -1} \beta(k) \mathbb{1}\{t = k\} \text{SH}_i + (\alpha_i \times \phi_s) + (\tau_t \times \phi_s) + \epsilon_{ist} \tag{1b}$$

In the third specification, we estimate the differential impact of #MeToo for women versus men using triple differences. Specifically, we estimate a version of Equation 1a but interacted with whether the complainant is female:

$$\begin{aligned} Y_{ist} = {} & \beta^{DDD}(\text{SH} \times \text{Post} \times \text{Female})_{it} + \beta^{CE}_M (\text{SH} \times \text{Post})_{it} \\ & + (\alpha_i \times \phi_s) + (\tau_t \times \phi_s) \\ & + (a_i \times \phi_s \times \text{Female}_i) + (\tau_t \times \phi_s \times \text{Female}_i) + \epsilon_{ist} \end{aligned} \tag{2a}$$

We make an analog of the standard parallel trends assumption, and assume that the outcomes of sexual harassment and control cases brought by *male* complainants trended in parallel within a state, under which β^{CE}_M identifies the ATT for male complainants. To identify $\beta^{CE}_W := \beta^{DDD} + \beta^{CE}_M$ as the ATT for female complainants, we make an analog of the parallel trends assumption: that in the absence of #MeToo, the difference in outcomes for sexual harassment versus control complaints filed by women would have trended in parallel to the difference in outcomes for sexual harassment versus control complaints filed by men (Olden and Møen, 2022).

Finally, we estimate the dynamic version of Equation 2a:

$$\begin{aligned} Y_{ist} = & \sum_{k \neq -1} \beta^{DDD}(k) \mathbb{1}\{t = k\}(\text{SH}_i \times \text{Female}_i) + \sum_{k \neq -1} \beta_M^{CE}(k) \mathbb{1}\{t = k\} \text{SH}_i \\ & + (\alpha_i \times \phi_s) + (\tau_t \times \phi_s) \\ & + (a_i \times \phi_s \times \text{Female}_i) + (\tau_t \times \phi_s \times \text{Female}_i) + \epsilon_{ist} \end{aligned} \tag{2b}$$

5 Results

We present estimates of Equation 1a in Table 2, Column 1, and the event study that corresponds to Equation 1b in Figure 3a. We estimate that #MeToo raised complainant win probability by $\hat{\beta}^{CE} = 10.1$ percentage points (std. err. = 1.6) or 66% over the control mean. A visual inspection of the event study shows that pre-treatment coefficients are not statistically significant, and there is little evidence of a pre-trend. Post-treatment coefficients are positive, significant, and indicate an average impact of #MeToo that aligns with the point estimate of the static regression. In Appendix C, we show that our findings are robust to multiple changes in specifications and data choices.

We show results for secondary outcomes—rates of settling and exiting to court—in Table A2. #MeToo had virtually no impact on the propensity to settle and a minor increase in the likelihood of exiting to court (0.7 percentage points, std. err. = 0.3) that is unlikely to be economically significant. We present the event studies for these secondary outcomes in Appendix Figures A3a – A3b.

Next, we examine heterogeneity by complainant gender. Since we only observe complainant gender in a subset of states, we first assess whether these states differ systematically from those that did not provide gender data. In Column 2 of Table 2, we re-estimate Equation 1a only in the subset of states with gender information. The estimated effects are similar to those in the full sample: the effect on case wins is nearly identical (0.101 vs. 0.116). This similarity provides reassurance that our main results are not driven by differences in sample composition across states.[10]

[10]States with and without gender information also do not notably differ in their female labor force participation (FLFP) rates or gender wage gaps. States that provided gender information had an average FLFP and gender wage gap of 58%

FIGURE 3a — IMPACT OF #METOO ON PROBABILITY COMPLAINANT WINS

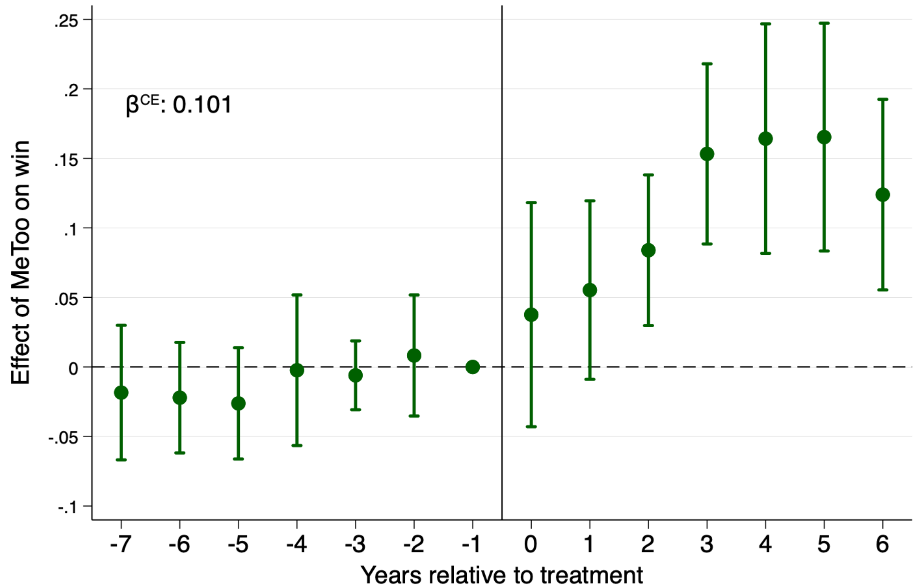


FIGURE 3b — IMPACT OF #METOO ON PROBABILITY COMPLAINANT WINS, BY GENDER

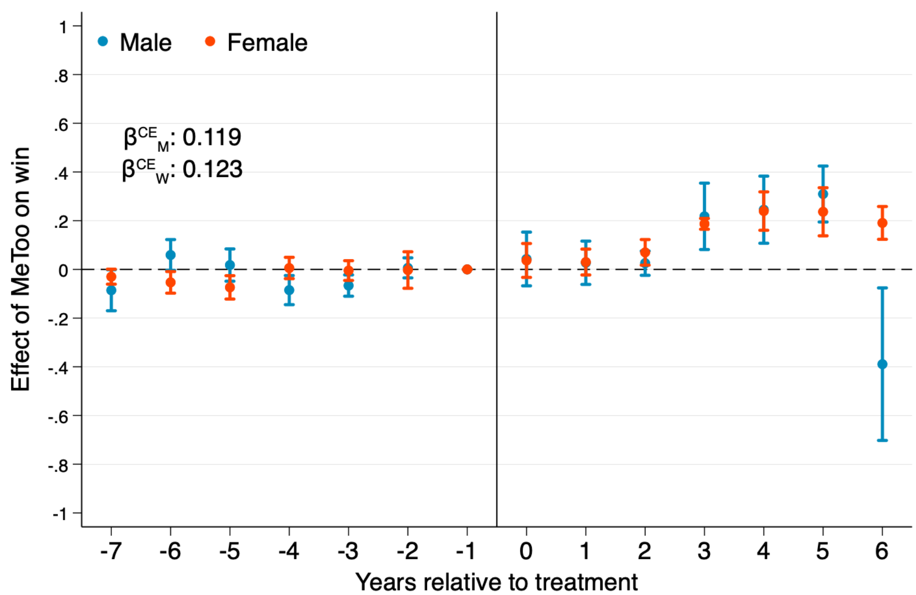


Figure 3: These figures provide estimates of Equations 1b and 2b. The x-axis in all subfigures represents 12-month intervals centered around the #MeToo start date. $t = -1$ is the 12 months preceding October 15, 2017, $t = 0$ is the 12-month period following October 15, 2017, and so on. In the top panel, we control for unit-by-state and year-month-by-state fixed effects, where the "year-month" specifies the date of the complaint resolution. In the bottom panel, we additionally interact these same fixed effects with complainant gender.

TABLE 2 — EFFECT OF #METOO ON LIKELIHOOD COMPLAINANT WINS SEXUAL HARASSMENT CASE

	All complaints	Complaints with gender	
SH × Post	0.101***	0.116***	0.119***
	(0.016)	(0.018)	(0.026)
SH × Post × Female			0.004
			(0.018)
Unit and Time × State FE	✓	✓	✓
Unit and Time × State × Female FE			✓
N	73,995	46,750	46,746
R^2	0.171	0.089	0.096
Control mean	0.152	0.226	0.229

Table 2: SH is sexual harassment. Columns 1 and 2 present estimates of Equation 1a. Column 3 presents estimates of Equation 2a. Fixed effects are basis of discrimination by state and year-month of case resolution by state. For the triple differences specification, the fixed effects are basis of discrimination by state by female complainant and year-month of case resolution by state by female complainant. Standard errors clustered at the basis (unit) level. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

A priori, we expected any effect of #MeToo to be largest for female complainants. This is not the case. In Column 3 of Table 2, we show the results of estimating Equation 2a. We find that female complainants do not benefit more from #MeToo compared to male complainants. #MeToo increases the probability that a man wins his case by $\hat{\beta}_M^{CE} = 0.119$ (std. err. 0.026), which is not significantly different from women ($\hat{\beta}_W^{CE} - \hat{\beta}_M^{CE} = 0.004$, std. err. 0.018). Figure 3b presents the corresponding event study, again exhibiting little evidence of a pre-trend. The orange coefficients for women closely align with the blue coefficients for men in every period. The effect on men dissipates only in the last period, six years after #MeToo. More broadly, Table A2 shows no meaningful gender differences across any outcomes.

6 Theoretical Framework and Empirical Decomposition

Our goal in this section is to decompose β^{CE} into a selection effect — how the average truthfulness of complaints changes after #MeToo — and an adjudicator treatment effect — how #MeToo affects adjudicator favorability toward complainants. To do so, we begin by presenting a simple theoretical

and 86%, respectively, compared to 57% and 83% for states that did not provide gender information (U.S. Bureau of Labor Statistics, 2023, 2024).

framework. In it, each individual considers whether to make a report of sexual harassment with the pre-existing knowledge of whether their potential accusation is true or false, where a true accusation is one that both accurately captures events experienced by the complainant and rises to the legal definition of sexual harassment. Making an accusation is costly but leads to probabilistic payments if their accusation is believed by an adjudicator. We use the model to study the optimal decision to report instances of sexual harassment (either truthfully or untruthfully) and to understand adjudicators' decisions to "believe" the accusations or not. We then consider how changes to parameters of the model after #MeToo affect these decisions, which allows for a decomposition result that we can take to the data.

After presenting the simple model, we discuss how its predictions are robust to a number of alternative assumptions. For example, while we present a model where individuals know the truthfulness of their potential accusations before they decide to report, our findings are essentially unchanged if instead individuals are unsure of the truthfulness of their claim, i.e., if they do not know whether an experience rises to the level of sexual harassment. The robustness of our theoretical findings to realistic alternatives suggests that our empirical decomposition is likely to faithfully capture the effects of #MeToo on adjudicators and on selection into reporting.

6.1 Model Setup

There are two time periods $t \in \{-1, 0\}$, where $t = -1$ corresponds to the time period before #MeToo and $t = 0$ corresponds to the time period after #MeToo. In each period, there is a unit mass of individuals forming set I^t whom we think of as the set of people who could plausibly report sexual harassment. Each individual $i \in I^t$ is active only in time period t and has a type $\theta_i = (c_i, \tau_i) \in \{L, H\} \times \{T, F\}$, where $0 = L < H < \infty$ is the cost of making an accusation, and T and F stand for whether a potential accusation would be "true" or "false," respectively. We assume that in each time period, there exists a positive measure of agents of each type $\theta \in \{L, H\} \times \{T, F\}$.

The timing of the stage game is as follows. Upon observing her type, each individual chooses to either *report* or *not report*. If an individual does not report, then the game ends immediately. If she reports, her case enters the review process. With probabilities p^t_{settle} and p^t_{court}, respectively and independently of all else in the model, a case is either settled or goes to court, which confer expected monetary payments π_S^t and π_C^t, respectively. We assume that the probabilities of settling and going to court are unaffected by the truthfulness of the claim.

For cases that do not settle and do not go to court, an investigation ensues. The investigation can either be "won" by the complainant, in which case she receives a monetary award π_{win}^t, or it can be "lost" by the complainant, in which case no payment is made. To determine whether a case is won or lost, the investigator relies on the level of (testimonial) *evidence* $e_i \in [0,1]$ generated during the investigation in favor of complainant i. As evidence is generated during the investigation, its realization is not observed in prior stages of the game, notably, at the time the individual decides to make a report or not. A higher level of evidence implies the complainant's case is more likely to be true; each complaint generates a level of evidence that is randomly and independently distributed, where the distribution depends on the truthfulness of the complaint. To formalize these notions regarding evidence, let G_T and G_F be distributions of evidence generated for true and false claims, respectively. We assume that G_T and G_F have full support over $[0,1]$ and are continuously differentiable with probability distribution functions g_T and g_F, respectively. We assume that the likelihood ratio $g_T(e)/g_F(e)$ is strictly increasing in e, that is, higher values of evidence are more likely to be uncovered if the complaint is true, i.e., more evidence supports the complaint. Following Kaplow (2011), we assume that there is a (time-varying) *evidentiary threshold* $\underline{e}^t \in [0,1]$ such that a case is "won" in time period t if and only if the complainant generates at least $\underline{e}^t$ level of evidence. We reiterate that the level of evidence generated is a noisy signal of the truthfulness of a complaint; for any $\underline{e}^t \in (0,1)$, there is a positive probability of both "type I error" (false complaints that are won) and "type II error" (truthful complaints that are lost).[11]

Each individual's payoff from reporting is equal to any monetary award she receives minus the cost of reporting, and her payoff from not reporting is normalized to zero.

Let

$$u_i^t := \sum_{\ell \in \{\text{settle, court, win}\}} \mathbb{E}_i^t[\pi_\ell^t | \ell, \tau_i] \cdot Pr_i^t[\ell | \text{report}, \tau_i] - c_i \tag{3}$$

[11] Kaplow's model takes as primitives (potentially heterogeneous) costs on adjudicators for type I vs type II errors. Because of the monotone likelihood ratio assumption on the evidence generating process, any collection of costs induces a threshold rule as being optimal. We do not explicitly model these costs, and instead assume that #MeToo can lower (raise) the evidentiary threshold used, which can occur by either increasing (decreasing) the cost of type II error or decreasing (increasing) the cost of type I error. In what follows, we aim to empirically identify #MeToo's effect on the threshold.

represent individual i's expected payoff from reporting. An individual i in time t files a report if and only if $u_i^t \geqslant 0$. Note that if an individual i files a report at time t, then so do all the other individuals of the same type at time t. Therefore, we abuse notation and refer to the expected utility of reporting of all agents of type θ in time t as u_θ^t.

6.2 Discussion of Modeling Assumptions

Our stylized model is meant to capture the key intuitions and mechanisms necessary to tie back to our empirical setting. Our goal, therefore, is not to maximize generality, but rather readability. In this section, we discuss some modeling assumptions and how they do and do not affect key takeaways.

First, our model considers only two time periods, one before #MeToo and one after. Our empirical specification considers multiple times in each of the pre- and post-#MeToo periods, and our theoretical results would remain similar but be more notationally complex with more periods.

Second, our model assumes four types of potential reporters, as indexed by cost and truthfulness. While we find it reasonable to dichotomize the "truthfulness" of claims, there is no reason to limit the support of costs. What is important for our analysis is to ensure that there are positive measures of both true and false reports in each period. Our assumptions that low-cost types face a cost $L = 0$ and that there exist positive measure sets of low-cost "true" and "false" individuals ensure the desired outcome. Moreover, our assumption that individuals observe their type fully prior to the decision to report implies that accusers are either "telling the truth" or "lying." This is not necessary, and we could have instead complicated our model by assuming individuals receive a (potentially uninformative) signal of the truthfulness of their potential allegation. This alternative would be more realistic if individuals are not sure whether a potential interaction rises to the level of sexual harassment in the eyes of the law when deciding to report. As with the restriction on the number of types, this alternative does not affect our takeaways because our theoretical results take as given the set of reporters pre- and post-#MeToo. That is, our model provides an economic justification for why an individual would or would not report in different time periods, but our results do not rely on the precise details of why individuals choose to report or not.

Third, and relatedly, we make no attempt to model the decision of a wrongdoer to harass.

Plausibly, #MeToo had a deterring effect on wrongdoing. Our model encodes whether a particular individual was or was not harassed into their type via the second component $\tau \in \{T, F\}$. Therefore, deterrence (or any other forces changing the amount of wrongdoing) is represented as a change in the distribution of types between the time periods. Note that we can also allow the precise costs paid by each type to vary across periods without affecting our takeaways.

Finally, our model assumes deterministic payments π^t_{settle}, π^t_{court}, and π^t_{win} in each time period, and that the probabilities of going to court or settling are independent of other features of the model. We make two observations. First, our results are unchanged if each monetary award from settling, court, and winning is a random variable with mean π^t_{settle}, π^t_{court}, and π^t_{win}, respectively. Second, our assumption of independence of settlement or court outcomes means that cases that go to investigation are neither positively nor negatively selected for truthfulness.[12] While this is a limiting assumption, it is common in the literature[13] and our results are not knife-edge—that is, they remain qualitatively valid if there is a sufficiently small (positive or negative) correlation between going to court or settlement and a complaint's truthfulness.

6.3 Results: Selection into Reporting versus Treatment Effect on Adjudicators

Recall that our key aim with the model is to decompose the total effect of #MeToo on the complainant win rate into a selection into reporting effect and a treatment of adjudicators effect. To aid us in doing so, we introduce some notation and terminology.

For each period $t \in \{-1, 0\}$ and each $\tau \in \{T, F\}$, let R^t_τ represent the set of reporters with truthfulness level τ. Because of the assumption of finitely many types and full support, each R^t_τ is (Lebesgue) measurable and has positive (Lebesgue) measure, which we denote by $|\cdot|$, i.e., $|R^t_\tau| > 0$.

[12]As previously discussed, complainants of dismissed cases receive no restitution. As dismissal typically occurs for administrative reasons, and occurs before the investigation stage, we exclude this outcome from our model.

[13]Specifically, it is common in the literature to assume that previous actions in multi-stage assessment systems do not affect the analysis of the analyzed stage. For example, Arnold, Dobbie, and Yang (2018) study the bail decisions of judges and whether they exhibit racial preferences. However, the authors implicitly do not allow their choices to reflect the actions of previous actors who interact with the defendant. As they state on page 1902, a "concern is that bail judges may be influenced by other court actors (e.g., prosecutors) when making decisions, such that racial bias stems from judges not overriding racially biased bail recommendations." Our assumption similarly attributes the effects of changes in the win rate after #MeToo, holding fixed the truthfulness distribution of cases, only to adjudicators.

Therefore,

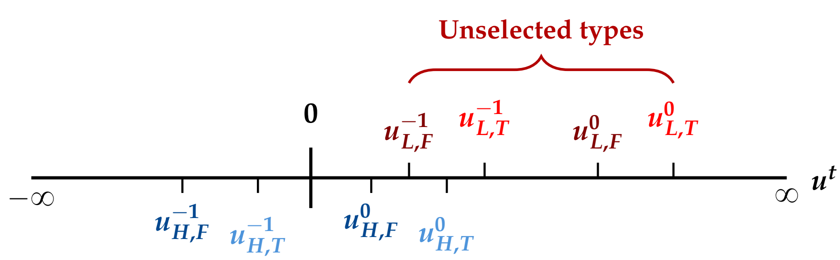
$$\mathcal{T}_t := \frac{|R_T^t|}{|R_T^t| + |R_F^t|} \in (0, 1)$$

fraction of complaints in period t are true, and $R_T^t, R_F^t > 0$ (i.e., that both true and false accusations are made in each period) ensures that $\mathcal{T}_t$ is strictly between 0 and 1.

Let $r^0 \subset R_T^0 \bigcup R_F^0$ be a positive measure set of reporters post-#MeToo, where we represent the subset of true and false reports within set r^0 as r_T^0 and r_F^0, respectively. We call set r^0 *unselected* if

$$\frac{|r_T^0|}{|r_T^0| + |r_F^0|} = \mathcal{T}_{-1}.$$

In words, a set of reporters r^0 in the post-#MeToo period is said to be unselected if the share of them who are truthful is equal to the share of truthful reporters in the pre-#MeToo period. To gain intuition into the properties of such a set, consider an example in which the distribution of types is the same in both periods, i.e., that #MeToo did not affect reporting costs or affect the incidence of sexual harassment. Instead, suppose that #MeToo raised the expected utility from reporting for each type, $u_\theta^0 \geqslant u_\theta^{-1}$ for all types θ. As depicted graphically below, this means that all types who reported pre-#MeToo continue to report after #MeToo (e.g. types (L, F) and (L, T) in dark and light red, respectively), and some types that did not report pre-#MeToo switch to doing so after #MeToo (e.g. types (H, F) and (H, T) in dark and light blue, respectively). The collection of types that report in both periods form an unselected subset of reporters post-#MeToo, while the new types who report can be either positively selected (i.e. more truthful on average) or negatively selected (i.e. less truthful on average).



Departing from this simple example, our definition of an unselected subset of reporters holds regardless of changes in the composition of types, because we place no restriction on the measure of the set r^0. In other words, an unselected subset of reporters always exists. Call an unselected r^0 *always reporters (ARs)* if it is a maximal unselected set, i.e., there does not exist another unselected set of reporters $\tilde{r}^0$ such that $|\tilde{r}^0| > |r^0|$. Given a set r^0 of always reporters, we refer to the remaining reporters post-#MeToo $R^0 \backslash r^0$ as *induced reporters (IRs)*. Because there is a finite measure of reporters in each period, a set of always reporters, and therefore induced reporters, always exists. If there are no compositional changes across periods and the same types report in both periods, all reporters post-#MeToo are always reporters.

The following proposition relates changes in the win rate of different groups to the selection of individuals into reporting and to changes to the adjudicators' evidentiary threshold, and its proof is contained in Appendix A.[14]

Proposition 1.

1. *If the expected win rate increases (decreases) for always reporters after #MeToo, then the evidentiary threshold used by adjudicators is lowered (raised) after #MeToo.*
2. *Induced reporters are more likely to be truthful than are always reporters if and only if induced reporters have higher expected win rates than always reporters after #MeToo.*

Part 1 discusses how the impact of the win rate on always reporters reveals changes to the evidentiary threshold: an increase in win rates for always reporters is equivalent to a lowering of the evidentiary threshold. Similarly, a decrease in win rates for always reporters reveals a raising of the evidentiary threshold. Part 2 discusses how selection into reporting affects outcomes of interest. If induced reporters are positively selected (more likely to be truthful), then we expect win rates to be higher for induced reporters after #MeToo than for always reporters. If induced reporters are negatively selected, then we expect the opposite.

6.4 Empirical Decomposition

β^{CE} identifies the average effect of #MeToo on a complainant's likelihood of winning their sexual harassment complaint. Therefore, β^{CE} comprises two effects: changes in the selection of which types

[14] We note that this result is stated in terms of always reporters and induced reporters. The findings, and arguments behind them, are unchanged if we replace these two groups with any unselected subset of reporters and the complement of that unselected subset, respectively. We state the result in terms of always and induced reporters to better comport with our upcoming decomposition.

of individuals file reports and changes in the evidentiary threshold imposed by adjudicators. In this section, we demonstrate an empirical method for decomposing these two effects, which allows us to make conclusions about selection into reporting and changes to adjudicators' evidentiary threshold as described in Proposition 1.

In order to decompose β^{CE} and interpret the results, we make two assumptions:

A1 Parallel Trends: $E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, ARs] = E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0]$, or that the untreated potential outcomes of sexual harassment always reporters trend the same as the untreated potential outcomes of control group reporters.

A2 Filing Invariance: Treatment effects for any two always reporter cases resolved after #MeToo are the same. This implies that cases filed before #MeToo but resolved after have the same treatment effects as always reporter cases both filed and resolved after #MeToo. In other words, adjudicators do not consider case filing time when selecting an evidentiary threshold for complaints to "win." Moreover, it implies that the evidentiary bar changes precisely one time: at the onset of #MeToo in October 2017. Using the notation of our theoretical model, this assumption states that there is a constant evidentiary threshold $\underline{e}^0$ after October 2017.

These assumptions are similar to standard assumptions in the causal inference literature. Below we describe these parallels and use empirical evidence to partially falsify them.

Assumption A1 is a parallel trends assumption and can therefore be falsified by plotting and examining pre-trends. The absence of pre-trends, which we verify below, is consistent with A1.

Assumption A2 is analogous to the assumption of simultaneous treatment and homogeneous treatment effects in a canonical difference-in-differences model (Card and Krueger, 1993; Angrist and Pischke, 2009). We assume that #MeToo treats all adjudicators once and suddenly (simultaneous treatment) and that treatment effects for always reporters do not grow over time (homogeneous treatment effects). To add credibility to this assumption, we plot the win rate for a subset of always reporters in the first six months after #MeToo and confirm that the win rate remains very stable over time (see Figure A4 and Section 6.4.2 for how we estimate effects on ARs). A2 is plausible if adjudicators do not observe case filing time, or are disallowed from considering it when making their determination. This assumption may be violated if there is seasonality to the kind of case filed, resulting in cases filed before #MeToo but resolved after differing systematically from post-#MeToo cases. We verify our results hold in light of this concern. While cases filed before #MeToo that resolve after are mechanically less likely to be filed in the winter due to the sample restriction of when they must end (see Figure A5), our results are nearly unchanged if we consider only winter cases for our analysis (see Table A3). In Tables A4 and A5, we compare results by all filing seasons and show that the relative magnitudes are broadly similar across seasons.

Therefore, we view these assumptions as plausible in our setting, and we turn to our decomposition result.

Proposition 2. *Let $\omega \in (0,1)$ represent the share of always reporters. Under assumption A1 and the standard parallel trends assumption from Section 4:*

$$ATT_{CE} = \omega ATT_{AR} + (1-\omega) ATT_{IR}, \tag{4}$$

where

- $ATT_{CE} = E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1] - E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1]$
  is the ATT of #MeToo, which is the "combined effect" on all reporters,
- $ATT_{AR} = E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, ARs] - E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, ARs]$
  is the ATT for always reporters, and
- $ATT_{IR} = E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, IRs] - E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, IRs]$
  is the ATT for induced reporters.

We present the proof of Proposition 2 in Appendix A.[15]

Proposition 2 both decomposes ATT_{CE}, and does so into causal effects of #MeToo on different populations. Applying Proposition 2 to answer our economic question of interest requires identifying the terms of Equation 4. We present our identification approach more formally in the following subsections, and quickly describe our approach here. ATT_{CE} is estimated from Equation 1a by $\hat{\beta}^{CE}$ (see Section 4). ω can be bounded using the number of cases reported (see Section 6.4.1). ATT_{AR} is

[15]Note that Proposition 2 considers a classic two-period difference-in-differences setting. However, this is merely for notational convenience, as the two-period ATT is numerically equivalent to a multi-period ATT given the absence of staggered treatment timing in our setting.

identified from our "overlap regression" under assumptions A1 and A2 (see Section 6.4.2). ATT_{IR} is then solved for given the other terms in Equation 4.

Note that we have not formally discussed analogous gender-specific decompositions. However, Equation 4 is valid for decomposing β_M^{CE} and β_W^{CE}, appropriately indexing the ATTs and ω with gender-specific terms.

6.4.1 Estimating the share of always reporters, ω

In this section we calibrate ω, the proportion of always reporters. As we will show in Section 7, induced reporters are positively selected for all values of ω, so the exact ω is not pivotal. Our objective in this section is to provide a framework rather than advocate for a specific value.

Let the reporting rate in the pre-period be $r_{\text{pre}} = \frac{\text{\# sexual harassment complaints reported prior to \#MeToo}}{\text{\# sexual harassment committed prior to \#MeToo}}$ and similarly for r_{post}. Since we do not know the true incidence of sexual harassment, we cannot determine these quantities directly. Instead, we can bound ω and use previous literature estimates to establish a range of values. Throughout the ω calculations, we use counts from the first year pre- and post-treatment to avoid bias from the decrease in cases following the Covid-19 pandemic.

ω **bounding method 1:** We can use prior literature to help estimate ω. Levy and Mattsson (2025) find that #MeToo decreased sexual *assault* incidence by 1.4 percentage points. The incidence of sexual assault is from the Campus Climate Survey, which measures the prevalence of sexual misconduct using self-reported data from students in U.S. universities. Sexual assault and sexual harassment are different, and college students are a selected sample. However, we can use 1.4pp as a proxy for the change in incidence of sexual harassment. Using this method, we get $\omega = 0.793$.

ω **bounding method 2:** We can use the number of control group complaints reported as a proxy for incidence. If the reporting rate for control group complaints is unaffected by #MeToo, then any change in the number of reported control complaints is due to changes in the overall propensity to discriminate: e.g., if control complaints increased by 17% after #MeToo, we can assume that the *incidence* of overall discrimination increased by 17%, and therefore sexual harassment incidence probably increased by a similar amount. Using this method, we get $\omega = 0.860$.

Our calibrated values are therefore $\omega \in \{0.793, 0.860\}$. We reiterate that these are illustrative, and our final conclusions do not depend on the specific value of ω.

6.4.2 Estimating ATT of always reporters

In this section we show how we estimate the ATT on always reporters. To do so, we compare cases resolved before #MeToo to cases filed before but resolved after #MeToo. We call the latter *overlap* cases. Since overlap cases are filed before #MeToo, assuming no anticipation effects, their selection could not have been affected by #MeToo. Our identifying assumption A1 is that overlap sexual harassment cases would have trended in parallel to control cases if not for #MeToo. Note that in overlap analyses, we run the same difference-in-differences specifications as Equations 1a and 1b, but restricting the sample to cases filed before #MeToo. This regression therefore compares overlap and pre-period sexual harassment cases to overlap and pre-period control cases (see Figure 4 for a diagram of the comparison the regression is making). Under A2, this regression allows us to identify the treatment effect for all always reporters, regardless of the timing of complaint filing. To validate that overlap cases are a reasonable proxy for always reporter cases (i.e., overlap cases are unlikely to be systematically different in some way), in Table A6, we show the characteristics of overlap cases and compare them to *placebo* overlap cases in 2016. Actual overlap cases do not differ substantially from overlap cases that would have been counted if #MeToo had happened in 2016.

We estimate Equation 1a on the overlap sample to yield $\hat{\beta}^O$ (for overlap), an estimate of ATT_{AR} under A1 and A2. To identify the gender-specific analogs, we estimate Equation 2a on the overlap sample to get $\hat{\beta}^O_M$ and $\hat{\beta}^O_W$. We estimate Equations 1b and 2b to reveal the dynamic analogs, respectively, of these unbiased estimates.

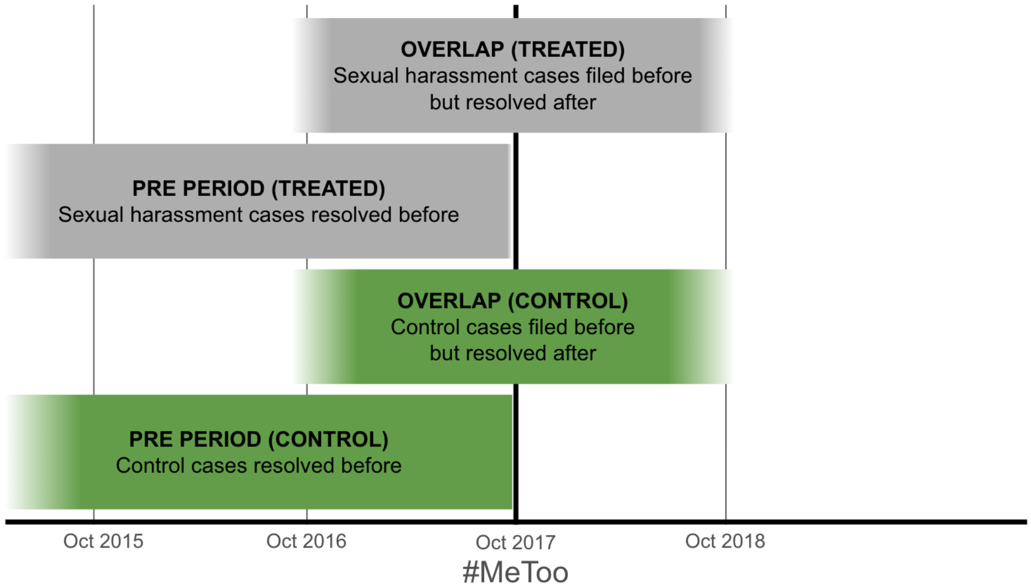


Figure 4: Stylized illustration of the two-by-two comparison that our difference-in-differences regression for overlap cases is making.

The estimates are presented in Table 3, and the dynamic analogs are shown in Figures 5a and 5b. In Table 3, the first two columns present values of $\hat{\beta}^O$, and the third column presents $\hat{\beta}^O_M$ (top row) and $\hat{\beta}^{DDD} = \hat{\beta}^O_W - \hat{\beta}^O_M$ (bottom row). The estimate for overlap cases is $\hat{\beta}^O = 0.087$ (std. err. 0.008), which is positive and significant at the 1% level. There is gender heterogeneity: $\hat{\beta}^O_M = 0.153$ is significantly larger than $\hat{\beta}^O_W = 0.086$. Figures 5a and 5b present a visual lack of pretrends for both genders, and again show a larger increase in the win rate for men than for women. We show results for secondary outcomes, such as rate of settlement and exiting to court, in Table A7.

7 Selection vs treatment: results

Our results suggest that there is both selection into reporting, and changes to adjudicators' evidentiary thresholds. Comparing $\hat{\beta}^O$ (0.087) to $\hat{\beta}^{CE}$ (0.101) suggests that induced reporters are positively selected on win probability, since $\hat{\beta}^O$ is smaller than $\hat{\beta}^{CE}$. In order to relate this difference to the average truthfulness of induced versus always reporters, Proposition 2 reveals that an important factor is the share of always reporters, ω. In Figure 6a, we trace out the treatment effects for induced reporters for different values of ω using Equation 4. The values of ω we calibrated in Section 6.4.1 are labeled ω_1, ω_2, and ω_3. The treatment effects for induced reporters are positive under

FIGURE 5a — IMPACT ON WINNING, OVERLAP

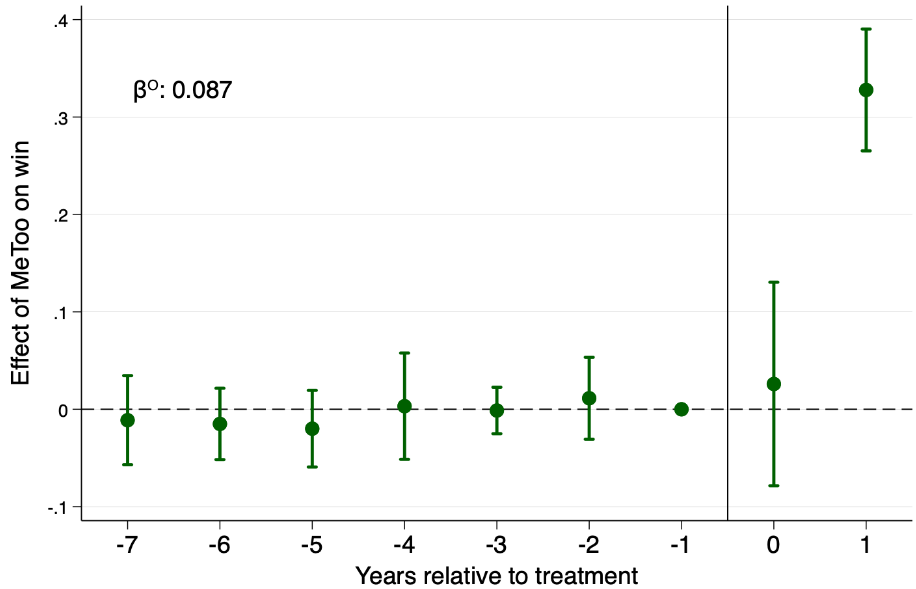


FIGURE 5b — IMPACT ON WINNING BY GENDER, OVERLAP

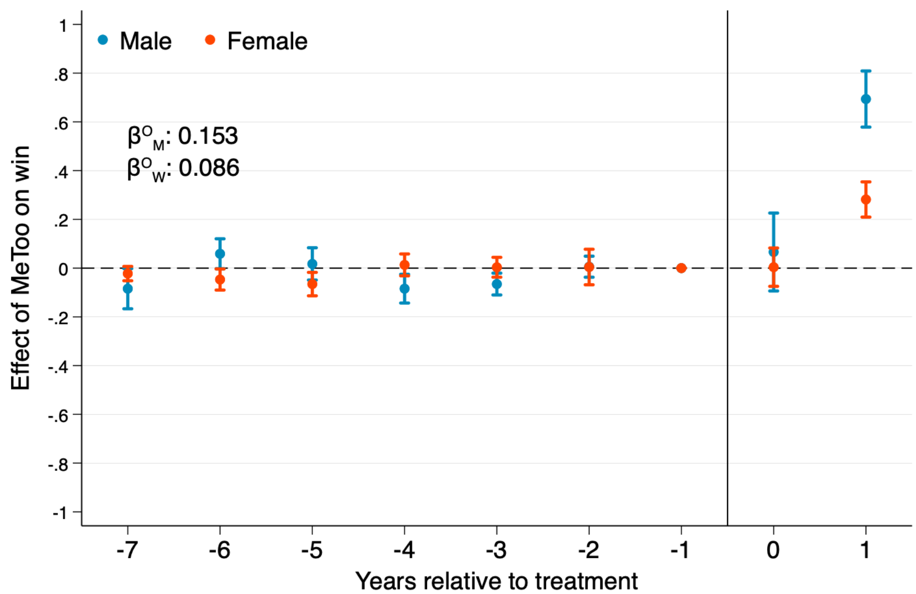


Figure 5: These figures provide estimates of Equations 1b and 2b for the subset of cases that are filed before #MeToo (the overlap sample from Figure 4). The x-axis in all subfigures represents 12-month intervals centered around the #MeToo start date. $t = -1$ is the 12 months preceding October 15, 2017, $t = 0$ is the 12-month period following October 15, 2017, and so on. We control for unit-by-state and year-month-by-state fixed effects, where the "year-month" specifies the date of the complaint resolution.

TABLE 3 — EFFECT OF #METOO ON LIKELIHOOD COMPLAINANT WINS SEXUAL HARASSMENT CASE IN OVERLAP SAMPLE

	All complaints	Complaints with gender	
SH × Post	0.087***	0.102***	0.153***
	(0.008)	(0.008)	(0.008)
SH × Post × Female			-0.067**
			(0.019)
Unit and Time × State FE	✓	✓	✓
Unit and Time × State × Female FE			✓
N	39,321	30,741	30,733
R^2	0.128	0.073	0.081
Control mean	0.152	0.226	0.229

Table 3: SH is sexual harassment. Columns 1 and 2 present estimates of Equation 1a estimated only on complaints filed before #MeToo (see Figure 4). Column 3 presents estimates of Equation 2a estimated only on complaints filed before #MeToo. Fixed effects are basis of discrimination by state and year-month of case resolution by state. For the triple differences specification, the fixed effects are basis of discrimination by state by female complainant and year-month of case resolution by state by female complainant. Standard errors clustered at the basis (unit) level. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

every value of ω. Interpreting these results in light of Proposition 1, we conclude that #MeToo both induced more truth-tellers to report and led adjudicators to lower their evidentiary threshold, leading to higher win rates for all complainants.

Next, we turn to examining heterogeneity by gender in this selection. The results of our overlap specification by gender are shown in Figure 6b. For women, the pattern is the same as the aggregate effects: $\hat{\beta}^O_W$ is $0.153 - 0.067 = 0.086$, smaller than $\hat{\beta}^{CE}_W$ (0.123). Figure 6b shows that treatment effects for female induced reporters are positive for all possible values of ω and, importantly, exceed the treatment effects for female always reporters for all values of ω (orange lines). For men, $\hat{\beta}^O_M$ (0.153) is substantially larger than $\hat{\beta}^{CE}_M$ (0.119), meaning that the treatment effect for male induced reporters is always lower than that for male always reporters (blue lines of Figure 6b). For large values of ω, the treatment effect for male induced reporters is actually *negative*. This means that for our preferred calibrations, the negative selection effect for men is so large that it dominates the positive adjudicator treatment effect, $\hat{\beta}^O_M$. Male induced reporters are actually less likely to win their case as a result of #MeToo than male reporters were prior to #MeToo.

Combining these gender heterogeneity results with Proposition 1 allows for the following

FIGURE 6a — ATT_{AR} AND ATT_{IR} BY SHARE OF ALWAYS REPORTERS

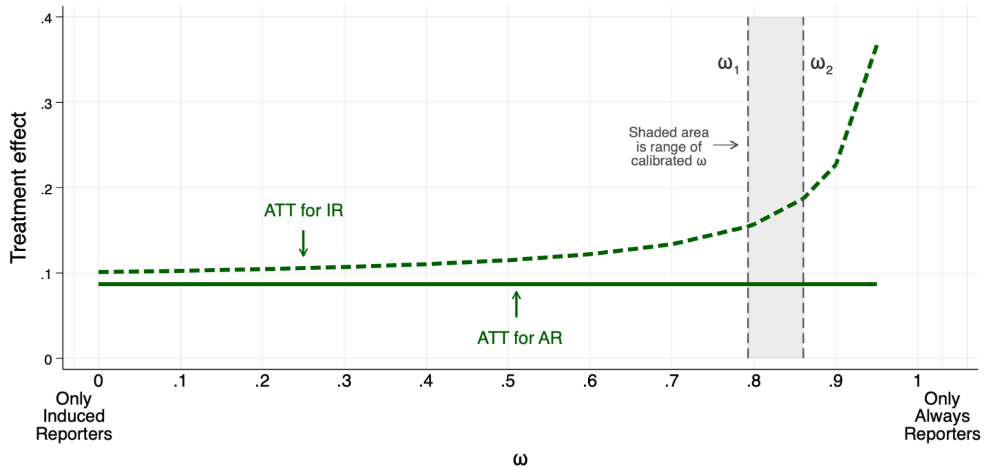


FIGURE 6b — ATT_{AR} AND ATT_{IR} BY GENDER

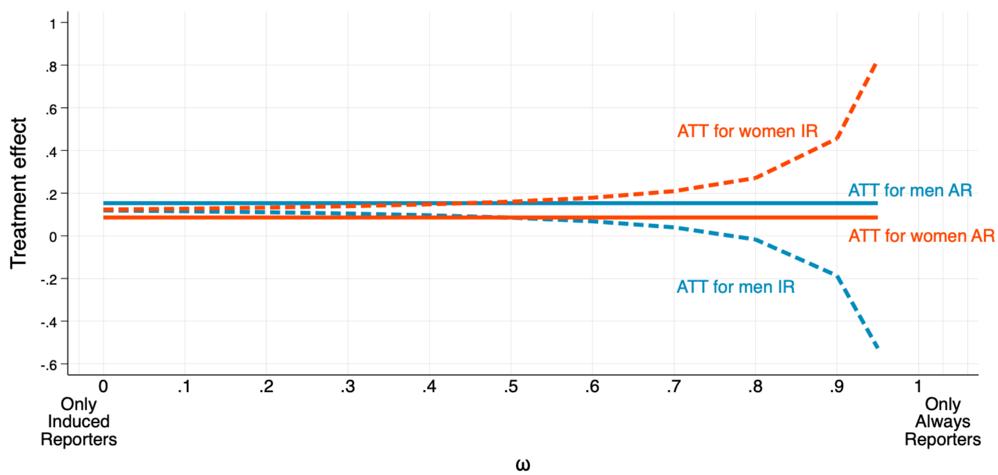


Figure 6: In the top panel, we indicate ω_1 and ω_2 from Section 6.4.1 with vertical lines. IR refers to induced reporters and AR refers to always reporters. The treatment effect for AR is constant across all values of ω. The treatment effect for IR asymptotes at $\omega = 1$, so the graphs are bounded at $\omega = 0.95$.

conclusions: #MeToo induced adjudicators to lower their evidentiary thresholds for both men and women, thus "believing" more complaints. #MeToo-induced female complainants were more likely to file a true complaint than were female always reporters, but #MeToo-induced male complainants were less likely to file a true complaint than were male always reporters.

In Appendix C we investigate the robustness of our results to alternative samples and alternative control groups. Broadly speaking, our main empirical takeaways are qualitatively and quantitatively similar across these alternatives.

8 Conclusion

In this paper, we study the #MeToo movement, which aimed to increase the reporting of sexual harassment, and evaluate a common criticism that the movement encouraged unsubstantiated accusations. First, we quantify the total impact of the #MeToo movement on sexual harassment complaints. Our baseline difference-in-differences estimates find that #MeToo benefited complainants by increasing the probability that they win their case, and that it did so equally for male and female complainants. However, this effect nests both changes in composition into reporting and the treatment effect of #MeToo on how much adjudicators "believe" complainants.

Second, to disentangle treatment and selection effects, we present a novel decomposition of the difference-in-differences estimate into an effect on always reporters (reporters whose composition of case fundamentals match those of reporters before #MeToo) and an effect on induced reporters (those encouraged to report by #MeToo who may be positively or negatively selected for truthfulness). Intuitively, these two groups reveal, respectively, the treatment effect and the combination of the treatment and selection effects. We estimate the effect on always reporters using "overlap" complaints filed before #MeToo but resolved after. We find that the probability of winning for induced reporters is higher than always reporters for women, but that the reverse is true for men.

We interpret our empirical results through a simple model, which suggests that #MeToo had a dual effect. First, adjudicators were more likely to "believe" complaints after #MeToo, holding the characteristics of the complaint fixed, and this direct effect was larger for men than for women. More formally, we find evidence that adjudicators lowered their evidentiary threshold for findings of probable cause that sexual harassment occurred. Second, women induced to report by #MeToo were more likely to file a true complaint than women reported before. On the other hand, men induced to report by #MeToo were less likely to file a true complaint than men who reported before.

These findings contribute to our broader understanding of social campaigns and movements that increase the reporting of wrongdoing, highlighting their dual effect on how adjudicators weigh evidence and on the composition of people who choose to report as a result.

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Appendix

A Proofs

Proof of Proposition 1

Proof of Part 1. Let r^0 be a set of always reporters. Recall that the win rate is defined only for cases that go through an investigation, and that court and settlement outcomes are independent of an individual's truthfulness level. Therefore, the win rate among always reporters in each period t is given by:

$$W^t := \frac{|r_T^0| \cdot Pr(e \geqslant \underline{e}^t | \tau = T) + |r_F^0| \cdot Pr(e \geqslant \underline{e}^t | \tau = F)}{|r_T^0| + |r_F^0|} = \frac{|R_T^{-1}| \cdot Pr(e \geqslant \underline{e}^t | \tau = T) + |R_F^{-1}| \cdot Pr(e \geqslant \underline{e}^t | \tau = F)}{|R_T^{-1}| + |R_F^{-1}|}$$

where the equality follows from the definition of always reporters. By the properties of probability distributions, $Pr(e \geqslant \underline{e}^0 | \tau = T) \geqslant Pr(e \geqslant \underline{e}^{-1} | \tau = T)$ and $Pr(e \geqslant \underline{e}^0 | \tau = F) \geqslant Pr(e \geqslant \underline{e}^{-1} | \tau = F)$ if and only if $\underline{e}^0 \leqslant \underline{e}^{-1}$. Therefore, $W^0 \geqslant W^{-1}$ if and only if $\underline{e}^0 \leqslant \underline{e}^{-1}$, as desired. □

Proof of Part 2. This follows straightforwardly using the machinery from the proof of part 1, because the same evidentiary threshold $\underline{e}^0$ is used for all cases (i.e., all reporter types) post-#MeToo, and because we assume that G_T dominates G_F in the likelihood ratio order, which implies that $Pr(e \geqslant \underline{e}^0 | \tau = T) \geqslant Pr(e \geqslant \underline{e}^0 | \tau = F)$ for any given $\underline{e}^0 \in [0, 1]$. □

Proof of Proposition 2

Proof. Before proving Proposition 2, we provide a useful lemma.

Lemma 1. *Let $\omega \in (0, 1)$ represent the share of always reporters. Under assumptions A1 and the standard parallel trends assumption from Section 4: $E[Y_{i0}(0) - Y_{i-1}(0) | D_{i0} = 0, ARs] = E[Y_{i0}(0) - Y_{i-1}(0) | D_{i0} = 1, IRs]$, that is, the untreated potential outcomes of the ARs and IRs are equal.*

Proof. First, note that the *standard parallel trends assumption* is that

$$E[Y_{i0}(0) - Y_{i-1}(0) | D_{i0} = 0] = E[Y_{i0}(0) - Y_{i-1}(0) | D_{i0} = 1]. \tag{5}$$

Also, Assumption A1 is that

$$E[Y_{i0}(0) - Y_{i-1}(0) | D_{i0} = 0] = E[Y_{i0}(0) - Y_{i-1}(0) | D_{i0} = 1, ARs]. \tag{6}$$

Because each reporter in the post period is either in the always reporter group or in the induced reporter group, and ω is the share of ARs, we can write the following identity:

$$E[Y_{i0}(0) - Y_{i-1}(0) \mid D_{i0} = 1] = \omega\, E[Y_{i0}(0) - Y_{i-1}(0) \mid D_{i0} = 1, \text{ARs}] \\ + (1-\omega)\, E[Y_{i0}(0) - Y_{i-1}(0) \mid D_{i0} = 1, \text{IRs}]\,. \tag{7}$$

Then it holds that:

$$\begin{aligned} E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0] &= E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1] \\ &= \omega E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, ARs] + (1-\omega)E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, IRs] \\ &= \omega E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0] + (1-\omega)E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, IRs] \\ &= E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, IRs], \end{aligned} \tag{8}$$

where the first equality follows from Equation 5, the second equality follows from Equation 7, the third equality follows from Equation 6, and the final equality comes from solving $E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0] = \omega E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0] + (1-\omega)E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, IRs]$.

Then combining Equations 6 and 8 yields the desired conclusion.

□

We now return to the proof of the proposition. Define

$$\begin{aligned} A &:= E\left[Y_{i0}(1) - Y_{i-1}(0) \mid D_{i0} = 1,\ AR\right], \\ B &:= E\left[Y_{i0}(1) - Y_{i-1}(0) \mid D_{i0} = 1,\ IR\right], \\ C &:= E\left[Y_{i0}(0) - Y_{i-1}(0) \mid D_{i0} = 1,\ AR\right], \text{and} \\ D &:= E\left[Y_{i0}(0) - Y_{i-1}(0) \mid D_{i0} = 1,\ IR\right]. \end{aligned}$$

Under the *standard parallel trends assumption* made in Section 4:

$$ATT_{CE} = E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1] - E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0].$$

We split the first term, $E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1]$, into the ω-weighted average of the change in potential treated outcomes of always and induced reporters, yielding

$$ATT_{CE} = \omega E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, AR] + (1-\omega)\overbrace{E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, IR]}^{B} \\ - E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0].$$

Adding and subtracting $E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, AR]$ yields

$$\begin{aligned} ATT_{CE} = \omega E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, AR] + (1-\omega)B - E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0] \\ + E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, AR] - E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, AR]. \end{aligned}$$

By assumption A1, $E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0] = E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, AR]$ for any always reporter i. This allows us to cancel terms from the previous expression, which implies

$$\begin{aligned} ATT_{CE} &= \omega E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, AR] + (1-\omega)B - E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, AR] \\ &= \omega E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, AR] + (1-\omega)B - (\omega + (1-\omega))E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, AR] \\ &= \omega\left(\overbrace{E[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, AR]}^{A} - \overbrace{E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, AR]}^{C}\right) \\ &\quad + (1-\omega)B - (1-\omega)\underbrace{E[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, AR]}_{C} \\ &= \omega(A - C) + (1-\omega)(B - C) \\ &= \omega(A - C) + (1-\omega)(B - D), \end{aligned}$$

where the final equality follows by Lemma 1, which establishes $C = D$. Therefore, recalling that $ATT_{AR} := A - C$ and $ATT_{IR} := B - D$ establishes the desired claim.

□

B Additional Tables and Figures

TABLE A1 — DATA SUMMARY BY STATE

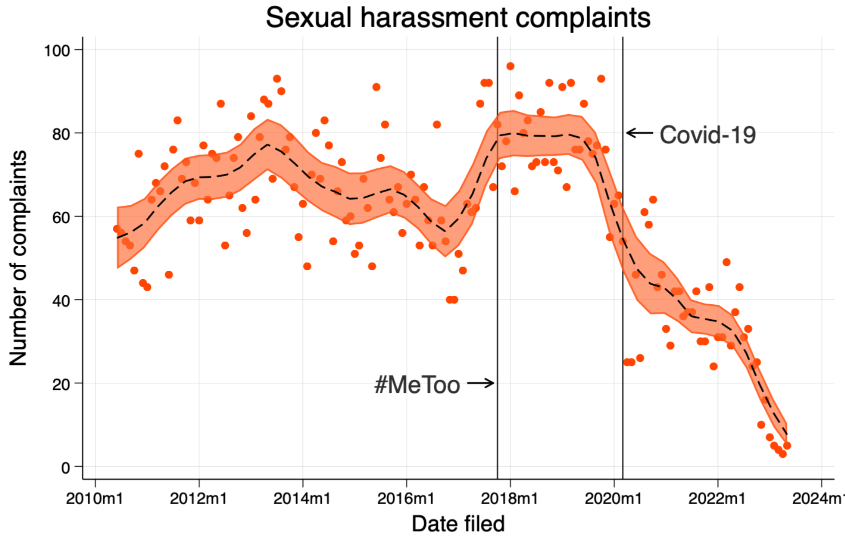
State	N	Sexual harassment	Complainant won	$ paid to complainant	Female complainant
CA	90,220	.247	.000	.	.
NY	48,183	.144	.270	.	.491
MA	17,540	.073	.116	.	.522
WI	16,135	.064	.	.	.
MI	7,350	.043	.0004	$9,155	.
FL	5,110	.005	.050	.	.
AK	2,028	.133	.067	$10,465	.
KY	1,862	.028	.007	$15,400	.
ND	1,801	.165	.024	$1,656	.
HI	1,756	.100	.002	$11,767	.
N	191,985	191,978	74,029	1,060	64,499

Table A1: This table presents means of variables by state. Dots indicate missing data that states either do not record or do not disclose via FOIA. Sexual harassment is the share of sexual harassment complaints out of all discrimination cases filed. "Complainant won" is the win rate across all cases in that state. The win rate in California is 0% because California mostly adjudicates complainants in formal court procedures (only 3.4% of complainants ever win at the investigation stage, and this number is 0% after meeting our sample restrictions). We used a Python API to determine the gender of the complainant based on their name. States are colored by voting history in the 2012, 2016, 2020, and 2024 presidential elections, with red (blue) states indicating that Republicans (Democrats) won that state in the majority of the last four elections, and purple indicating each party won two elections, as taken from https://en.wikipedia.org/wiki/Red_states_and_blue_states.

Table 1. Detailed Survey Items and Categorization	
Verbal sexual harassment	• Someone whistling, honking, making kissy noises, "Pssst" sounds, or leering/staring aggressively at you. • Someone saying things like, "Hey Baby," "Mmmm Sexy," "Yo Shorty," "Mami/Mamacita," "Give me a smile," or similar comments in a way that is disrespectful and/or unwanted and/or made you feel unsafe. • Someone calling you a sexist slur, like "Bitch," "Slut," "Cunt," "Ho," or "Thot." • Someone misgendering you or calling you a homophobic or transphobic slur, like "Fag," "Dyke," or "Tranny." • Someone talking about your body parts inappropriately or offensively (such as your legs, crotch, butt, or breasts), saying sexually explicit comments ("I want to do BLANK to you") or asking inappropriate sexual questions. • Someone making threats to harm you, to harm someone you know, or to share personal information you don't want shared (examples could include your sexual orientation or drug use history or immigration status) • Someone saying you must date them or do a sexual act for them in exchange for something (such as a good grade, a promotion, a job, drugs, food, or something similar) or instead of something (like paying rent or a citation, etc.). • Someone repeatedly asking you for a date or your phone number when you've said no or ignored them.
Cyber sexual harassment	• Someone repeatedly texting or calling you in a harassing way. • Someone electronically sending you or showing you sexual content without your permission, such as over email, Snapchat, or Facebook or on their phone or computer. • Someone taking and/or sharing sexual pictures or videos of you without your permission.
Physically aggressive sexual harassment	• Someone flashing or exposing their genitals to you without your permission. • Someone physically following you without your permission. • Someone purposely touching you or brushing up against you in an unwelcome, sexual way.
Sexual assault	• Someone forcing you to do a sexual act without your permission or one that you don't want to do (including while you are under the influence of alcohol or drugs).

Figure A1: Definitions of different types of sexual harassment

FIGURE A2 — NUMBER OF COMPLAINTS FILED OVER TIME



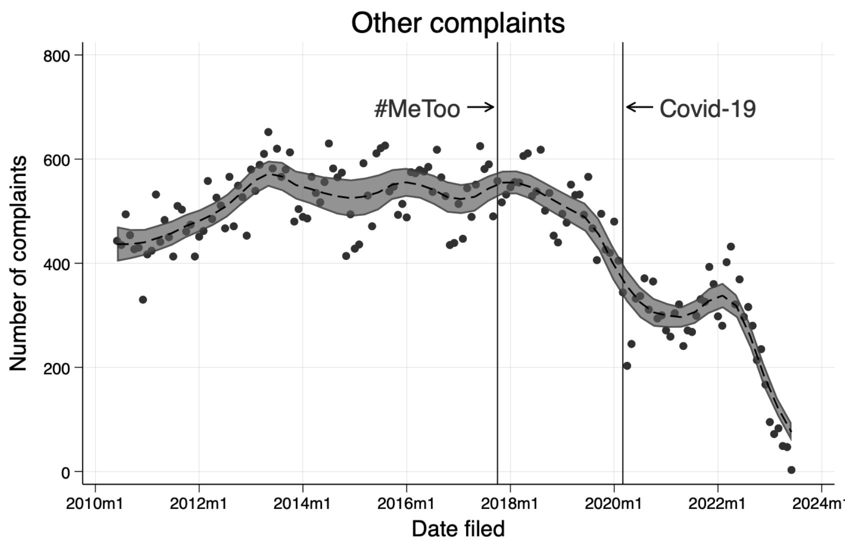


Figure A2: The figure shows the number of employment discrimination complaints filed over time, excluding California, Florida, and Wisconsin. These states are omitted because data for them begin only in early 2017, which would otherwise create an artificial increase in filings following the treatment period.

FIGURE A3a — IMPACT ON SETTLEMENT

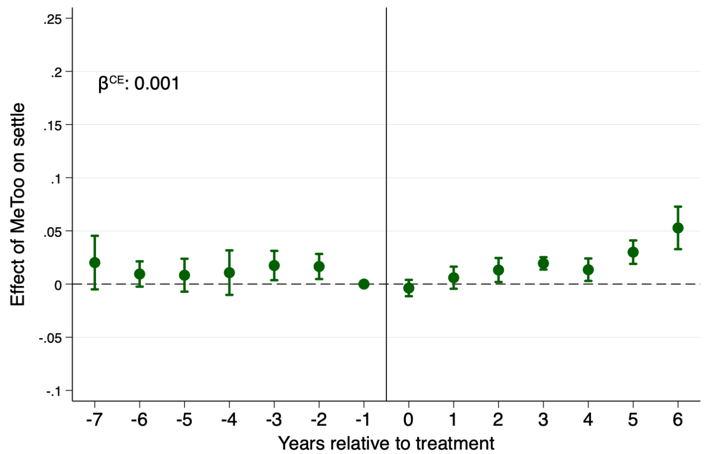


FIGURE A3b — IMPACT ON EXITING TO COURT

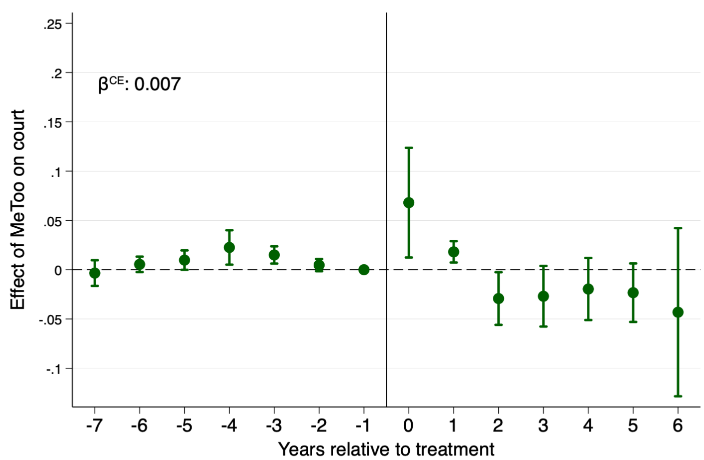


Figure A3: These figures provide estimates of Equation 1b. The x-axis in all subfigures represents 12-month intervals centered around the #MeToo start date. $t = -1$ is the 12 months preceding October 15, 2017, $t = 0$ is the 12-month period following October 15, 2017, and so on. We control for unit-by-state and year-month-by-state fixed effects, where the "year-month" specifies the date of the complaint resolution.

TABLE A2 — EFFECTS OF #METOO ON ALL COMPLAINT OUTCOMES

	Settled			Court		
	All complaints	**Complaints with gender**		**All complaints**	**Complaints with gender**	
SH × Post	0.001	-0.001	0.001	0.007*	0.009***	0.002
	(0.004)	(0.003)	(0.005)	(0.003)	(0.002)	(0.003)
SH × Post × Female			-0.003			0.007
			(0.005)			(0.005)
Unit and Time × State FE	✓	✓	✓	✓	✓	✓
Unit and Time × State × Female FE			✓			✓
N	175,495	64,499	64,499	175,801	64,499	64,499
R^2	0.099	0.103	0.110	0.631	0.140	0.150
Control mean	0.071	0.094	0.094	0.388	0.048	0.048

Table A2: Settled is whether a case was settled. Court is whether a case was taken to court. Time fixed effects are year-month of case resolution. Standard errors clustered at the case type level. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

TABLE A3 — COMPLAINANT WIN RATE AND CASE DURATION BY SEASON (OVERLAP CASES)

Season	Win Rate (Mean)	Duration (Mean)
Spring	0.1008	354.98
Summer	0.1633	297.07
Fall	0.1252	303.85
Winter	0.0574	443.52
Total	0.1270	324.52

TABLE A4 — MAIN OVERLAP RESULTS BY SEASON OF CASE FILING

	All complaints	Complaints with gender	
Panel A: Winter			
SH × Post	0.054*	0.069*	0.230
	(0.025)	(0.032)	(0.190)
SH × Post × Female			-0.179
			(0.227)
Unit and Time × State FE	✓	✓	✓
N	8,365	6,567	6,543
R^2	0.175	0.126	0.149
Control mean	0.152	0.226	0.229
Panel B: Spring			
SH × Post	0.123***	0.141***	0.275***
	(0.028)	(0.032)	(0.017)
SH × Post × Female			-0.223***
			(0.041)
Unit and Time × State FE	✓	✓	✓
N	9,807	7,644	7,626
R^2	0.169	0.116	0.134
Control mean	0.152	0.226	0.229
Panel C: Summer			
SH × Post	0.052***	0.060***	0.001
	(0.008)	(0.009)	(0.006)
SH × Post × Female			0.106**
			(0.031)
Unit and Time × State FE	✓	✓	✓
N	11,232	8,948	8,933
R^2	0.171	0.118	0.140
Control mean	0.152	0.226	0.229
Panel D: Fall			
SH × Post	0.133***	0.136***	0.275***
	(0.024)	(0.025)	(0.016)
SH × Post × Female			-0.185***
			(0.026)
Unit and Time × State FE	✓	✓	✓
Unit and Time × State × Female FE			✓
N	9,566	7,550	7,526
R^2	0.188	0.139	0.160
Control mean	0.152	0.226	0.229

TABLE A5 — COMPARISON OF $\hat{\beta}^{CE}$ AND $\hat{\beta}^{O}$ BY FILING SEASON FOR COMPLAINTS WITH GENDER

Season Filed	$\hat{\beta}^{CE}$	$\lessgtr$	$\hat{\beta}^{O}$
All			
TWFE	**0.116**	$>$	**0.102**
Triple Diff (Men)	0.119	$<$	0.153
Triple Diff (Women)	0.123	$>$	0.086
Winter			
TWFE	0.120	$>$	0.069
Triple Diff (Men)	0.083	$<$	0.230
Triple Diff (Women)	0.132	$>$	0.051
Spring			
TWFE	0.158	$>$	0.141
Triple Diff (Men)	0.199	$<$	0.275
Triple Diff (Women)	0.129	$>$	0.052
Summer			
TWFE	0.108	$>$	0.060
Triple Diff (Men)	0.048	$>$	0.001
Triple Diff (Women)	0.164	$>$	0.107
Fall			
TWFE	0.046	$<$	0.136
Triple Diff (Men)	0.101	$<$	0.275
Triple Diff (Women)	0.027	$<$	0.090

Table A5: Each cell represents the coefficient from estimating Equation 1a and 2a on the full sample (Col 1) and overlap sample (Col 2) while restricting to cases filed in each season.

TABLE A6 — COMPARISON OF ACTUAL OVERLAP CASES (2017) VERSUS PLACEBO OVERLAP CASES (2016)

Variable	(1) 2016	(2) 2017	(3) Diff	(4) p-value
Complainant is female	0.50	0.50	-0.00	0.90
Sexual harassment	0.11	0.08	-0.02	0.00***
Duration (days)	271.91	259.69	-12.22	0.00***
Observations	3,832	6,230	10,062	

Figure A4 — Win rate of overlap cases resolved after #MeToo

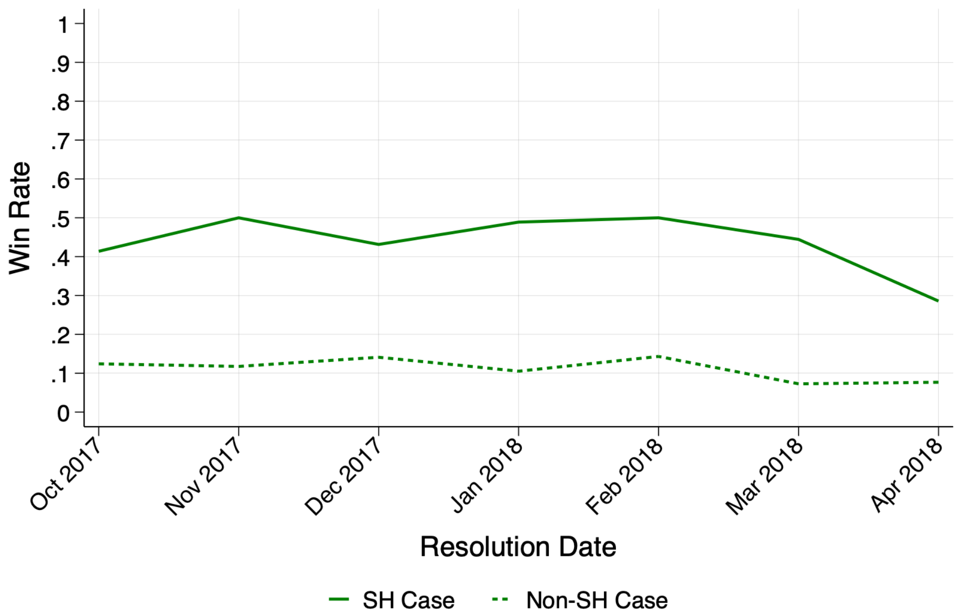


FIGURE A5 — PERCENT OF OVERLAP CASES FILED IN EACH CALENDAR MONTH

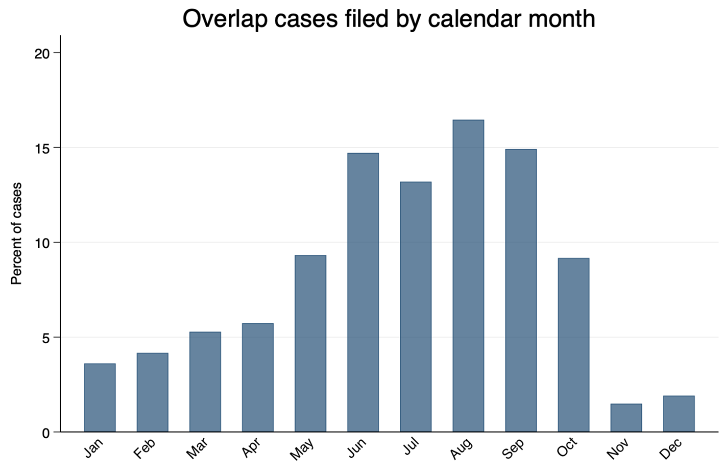


TABLE A7 — EFFECTS OF #METOO ON ALL COMPLAINT OUTCOMES, OVERLAP CASES

	Settled			Court		
	All complaints	**Complaints with gender**		**All complaints**	**Complaints with gender**	
SH × Post	-0.022**	-0.021***	-0.023*	-0.013*	-0.012	-0.019***
	(0.006)	(0.005)	(0.011)	(0.006)	(0.006)	(0.004)
SH × Post × Female			0.006			0.007
			(0.013)			(0.005)
Unit and Time × State FE	✓	✓	✓	✓	✓	✓
Unit and Time × State × Female FE			✓			✓
N	59,471	41,628	41,627	59,488	41,628	41,627
R^2	0.109	0.108	0.115	0.656	0.129	0.138
Control mean	0.071	0.094	0.094	0.388	0.048	0.048

Table A7: Settled is whether a case was settled. Court is whether a case was taken to court. Time fixed effects are year-month of case resolution. Standard errors clustered at the case type level. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

C Robustness

In this section, we present a variety of robustness checks to the main results presented in Section 5. Our goal is to investigate our main empirical findings in alternative samples and under alternative definitions of control complaints. For each robustness check we consider, we investigate two sets of findings. First, whether both $\hat{\beta}^O_M$ and $\hat{\beta}^O_W$ are positive, which implies by Propositions 1 and 2 that the evidentiary threshold adjudicators used when evaluating complaints lowered for sexual harassment cases following #MeToo for both genders. Second, whether male or female induced reporters were positively or negatively selected on the dimension of truthfulness compared to always reporters, which we investigate by comparing the ordinal rankings of the combined effect coefficients ($\hat{\beta}^{CE}_M$ and $\hat{\beta}^{CE}_W$, respectively) to the overlap coefficients ($\hat{\beta}^O_M$ and $\hat{\beta}^O_W$, respectively) (again, see Propositions 1 and 2).

As a summary of our findings, we first present Table A8, which provides a comparison of the coefficients for each of our alternative samples and definitions of control complaints. Broadly, these results are similar to those presented in the main analysis. We describe each of these robustness checks below.

In the first robustness check we consider, we restrict the sample to complaints filed under only one basis of discrimination. This removes ambiguity from multi-basis cases, which may involve overlapping types of discrimination.[16] The estimated effect on the probability of winning remains positive and significant for both combined and overlap coefficients. $\hat{\beta}^{CE}_M$ is smaller than $\hat{\beta}^O_M$, and $\hat{\beta}^{CE}_W$ is larger than $\hat{\beta}^O_W$, both of which directionally align with our main results. This implies that male reporters are negatively selected and female reporters are positively selected, even for cases filed under one basis of discrimination, which is consistent with our main results. The full results for this robustness check are presented in Tables A9 and A10.

In the second robustness check we consider, we exclude complaints alleging retaliation from the analysis. In our main specification, we retain retaliation complaints as part of the control group, but retaliation cases may blur the distinction between treatment and control if they are responses to earlier sexual harassment. Excluding these complaints yields nearly identical results to our main specification for both men and women. Both $\hat{\beta}^O_M$ and $\hat{\beta}^O_W$ remain positive and significant. The direction of selection is also consistent with our main results for both men and women. The combined effect for men $\hat{\beta}^{CE}_M$ (0.113) is smaller than $\hat{\beta}^O_M$ (0.153), and the combined effect for women $\hat{\beta}^{CE}_W$ (0.119) is larger than $\hat{\beta}^O_W$ (0.082), both of which are directionally consistent with our main results. The estimates for this robustness check are presented in Tables A9 and A10.

In the third robustness check we consider, we restrict our sample to complaints filed by the start of the Covid-19 pandemic in March 2020. This also addresses concerns that post-2020 complaints are

[16] It is difficult to determine using our data how single- versus multi-tagged complaints differ on unobservables. On observables, complainants of single-tagged cases are as likely to win their case as their multi-tagged counterparts (15.6% versus 15.9%), but single-tagged cases last longer (241 days versus 105 days), are more likely to be dismissed or settled, and are less likely to exit to court than multi-tagged cases.

systematically different as a result of pandemic-related changes in workplace dynamics or agency operations. $\hat{\beta}^O$ remains positive and significant (0.087, std. err. = 0.008) when considering this more restrictive set of complaints, indicating that adjudicators lowered their evidentiary threshold after #MeToo, consistent with our main findings. $\hat{\beta}^O_M$ is larger than $\hat{\beta}^{CE}_M$ for men who filed before Covid-19 ($0.153 > 0.057$), which also directionally aligns with the main results in indicating that men are negatively selected. Similarly, we find that $\hat{\beta}^O_W$ is *also* larger than $\hat{\beta}^{CE}_W$ for women who filed before Covid-19 ($0.086 > 0.074$), which suggests that women 'early filers' are negatively selected. Since our main results show that women's complaints are stronger after #MeToo, this suggests post-Covid-19 women's cases are potentially driving the positive selection. The estimates for this robustness check are presented in Tables A9 and A10.

In the final robustness check we consider, Table A11 presents our main results using a broader range of discrimination complaints filed across housing, public accommodations, education, and employment sectors. Both $\hat{\beta}^{CE}$ and $\hat{\beta}^O$ remain positive and significant (0.130, std. err. = 0.007 and 0.176, std. err. = 0.019), indicating that adjudicators lowered their evidentiary threshold after #MeToo even when pooling cases across multiple areas of public life. Similarly, the direction of selection for both genders is consistent with our main results. $\hat{\beta}^O_M$ is larger than $\hat{\beta}^{CE}_M$ ($0.207 > 0.203$), while $\hat{\beta}^{CE}_W$ is larger than $\hat{\beta}^O_W$ ($0.249 > 0.204$), indicating men are negatively selected while women are positively selected among all discrimination complaints in our sample. These findings are presented in Tables A11 and A12.

TABLE A8 — COMPARISON OF $\hat{\beta}^{CE}$ AND $\hat{\beta}^{O}$ ACROSS ROBUSTNESS CHECKS FOR COMPLAINTS WITH GENDER

Main results	$\hat{\beta}^{CE}$	$\lesseqgtr$	$\hat{\beta}^{O}$
Triple Diff (Men)	0.119	$<$	0.153
Triple Diff (Women)	0.123	$>$	0.086
Robustness Check	$\hat{\beta}^{CE}$	$\lesseqgtr$	$\hat{\beta}^{O}$
Single-tagged			
Triple Diff (Men)	0.084	$<$	0.144
Triple Diff (Women)	0.085	$>$	0.067
No retaliation			
Triple Diff (Men)	0.113	$<$	0.153
Triple Diff (Women)	0.119	$>$	0.082
Pre-Covid			
Triple Diff (Men)	0.057	$<$	0.153
Triple Diff (Women)	0.074	$<$	0.086
All jurisdictions			
Triple Diff (Men)	0.203	$<$	0.207
Triple Diff (Women)	0.249	$>$	0.204

Table A8: Each cell represents the coefficient from estimating Equation 1a on the full sample (Col 1) and overlap sample (Col 2) while restricting to cases filed on only one basis (Single-tagged), excluding retaliation-based cases in the control group (No retaliation), only incorporating cases filed before the start of the Covid-19 pandemic (Pre-Covid), or including all jurisdictions of discrimination.

Table A9 — Robustness of $\hat{\beta}^{CE}$

	Single-tagged			No retaliation			Pre-Covid		
	All complaints	**Complaints with gender**		**All complaints**	**Complaints with gender**		**All complaints**	**Complaints with gender**	
SH × Post	0.072***	0.077***	0.084**	0.100***	0.112***	0.113***	0.053***	0.062***	0.057***
	(0.008)	(0.011)	(0.029)	(0.016)	(0.018)	(0.027)	(0.011)	(0.013)	(0.015)
SH × Post × Female			0.001			0.006			0.017
			(0.033)			(0.019)			(0.011)
Unit and Time × State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	37,714	26,466	26,462	66,727	42,339	42,334	57,824	40,616	40,611
R^2	0.168	0.109	0.121	0.177	0.092	0.100	0.144	0.074	0.081
Control mean	0.153	0.209	0.210	0.156	0.231	0.234	0.165	0.226	0.228

Table A9: This table is equivalent to Table 2, but the estimation sample is limited to cases filed on only one basis, excludes retaliation-based cases in the control group, or includes only cases filed before the start of the Covid-19 pandemic (March 2020), respectively.

Table A10 — Robustness of $\hat{\beta}^O$

	Single-tagged			No retaliation			Pre-Covid		
	All complaints	**Complaints with gender**		**All complaints**	**Complaints with gender**		**All complaints**	**Complaints with gender**	
SH × Post	0.073***	0.082***	0.144***	0.088***	0.101***	0.153***	0.087***	0.102***	0.153***
	(0.012)	(0.014)	(0.006)	(0.007)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)
SH × Post × Female			-0.077**			-0.071**			-0.067**
			(0.028)			(0.021)			(0.019)
Unit and Time × State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	23,374	18,387	18,379	35,587	27,658	27,650	39,321	30,741	30,733
R^2	0.152	0.104	0.115	0.134	0.077	0.084	0.128	0.073	0.081
Control mean	0.153	0.209	0.210	0.156	0.231	0.234	0.165	0.226	0.228

Table A10: This table is equivalent to Table 3, but the estimation sample is limited to cases filed on only one basis, excludes retaliation-based cases in the control group, or includes only cases filed before the start of the Covid-19 pandemic (March 2020), respectively.

Table A11 — $\hat{\beta}^{CE}$ pooling housing, public accommodations, education, and employment complaints

	All complaints	**Complaints with gender**	
SH × Post	0.130***	0.236***	0.203***
	(0.007)	(0.019)	(0.021)
SH × Post × Female			0.046***
			(0.011)
Unit and Time × State FE	✓	✓	✓
Unit and Time × State × Female FE			✓
N	142,198	84,470	84,384
R^2	0.149	0.091	0.099
Control mean	0.139	0.205	0.207

Table A11: This table is equivalent to Table 2, but we change the sample to include discrimination complaints in employment, housing, public accommodation, and education.

Table A12 — $\hat{\beta}^O$ pooling housing, public accommodations, education, and employment complaints

	All complaints	Complaints with gender	
SH × Post	0.176***	0.206***	0.207***
	(0.019)	(0.019)	(0.015)
SH × Post × Female			-0.003
			(0.011)
Unit and Time × State FE	✓	✓	✓
Unit and Time × State × Female FE			✓
N	76,491	54,787	54,730
R^2	0.131	0.074	0.081
Control mean	0.139	0.205	0.207

Table A12: This table is equivalent to Table 3, but we change the sample to include discrimination complaints in employment, housing, public accommodation, and education.

D Data Appendix

D.1 FOIA process

To obtain data on civil complaints of sexual harassment, we filed Freedom of Information Act requests with all US states that have state agencies responsible for handling discrimination complaints and with the federal Equal Employment Opportunity Commission (EEOC). Two states (Arkansas and Mississippi) did not have such a state agency. In its broadest form, our request included information on all closed employment discrimination, public accommodation discrimination, housing discrimination, and education discrimination cases that were filed between June 1, 2010, and June 1, 2023. Requested information would include the respondent's name, filing date, resolution date, alleged basis and issue of discrimination, decision on the complaint (probable cause, no probable cause, etc.), outcome (went to court, dismissed, etc.), and compensation paid to the complainant. Before filing the FOIA request, we would contact someone at the agency whenever possible to ensure our requested data was available and make appropriate changes to our request if needed. Contact information was usually found on the agency's website or directory, specified below.

As of February 2025, we have received usable data from 19 states plus the EEOC.

1. Alaska: Received a spreadsheet after submitting a request via email to hrc@alaska.gov.
2. California: Received a spreadsheet after submitting a request via the online portal.
3. Delaware: Received a spreadsheet after submitting a request via email to dos.foia@delaware.gov.
4. Florida: Received a spreadsheet in PDF format after submitting a request via email to records@fchr.myflorida.com.
5. Hawaii: Received a spreadsheet after submitting a request via email to DLIR.HCRC.INFOR@hawaii.gov.
6. Illinois: Received a spreadsheet in PDF format after submitting a request via email to IDHR.FOIA@illinois.gov.
7. Kentucky: Received a spreadsheet after submitting a request via email to kchr.mail@ky.gov.
8. Maine: Compiled cases from the website after submitting a request via email to foaa@mhrc.maine.gov.
9. Massachusetts: Received a spreadsheet after submitting a request via email to cadrao@mass.gov.
10. Michigan: Received a spreadsheet after submitting a request via email to MDCR-FOIA@michigan.gov.
11. Montana: Received a spreadsheet after submitting a request via the online portal.
12. New York: Received a spreadsheet after submitting a request via the online portal.
13. North Carolina: Received a spreadsheet after submitting a request via email to

oah.postmaster@oah.nc.gov.

14. North Dakota: Received a spreadsheet after submitting a request via email to labor@nd.gov.
15. Rhode Island: Received PDFs of case files after submitting a request via email to John.Bogue@richr.ri.gov.
16. South Carolina: Received a spreadsheet after submitting a request via email to information@schac.sc.gov.
17. Texas: Received a spreadsheet in PDF format after submitting a request via the online portal.
18. Washington: Received a spreadsheet after submitting a request via email to records@hum.wa.gov.
19. Wisconsin: Received a spreadsheet after submitting a request via email to OpenRecords@dwd.wisconsin.gov.

We have **received unusable data** from 5 states:

1. Georgia: Received a spreadsheet in PDF format after submitting a request via email to info@gceo.state.ga.us. Data are unusable because there is no data from the pre-period.
2. Maryland: Received a spreadsheet after submitting a request via email to mccr@maryland.gov. Data are unusable because there is no data from the pre-period.
3. Minnesota: Received a spreadsheet after submitting a request via the online portal. Data are unusable because there is no data from the pre-period.
4. Oklahoma: Received a spreadsheet in PDF format after submitting a request via email to contact@oag.ok.gov. Data are unusable because bases of discrimination were unavailable and were not provided upon further request.
5. Pennsylvania: Received PDFs of case files after submitting a request via email to RA-RTK_PHRC@state.pa.us. Data are unusable because there is no data from the pre-period.

Our request was **closed/denied by** the following 19 states:

1. Alabama: Request submitted via the online portal and denied due to lack of state citizenship.
2. Colorado: Request submitted via email to dora_ccrd@state.co.us and denied on the basis of state law.
3. Idaho: Request was closed after the state said that they do not release any files on the basis of confidentiality.
4. Indiana: Request submitted via the online portal and denied due to the range of data requested.
5. Iowa: Request submitted via email to icrc@iowa.gov and denied on the basis of state law.
6. Louisiana: Request submitted via email to GovPublicRecords@la.gov and denied on the basis of state law.

7. Missouri: Request submitted via email to mchr@labor.mo.gov and denied on the basis of state law.
8. Nebraska: Request submitted via email to neoc.response@nebraska.gov and denied on the basis of state law.
9. Nevada: Request submitted via email to detrmedia@detr.nv.gov and denied on the basis of state law.
10. New Hampshire: Request submitted via email to humanrights@hrc.nh.gov and denied based on NH Administrative Rule Part 219.04.
11. New Jersey: Request submitted via the online portal and denied on the basis of state law.
12. New Mexico: Request submitted via email to ipra.workforce@dws.nm.gov and closed after the state asked for payment of $6000 to process the request.
13. Ohio: Request submitted via telephone and closed after the state shared that their retention period was only 5 years.
14. South Dakota: Request submitted via the online portal and denied on the basis of state law.
15. Tennessee: Request submitted via email to ask.thrc@tn.gov and denied due to lack of state citizenship.
16. Utah: Request submitted via email to sdanielson@utah.gov and closed after the state said that they did not keep any records of charge filing or resolution dates on specific charges.
17. Vermont: Request submitted via email to human.rights@vermont.gov and denied on the basis of state law.
18. Virginia: Request submitted via the online portal and decided not to pursue after the state asked for a payment of $1097.32 to process the request.
19. Wyoming: Request submitted via the online portal and decided not to pursue after the state asked for payment of $440 to process the request.

Our request is **still pending** in 5 states:

1. Arizona: Request submitted via the online portal.
2. Connecticut: Request submitted via the online portal.
3. Kansas: Request submitted via email to khrc@ks.gov.
4. Oregon: Request submitted via email to public.records@boli.oregon.gov.
5. West Virginia: Request submitted via contact us page.

D.2 Variable definitions

After receiving the complaint data, we categorize each complaint as belonging to one of five jurisdictions: employment, public accommodation, housing, education, and unspecified.

We categorize the basis of discrimination for each complaint into one of nine categories: age, disability, nationality, race, religion, retaliation, sex, and other (which includes cases where the basis is missing in the raw data). The original basis of discrimination as provided by states is much more specific than our nine categories. For example, we classify ”Equal pay” (EEOC basis) and ”Sex: Pregnancy” (North Dakota basis) both as sex.

Each case can have multiple bases. When a complaint is filed on multiple bases, we use the primary basis, usually the first listed. If the primary basis cannot be determined, complaints are classified according to the following order, with later bases taking precedence: other, age, retaliation, nationality, religion, LGBTQ, disability, race, and sex.

In addition to the basis of discrimination, each case is assigned an issue by the state. The issue is the type of action alleged. The most common issues are discharge, harassment, terms/conditions, and wages. A complaint is classified as a sexual harassment case if sexual harassment is listed as an issue and the complaint was filed based on sex in the raw data. Cases that have sexual harassment listed as an issue but were not filed on the basis of sex are excluded from classification.

A complaint is classified as a sex-based case if it was filed on the basis of sex, regardless of the issue or complainant's gender.

Compensation to the complainant is preserved from the raw data and summed together when complainants receive monetary relief at different stages in the complaint process (e.g., at the hearing and in court). Compensation is labeled as missing (instead of 0) when the complainant's case is dismissed or when such information is not available. We created an indicator variable for missing relief to account for such cases.

Complainants are considered to have “won” their case if the raw data indicates that ”reasonable cause”/”substantial evidence” of discrimination or the equivalent was found. If the raw data indicates that there was no reasonable cause/substantial evidence of discrimination, complainants are considered to have “lost” their case. In any cases where there is no explicit mention of the agency's/court's findings, including cases that were settled, the winning indicator is marked as missing.

Cases that were settled, dismissed, or went to court are tracked with corresponding indicator variables.

We create an indicator variable for whether a case was filed after #MeToo.

Some states indicate whether the complainant is female. In some cases, the raw data explicitly states this, while in others, the raw data provides only the complainant's name and not their sex. In these cases, we use an API called Genderize.io to predict the complainant's most likely gender and then visually inspect the resulting gender. In cases where both types of information are missing from the raw data, this variable is marked as missing.

Outcome Table: This table displays all of the case outcomes in our raw data and how we map each one to a standardized outcome variable.

Value	**Variable**
A1 - Complaint Withdrawn	dismissed == 1
A2 - Complaint not Timely	dismissed == 1
A4 - Complainant Not Available	dismissed == 1
A5 - Failure of CP to Proceed or Cooperate	dismissed == 1
A6 - Complainant to Court	court == 1
A7 - Administrative Dismissal	dismissed == 1
AC: General	dismissed == 1
Adjusted and Withdrawn	settle == 1
Adjusted/Terms of Settlement	settle == 1
Admin Closure	dismissed == 1
Administrative Closure	dismissed == 1
Administrative Dismissal	dismissed == 1
Annulment Issued	dismissed == 1
B1 - Successful Settlement	settle == 1
B2 - Predetermination Settlement (PDS)	settle == 1
B2a - Predetermined Settlement	settle == 1
B3 - Not Substantial Evidence (Exception)	win == 0
B4 - Conciliation Finalized	win == 1

Value	Variable
C01 - Signed/notarized complaint not returned	dismissed == 1
C1 - Hearing Decision for Complainant	win == 1
C2 - Hearing Decision for Respondent	win == 0
C4 - Pre-Hearing Settlement	settle == 1
C5 - Administrative Dismissal	dismissed == 1
CO1 - Post-Investigation Administrative closure	dismissed == 1
CO2 - Post-Investigation Settlement Agreement	settle == 1
CO3 - Post-Investigation Legal Determination of Inability to proceed	dismissed == 1
CP Filed Private Lawsuit	court == 1
CPT withdrawn - Resolved by parties	settle == 1
CPT withdrawn/dropped	dismissed == 1
CPT withdrawn - Cp electing court action	court == 1
Case Settled	settle == 1
Cause	win == 1
Cause Finding Hearing	win == 1
Cause/No Cause	win == 1
Charge Issued	win == 1
Closed - Chapter 478 (removed to court)	court == 1
Closed - Conciliated	win == 1
Closed - Dismissed	dismissed == 1
Closed - Failure to Cooperate	dismissed == 1
Closed - Lack of Probable Cause	win == 0
Closed - No Violation	win == 0
Closed - Pre-Determination Settlement	settle == 1
Closed - R&A Dismissal	dismissed == 1
Closed - Settled At Hearing	settle == 1
Closed - Unable to Locate Complainant	dismissed == 1
Closed - Violation/Enforcement	win == 1
Closed - Withdrawn	dismissed == 1
Closed - Withdrawn With Settlement	settle == 1
Complainant Elected Court Action	court == 1
Complainant Failed To Cooperate	dismissed == 1

Value	Variable
Complainant Filed Suit	court == 1
Complaint Dismissed	dismissed == 1
Complaint Rejected	dismissed == 1
Complaint Withdrawn	dismissed == 1
Complaint Withdrawn by Complainant After Resolution	settle == 1
Complaint Withdrawn by Complainant Without Resolution	dismissed == 1
Conciliated	win == 1
Conciliation	win == 1
Cp not available	dismissed == 1
Cpt Withdrawn	dismissed == 1
Determination Dismissing Complaint Issued	dismissed == 1
DF	win == 1
Dismissed w/o Pred.	dismissed == 1
DWOP	dismissed == 1
Dismissal	dismissed == 1
Dismissed for Lack of Jurisdiction	dismissed == 1
E03 - Untimely for MDCR and / or EEOC	dismissed == 1
EEOC=NJ & NDHRA=NPC	win == 0
Expired	dismissed == 1
Failure to Cooperate	dismissed == 1
Failure to Locate	dismissed == 1
Failure to Proceed	dismissed == 1
I01 - Insufficient evidence - adjusted	win == 0
I02 - Insufficient evidence	win == 0
I03 - Settlement Agreement	settle == 1
I04 - Unable to locate claimant	dismissed == 1
I05 - Claimant failure to cooperate	dismissed == 1
I06 - Wrong respondent	dismissed == 1
I07 - Claimant in court	court == 1
I11 - Withdrawn adjusted	settle == 1
I12 - Withdrawn - pursue in Court - no RTS	court == 1
I13 - Withdrawn - pursue in Court - with RTS	court == 1
I14 - Withdrawn - Not interested in pursuing	dismissed == 1
I16 - Decided by Court - no ruling on merits	court == 1
I17 - Decided by Court- w/adjustment	win == 1; court == 1
I18 - Decided by Court – no adjustment	court == 1

Value	Variable
Insufficient Evidence	win == 0
Intake Closure	dismissed == 1
Investigated and Dismissed	dismissed == 1
Judgment - Favorable	win == 1
Judgment - Unfavorable	win == 0
L01 - Post-Investigation Administrative closure	dismissed == 1
L02 - Post-Investigation Settlement Agreement	settle == 1
L03 - Post-Investigation Legal Determination of Inability to proceed	dismissed == 1
Lack of Service/Letter of Service	dismissed == 1
Lack of Substantial Evidence	win == 0
M01 - Settlement Agreement	settle == 1
M02 - Withdrawn adjusted	settle == 1
M03 - Withdrawn - not interested in pursuing	dismissed == 1
M1 - Mediation Successful	settle == 1
M2 - CP Withdrawn with Settlement	settle == 1
M3 - Complaint Withdrawn in Mediation	settle == 1
M4 - Mediatin Predetermination Settlement	settle == 1
Mediation/Settlement	settle == 1
NPC	win == 0
Negotiated Settlement	settle == 1
No Basis to Proceed	dismissed == 1
No Cause	win == 0
No Follow-Up Info Rcvd from Requestor	dismissed == 1
No Probable Cause	win == 0
No Reasonable Cause	win == 0
Notice of Right to Sue	court == 1
Notice of Rights	win == 0
P02 - Post-Charge Settlement Agreement	win == 1; settle == 1
P03 - Decided by MCRC Order - w/adjustment	win == 1
PC Determination Issued	win == 1
Post Cause Conciliation	win == 1
Pre-finding Settlement	settle == 1
Probable Cause	win == 1

Value	Variable
RC - Resolution Conference Closure	settle == 1
Reasonable Cause Recommendation - No Cause	win == 0
Reasonable Cause Recommendation - No Remedy	win == 1
Reasonable Cause Recommendation - Order	win == 1
Reasonable Cause Recommendation - Settled	win == 1; settle == 1
Reasonable Cause Recommendation - Withdrawn	win == 1; dismissed == 1
Relief covered by Ct order/consent decree	settle == 1
Resolved between Parties	settle == 1
Right to Sue	court == 1
Serve Annulment (Post-Investigation)	court == 1
Serve Final Order Dismissing Complaint	dismissed == 1
Serve Order After Hearing: Dismissing Complai	win == 0; dismissed == 1
Serve Order After Hearing: Sustaining Complain	win == 1
Serve Order After Stipulation of Settlement	settle == 1
Settled	settle == 1
Settlement	settle == 1
Split Decision: PC/LOPC or PC/LOJ	win == 1
Substantial Evidence	win == 1
Successful Conciliation	win == 1
Transfer to EEOC (Closed at Commission)	win == 0; dismissed == 1
Transferred to Circuit Court	court == 1
Unable to Locate	dismissed == 1
Unable to Serve	dismissed == 1
Untimely filed	dismissed == 1
VRA	settle == 1
WD	dismissed == 1
WDB	settle == 1
Withdrawal	dismissed == 1
Withdrawal with Benefits	settle == 1
Withdrawal With Settlement	settle == 1
Withdrawal Without Settlement	dismissed == 1
Withdrawn	dismissed == 1
Withdrawn With Resolution	settle == 1
Withdrawn Without Resolution	dismissed == 1