Accusing and Believing in Equilibrium: Evidence from #MeToo*

Anya Marchenko† Brown University

Bobak Pakzad-Hurson‡ Brown University

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Abstract

We study how #MeToo changed the strength of sexual harassment accusations and adjudicators' willingness to believe them. #MeToo caused the win rate of sexual harassment complaints to rise by 10.1pp, potentially reflecting stronger complaints (selection) or more favorable adjudicators (direct treatment). We develop a framework to disentangle these effects using complaints filed before but resolved after #MeToo. We find #MeToo made adjudicators more favorable to complainants. Complaints induced by #MeToo were stronger. However, this masks significant heterogeneity: female induced reporters brought stronger complaints, while male induced reporters brought weaker complaints. That is, #MeToo encouraged unfounded accusations only by men.

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†Marchenko: marchenko@brown.edu

‡Pakzad-Hurson: bph@brown.edu

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 the reporting of terrorism-related activity, Black Lives Matter the reporting of police misconduct, and Environmental, Social, and Governance 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 attitudes 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 more complainants).

It is not obvious how #MeToo changed the composition of cases reported, because the answer depends on the unobserved correlation between case strength and the costs of reporting. The accusations it induced may be the strongest ones (if severe cases had been the costliest to report due to stigma or fear of retaliation) or the weakest ones (previously deterred by low odds of success).

Public opinion reflected this ambiguity. The majority of people surveyed by NPR[1] thought #MeToo created accountability for perpetrators, but 43% 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 chief ways to measure the credibility of accusations made following the movement: First, are adjudicators more likely to believe them, all else equal? Second, are they stronger according to an objective metric? Following conventions in the justice system and prior literature (Lisak et al. 2010; Lonsway and Archambault 2012), we proxy for the strength of an accusation by whether it is *founded* or *unfounded*. The FBI defines an accusation as being unfounded if it is either false (i.e., fabricated) or baseless (i.e., the incident is truthfully recounted but does not meet the legal threshold for sexual harassment). Therefore, our second question can be rephrased to ask whether accusations are more or less likely to be founded as a result of #MeToo.

We investigate 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 accusations may or may not be more likely to be founded. On the other hand, if more founded (unfounded) accusations are made, then adjudicators may be more (less) likely to believe them. To answer these questions, we first empirically estimate the causal impact of #MeToo on the probability that sexual harassment complainants win their cases using a difference-in-differences design. We then use a theoretical model to decompose this empirical estimate into

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

two effects: *direct treatment* ("Is the adjudicator more or less willing to believe a given complaint after #MeToo?") and *selection into reporting* ("Are complaints induced by #MeToo more or less likely to be founded than prior complaints?").

Our data comprise sexual harassment complaints—such as inappropriate comments or requests for sexual favors—brought to state anti-discrimination agencies. These agencies serve as neutral third-party arbiters, offering a standardized process to resolve discrimination disputes. Following a complaint, the agency conducts an investigation into its merits, which may include visiting the employer site, conducting interviews, or reviewing personnel files. The agency then rules on whether discrimination occurred. These cases 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 is one where discrimination is assessed more likely to have occurred than not (i.e., more likely to be founded than unfounded).

Sexual harassment complaints are an ideal setting to study the joint impacts of the direct treatment and selection into reporting effects. First, because of the low evidentiary threshold, it is plausible that a non-trivial fraction of accusations are unfounded; with a higher burden of proof, all but the most substantiated accusations may not be made in equilibrium. Second, sexual harassment cases are frequently of a "he-said-she-said" nature and lack the forensic evidence 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), potentially giving more leeway for adjudicators' beliefs to guide the final ruling.[2]

We proceed in two parts. 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 by filing Freedom of Information Act requests with state anti-discrimination agencies.[3] We receive a total of 192,000 usable employment discrimination complaints filed between 2010 and 2023, which forms our main sample.[4] We observe the basis of discrimination alleged, filing and resolution dates, the outcome, and in some states the complainant's name and gender.[5]

2. 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 overturning juries' guilty verdicts in cases involving sexual violence." The setting we study does not have such formal evidentiary rules, allowing for the possibility that testimonial evidence plays a strong role in determining final outcomes.

3. The Equal Employment Opportunity Commission—the federal analogue of these state agencies—no longer provides information on outcomes of individual sexual harassment complaints via Freedom of Information Act requests, making the data in this paper the only detailed public record of both filings and outcomes of US workplace sexual harassment complaints post-#MeToo to our knowledge.

4. We consider additional complaints which allege discrimination in non-employment settings for robustness.

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

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 level of evidence supporting the complaint as well as how favorable adjudicators are given a fixed level of evidence. An increase in the win rate can therefore reflect complaints filed with better evidence, more favorable adjudicators,[6] or both. Since this 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 use a triple difference specification to examine effects by gender and find the effect for women is positive and significant, but not statistically different from the effect for men. While this finding may be surprising given #MeToo's focus 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 complaints and adjudicator behavior before and after #MeToo, even when the evidence presented in a given complaint is not directly observable in the data. We present a simple model in which each complaint is either founded or unfounded. The distribution of complaints can change over time; for example, if #MeToo deters sexual harassment, the share of founded complaints may change. Founded complaints are assumed to typically generate more evidence than unfounded ones, and adjudicators base their rulings on the amount of evidence present via a threshold rule: a complainant "wins" if and only if her complaint generates sufficient evidence. Therefore, founded cases win more often in expectation. This links win rates in the data and the underlying composition of founded versus unfounded complaints: first, holding fixed the share of founded accusations, a higher win rate after #MeToo indicates that adjudicators lower their evidentiary threshold and "believe" more accusations. Second, holding fixed adjudicators' evidentiary threshold, a sample with a higher win rate must have a higher share of founded complaints than a sample with a lower win rate.

We use our model findings to decompose the combined effect of #MeToo under plausible assumptions. The key step is partitioning post-period reporters into two groups: *always reporters* (a subset of reporters who are as likely to make a founded accusation as reporters pre-#MeToo) and *induced reporters* (the remaining reporters, whose complaints may be more or less likely to be founded). Because always reporters are neither positively nor negatively selected by definition, the impact of #MeToo on always reporters reflects only the direct treatment effect on adjudicators. 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. Comparing the treatment effect on overlap cases to the combined treatment effect reveals the direction of selection for induced reporters. Intuitively, if the overlap effect is smaller than the

6. Other changes affecting the overall leniency of investigations, such as more favorable treatment of complaints by staffers or witnesses, are included in this "adjudicator" effect.

combined effect, induced reporters must be positively selected to make up the difference; if it is bigger, they must be negatively selected. The size of the selection effect, but not its sign, depends on the (latent) share of complainants who are always reporters. The bigger this share, the larger is the selection effect. We present a decomposition which yields the relative size of the direct treatment versus selection effects as a function of the share of always reporters, and provide calibrations on this share from related studies.

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. 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. This implies that complaints induced to report by #MeToo are *positively* selected for foundedness.

However, these aggregate findings disguise interesting gender heterogeneity: the positive selection in new cases is driven by women. Our theoretical framework easily extends to within-gender comparisons (or other within-group comparisons), meaning that we can investigate the treatment and selection effects within gender analogously to our aggregate approach. The effect for women in overlap complaints is 8.6 percentage points (for men, 15.3 pp), while the overall effect for women is 12.3 percentage points (for men, 11.9 pp). This reveals a striking pattern: female induced reporters after #MeToo are positively selected for foundedness, whereas male induced reporters are *negatively* selected. In other words, men induced to report by #MeToo are more likely to make an unfounded accusation than are male always reporters. Moreover, our preferred calibrations suggest the selection effect for men is so large that it dominates the treatment effect of adjudicators; men induced to report by #MeToo win less often than would male always reporters in a counterfactual world where #MeToo never happened.

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. But the *newly induced* 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 founded. Men, on the other hand, started reporting sexual harassment complaints that were less likely to be founded.

Which mechanisms may explain these patterns, specifically the differences across gender? While details of specific cases are sparse, we make a number of observations. First, effects for both men and women are concentrated in Democrat-voting states where support for #MeToo was likely largest. This suggests that adjudicators and complainants moved in tandem: when adjudicators become more lenient, the pool of reporters changed. Second, effects are largest for complainants employed by Fortune 500 firms, but are similar in male-dominated and female-dominated industries. This suggests that the impact of #MeToo on accusing and believing was more driven by firm-level

differences, rather than industry-level differences. Finally, given evidence that sexual harassment is rarely *falsely* reported (Lisak et al. 2010; Heenan and Murray 2006), and given that unfounded complaints are defined as either being false or failing to meet the legal definition of wrongdoing, it is plausible that our negative selection results for men may simply reflect that male induced reporters were unclear on what legally qualifies as sexual harassment.

1.1 Related Literature

There is a growing empirical literature studying the effects of the #MeToo movement on sexual wrongdoing. The closest study to ours is Levy and Mattsson (2025), which uses crime data to estimate the 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. They interpret these findings as evidence of increased reporting, rather than increased incidence, of sexual violence. This conclusion is corroborated by Gauthier (2025), who finds that the #MeToo movement had the effect of increasing reporting while deterring sex crime. Our study builds on this work by focusing on civil administrative cases, which capture harassment that is arguably more representative of the workplace incidents that #MeToo 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, perhaps reflecting a greater margin for change in the assessment of cases that we study.

Our claim that the choice to report is interdependent with the favorability of adjudicators is consistent with prior literature on sexual misconduct in the workplace, which finds that the rate of reporting is an equilibrium object. Boudreau et al. (2024) show that the costs of reporting sexual harassment, which are based in part on a fear of retaliation, are endogenous to the reporting and punishment institutions in use. For example, when outside job opportunities worsen, workers report less sexual harassment to avoid retaliation from the employer (Dahl and Knepper 2026). Women report more sexual harassment in male-dominated workplaces where high wages may be acting as a compensating differential (Folke and Rickne 2022; Hersch 2011, 2018), and they quit at higher rates following #MeToo (Batut, Coly, and Schneider-Strawczynski 2021). More broadly, the gender composition of managers within a firm affects the severity of consequences for cross-gender violence (not necessarily sexual) (Adams-Prassl et al. 2024), and therefore plausibly the rate at which wrongdoing occurs. In short, changing the costs or benefits of reporting may have large effects on the reporting rate. This paper studies what happens downstream once sexual harassment is reported and how changes in social attitudes impact who reports versus who is believed.

Our modeling approach also relates to the design of punishment mechanisms, as first studied by Becker (1968). Specifically, our theoretical framework borrows from Kaplow (2011); 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 an unfounded allegation) and type II

error (not believing a founded allegation). We make no attempt to explicitly model or measure the adjudicator's preferences over these types of errors. Instead, we allow #MeToo to flexibly change the evidentiary threshold through unmodeled changes to the costs of type I versus type II errors. An important implication is that our model allows for accusations to be "false,"[7] which differs from other models of the #MeToo movement (Cheng and Hsiaw 2022; Boudreau et al. 2024).[8] 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).

The effects we estimate reflect changes in individual behavior rather than changes in legal regimes. Numerous non-legal factors have been shown to affect judicial behavior, including lunch breaks, outcomes of recent sporting events, temperature, and economics training (Danziger, Levav, and Avnaim-Pesso 2011; Eren and Mocan 2018; Heyes and Saberian 2019; Ash, Chen, and Naidu 2026). But unlike these examples, the #MeToo movement was a society-wide, durable shift in attitudes towards sexual harassment that potentially affects adjudicators at scale—for example, female judges in China became more likely to grant divorces when domestic violence was indicated after #MeToo, corroborating that #MeToo can directly influence the behavior of judges by changing their attitudes (Cai et al. 2025).[9]

Methodologically, we contribute a framework to decompose the treatment effect recovered from a difference-in-differences regression into a direct treatment effect and a selection effect, using two assumptions beyond the standard parallel trends assumption: an additional parallel trends assumption on non-selected individuals, and an assumption that treatment homogeneously affects all individuals whose outcome is determined after treatment.[10] Our approach is potentially applicable to other policy settings—public housing or grant applications, permit decisions, insurance claims—that have a 'delayed-resolution' feature.

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,

7. Prior studies estimate that fewer than 10% of sexual assault complaints are false, in the sense that the alleged wrongdoing was not committed or attempted (Heenan and Murray 2006; Lisak et al. 2010). In our data, 83% of cases that reach the investigation stage lose, suggesting far more complaints are rejected by adjudicators than would be expected based on the rate of false accusations alone. One factor that explains this discrepancy is that we study sexual *harassment*, not sexual *assault*. A second factor is that not all unfounded complaints are false. Recalling the terminology used by the justice system, it is plausible that the typical complaint that loses in our setting is not false but rather baseless, i.e., it does not meet the legal definition of sexual harassment.

8. By contrast, Chassang and Padró i Miquel 2018 theoretically study settings in which accusations can be unfounded. The focus of their investigation is on the optimal design of punishment policies when reporters may experience retaliation for bringing forth accusations. This relates to the consideration of adjudicators in the current paper, but our goal is to measure *how* adjudicators respond to #MeToo, to describe the specific reasons for their responses.

9. In this regard, our paper also relates to recent research on the Black Lives Matter (BLM) movement, which studies changes in the number of reports, and subsequent rates, of crime (Campbell 2024; Devi and Fryer Jr 2020). However, to our knowledge, there has been no work relating BLM to the specific accusations of wrongdoing – that is, on whether accusations against specific police officers post-BLM are more or less likely to be founded, or on how adjudicators of police violence changed the way they assess accusations. Our model-based approach of disentangling the direct treatment and selection effects using "overlap" cases is likely applicable to these settings.

10. While there is a rich literature dealing with treatment versus selection effects in IV analyses (Kirkeboen, Leuven, and Mogstad 2016; Mountjoy 2022; Black et al. 2022) or RCTs (Lee 2009), we are not aware of any analogous work for difference-in-differences. For related theoretical work, see Castro-Pires and Georgiadis 2026.

Section 5 presents our aggregate estimates, Section 6 presents a model of the institutional setting, Section 7 empirically decomposes the combined effect into objects of interest, Section 8 presents the results of the decomposition, and Section 9 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 a fourth of men in the US report experiencing sexual harassment in their lifetime.[11] 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.

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 individual's work (Legal Information Institute).[12] 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.[13]

2.3 Filing a sexual harassment complaint

Sexual harassment is typically treated as a civil offense and addressed through administrative complaints or civil lawsuits. More serious conduct, such as rape, physical assault, or stalking, can

11. 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).

12. Figure A1 provides specific examples of sexual harassment.

13. 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 (Frisch 2013).

rise to the level of a criminal offense or may be classified as a different type of sex crime. This paper focuses on civil cases.

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 HR before filing with a state agency. According to state officials, complainants may not report sexual harassment to their employer if 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 the respondent's name, filing and resolution dates, alleged basis and issue of discrimination, the agency's decision, and any compensation awarded. In Appendix F, we detail our FOIA request process.

We analyze three samples, each comprising discrimination complaints of different types filed across various states. Our main analysis uses the most restricted sample, which includes only employment discrimination, reflecting that #MeToo was primarily a movement targeting sexual wrongdoing in the labor market. This sample contains 191,985 complaints across 10 states and is the focus of the paper's main results. We use two broader samples in robustness analyses. The second sample includes all data in our main sample plus data from Connecticut, which includes allegations of either employment or public accommodation discrimination (while Connecticut does not distinguish between these two types of complaints, we include them together because employment discrimination allegations are 25 times more common than public accommodation discrimination in the rest of our data). This second sample contains 211,761 complaints across 11 states. The third sample includes all types of discrimination complaints (employment, public accommodation, housing, and education) and comprises 351,222 complaints across 18 states. Summary statistics for all three samples are presented in Appendix B.1.

We harmonize the types of alleged discrimination with federal discrimination categories: race, religion, sex (including sexual harassment), nationality, age, disability, and retaliation (punishment for filing a discrimination complaint).[14]

Our treatment group consists of sexual harassment complaints, and other types of discrimination

14. 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

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). 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 unresolved after 766 days, or more than two standard deviations above the mean resolution time.

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 made 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.[15]

Our main outcome of interest is the finding of the investigation stage, as the investigation finding offers a clear signal as to the level of evidence supporting the complainant. Therefore, we restrict our primary analysis to cases that reach the investigation stage. We consider a complainant to have "won" if there is a finding of reasonable or probable cause of discrimination during the investigation. We consider the complainant to have "lost" if no probable cause is found. The outcome of the investigation is often payoff relevant; typically, anti-discrimination agencies use the result of the investigation as the main determinant in assigning remedies. 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 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 label a case "Settled" if the raw data indicates a settlement or mediation; "Court" if the case is removed to a court proceeding overseen by a judge (in which case we do not observe the outcome); and "Dismissed" if it is administratively closed due to logistical issues such as being out of the agency's jurisdiction, filed in an untimely manner, or involving a complainant who could not be contacted. No restitution is made in dismissed cases.[16] These outcomes are mutually exclusive, with a few rare exceptions that arise from exigencies of each state's system. We provide additional details on mapping the raw data into case outcomes in Appendix F.

Approximately 7% of cases settle, 8% are dismissed outright, and 43% go to court, where we do

15. The outcomes in our data are terminal, meaning we observe the discrimination ruling for every investigated case. Moreover, outcomes are non-duplicative for all but 0.17% of cases, meaning that nearly every investigated case receives only one outcome code in our data. Our results are robust to excluding the small number of cases with multiple outcomes.

16. 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 for doing so is likely very low given the small percentage of cases that are dismissed outright.

not observe their outcome. Of the remaining 42% of cases that are investigated by the agency, 83% (0.35 / 0.42) lose. Table 1 provides summary statistics and differences in means before and after #MeToo. In Appendix B.1, we detail trends in the rate of sexual harassment allegations over time, both before and 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. Our main estimating equation is a within-state two-way fixed effects specification:

$$Y_{is(i)m(i)} = \beta^{CE}\left(\mathbb{1}\left[g(i) = \text{SH}\right] \times \mathbb{1}\left[m(i) \geqslant \text{Post}\right]\right) + \alpha_{g(i)s(i)} + \mu_{m(i)s(i)} + \epsilon_{is(i)m(i)} \tag{1a}$$

where i is a complaint, $g(i) \in \{\text{sexual harassment, age, disability, nationality, race, religion, retaliation}\}$ indexes complaint type, $s(i)$ indexes state, and $m(i)$ indexes the year-month of when the complaint was resolved.[17] Time fixed effects $m(i)$ are defined at complaint resolution rather than complaint filing to capture changes in adjudicator behavior at the time of decision. The treatment indicator $\mathbb{1}\left[g(i) = \text{SH}\right] \times \mathbb{1}\left[m(i) \geqslant \text{Post}\right] = 1$ if i is a sexual harassment complaint resolved after #MeToo begins. $Y_{is(i)m(i)}$ is an indicator for whether the complainant wins their case (versus loses), conditional on reaching the investigation stage. This regression therefore compares the win rate of sexual harassment complaints (treated) to the win rates of other types of discrimination complaints (control) in the same state, before and after #MeToo. Following Levy and Mattsson (2025), we cluster standard errors at the state-by-complaint-type level to align with how treatment is assigned, and to account for within-state correlation in complaint adjudication, for a total of 57 clusters.

We include interactions with state dummies $s(i)$ to account for differences in how complaints are adjudicated between states. $\alpha_{g(i)s(i)}$ are complaint type-by-state fixed effects accounting for constant state policies that differentially affect sexual harassment and non-sexual harassment outcomes. $\mu_{m(i)s(i)}$ are time-by-state fixed effects accounting for time-varying state policies affecting all filed cases. 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, $\mu_{m(i)s(i)}$ control for the impact of these policies.

β^{CE} ("combined effect") is our main coefficient. In order to interpret this coefficient as a parameter of interest, we assume throughout that #MeToo is unanticipated (no effect in the pre-periods), and that in the absence of #MeToo, the outcomes of sexual harassment and control complaints would have trended in parallel within a state. To formalize, assume a simple two period model where $t = -1$ is the pre-period and $t = 0$ is the post-period. Suppressing state indices, let $Y_{it}(1)$ be the potential outcome if a complaint had received treatment in time t and $Y_{it}(0)$ if a complaint had not received

17. Since some retaliation cases may be filed in response to sexual harassment, in Appendix C, we drop retaliation complaints from the control group and confirm this does not change our results.

treatment in time t. Let D_{it} indicate actual treatment status at time t, so $D_{i0} = 1$ for (and only for) sexual harassment complaints resolved after #MeToo. Under a *standard parallel trends assumption* that $\mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0] = \mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1]$, β^{CE} identifies the average treatment effect on the treated, $ATT^{CE} := \mathbb{E}[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1] - \mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1]$.

We call β^{CE} the combined effect because it captures the effect of changes in the probability of winning holding case characteristics fixed (direct treatment), as well as the effect of changes in the composition of cases reported (selection).[18] The sign of β^{CE} is, by itself, insufficient to determine the direction or magnitude of these two effects. To see why this is the case, suppose #MeToo makes adjudicators more likely to believe all sexual harassment complainants. This implies a positive direct treatment effect. But we may still recover a *negative* β^{CE} if #MeToo induces mass reporting of grievances that do not rise to the level of sexual harassment. In other words, a sufficiently high share of new unfounded complaints may result in a negative β^{CE}, even if adjudicators' attitudes overall are more favorable to complainants. Analogously, a sufficiently high share of new founded complaints may result in a positive β^{CE}, even if adjudicators' attitudes overall are less favorable to complainants. Decomposing β^{CE} will be the task of the second half of this paper.

For our event studies, we estimate a dynamic analogue of Equation 1a, where t indexes yearly leads and lags around #MeToo, $t = -1$ is the 12 month period before October 15, 2017, and SH is a sexual harassment complaint:

$$Y_{is(i)m(i)} = \sum_{t \neq -1} \beta(t) \left(\mathbb{1}\left[g(i) = \text{SH}\right] \times \mathbb{1}\left[m(i) \in t\right] \right) + \alpha_{g(i)s(i)} + \mu_{m(i)s(i)} + \epsilon_{is(i)m(i)} \tag{1b}$$

In a third specification, we estimate the differential impact of #MeToo for women versus men using triple differences. Specifically, we estimate a version of Equation 1a where treatment is interacted with an indicator variable representing the complainant's gender:

$$\begin{aligned} Y_{is(i)m(i)} = \; & \beta^{DDD} \left(\mathbb{1}\left[g(i) = \text{SH}\right] \times \mathbb{1}\left[m(i) \geqslant \text{Post}\right] \times \mathbb{1}\left[i = \text{Female}\right] \right) \\ & + \beta^{CE}_{M} \left(\mathbb{1}\left[g(i) = \text{SH}\right] \times \mathbb{1}\left[m(i) \geqslant \text{Post}\right] \right) \\ & + \alpha_{g(i)s(i)} \; + \; \mu_{m(i)s(i)} \\ & + \left(\alpha_{g(i)s(i)} \times \mathbb{1}\left[i = \text{Female}\right] \right) \; + \; \left(\mu_{m(i)s(i)} \times \mathbb{1}\left[i = \text{Female}\right] \right) + \epsilon_{is(i)m(i)} \end{aligned} \tag{2a}$$

We assume that 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: 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

18. These changes may have occurred for several reasons, including as a result of state policies instituted after #MeToo that may have *differentially* impacted sexual harassment and control complaints. Because our fixed effects would not absorb such differential impacts, they would appropriately be captured in β^{CE} as part of the #MeToo effect.

outcomes for sexual harassment versus control complaints filed by men (Olden and Møen 2022).

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

$$
\begin{aligned}
Y_{is(i)m(i)} = & \sum_{t \neq -1} \beta^{DDD}(t) \Big(\mathbb{1}\big[g(i) = \text{SH}\big] \times \mathbb{1}\big[m(i) \in t\big] \times \mathbb{1}\big[i = \text{Female}\big] \Big) \\
& + \sum_{t \neq -1} \beta_M^{CE}(t) \Big(\mathbb{1}\big[g(i) = \text{SH}\big] \times \mathbb{1}\big[m(i) \in t\big] \Big) \\
& + \alpha_{g(i)s(i)} + \mu_{m(i)s(i)} \\
& + \Big(\alpha_{g(i)s(i)} \times \mathbb{1}\big[i = \text{Female}\big] \Big) \; + \; \Big(\mu_{m(i)s(i)} \times \mathbb{1}\big[i = \text{Female}\big] \Big) + \epsilon_{is(i)m(i)}
\end{aligned}
\tag{2b}
$$

5 Aggregate Results

We present estimates of Equation 1a in Table 2, Column 1, and the event study that corresponds to Equation 1b in Figure 3a. Our data covers the 7 years prior to, and the 6 years after, #MeToo, so the horizontal axis of the event study plot spans from -7 to 6. We estimate that #MeToo raised complainant win probability by $\hat{\beta}^{CE} = 10.1$ percentage points (std. err. = 2.9) or 66% over the control mean. A visual inspection of the event study shows little evidence of pre-trends and that pre-treatment coefficients are not statistically significant. Post-treatment coefficients are positive and indicate an average impact of #MeToo that aligns with the point estimate of the static regression.

This (perhaps large) treatment effect is plausible given the salience of #MeToo, the absence of pretrends, and the scope for adjudicator discretion in our setting. First, we show a surge of sexual harassment complaints filed immediately in the wake of #MeToo in Appendix B.1, suggesting that the movement was highly salient at the start of the treatment period and that this salience was concentrated in sexual harassment cases. Second, it is unlikely that our results are driven by broader changes in policy or political attitudes (e.g., the start of the first Trump presidency in 2016): any such effect would either be absorbed by year fixed effects (if it affected all discrimination cases) or manifest as pretrends (if it affected only sexual harassment cases), which we do not observe. Third, unlike criminal proceedings, our setting involves relatively little hard evidence and substantial scope for adjudicator discretion, making shifts in societal norms particularly consequential.

In Appendix C, we show that our findings are robust to multiple changes in specifications and data choices, including: restricting to complaints alleging only one type of discrimination to eliminate potential ambiguity from overlapping discrimination types; excluding retaliation complaints from the control group in case they are responses to previous sexual harassment; restricting to complaints filed pre-Covid-19 to ensure our main effect is not being driven by pandemic-related disruptions and to remove mechanical over-representation of quick-to-resolve complaints at the end of our sample; re-running our main results with time fixed effects defined at case filing rather than case resolution; and considering two broader samples that include additional discrimination complaints (including housing, public accommodation, and education) across

additional states (up to 18 in total) to test whether our main effect can be replicated outside of labor markets. $\hat{\beta}^{CE}$ remains positive and significant in every robustness check, retaining a similar order of magnitude as our preferred specification. Finally, we conduct a permutation test where we reassign treatment to *untreated* discrimination types and find that these placebo tests fail to generate economically and statistically significant effects.

We show results and event studies for secondary outcomes—rates of settling and exiting to court—in Appendix B.2. #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. = 0.6) that is unlikely to meaningfully affect our main analysis.[19]

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 winning 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.[20]

A priori, we expected the effect of #MeToo to be concentrated in cases brought by 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 men win their 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. = 0.022). Figure 3b presents the corresponding event study, again exhibiting little evidence of pre-trends. The orange coefficients for women closely align with the blue coefficients for men, and the average of the post-period coefficients roughly equals the point estimate. The only exception is at $t = 6$, but we interpret this as an artifact of few observations with gender in the last period (note the larger standard errors on the male coefficient at $t = 6$). More broadly, in Appendix B.2, we show that no meaningful gender differences exist across any outcomes.

Finally, we examine heterogeneity by state in Appendix B.3, grouping states into red, blue, or purple by their voting history (see Table A1 for classification). We find that our results are driven by blue (i.e., Democrat-voting) states. The effect of #MeToo in red and purple states is virtually zero. This aligns with prior literature on the geography of #MeToo, which finds that the positive

19. More specifically, we claim that #MeToo's effect on the selection of cases into exiting to court is unlikely to significantly drive the effect of #MeToo on win rates. This can be seen via a Lee 2009-style bounding exercise on the excess court cases post #MeToo: we reassign the 0.7pp excess court cases post #MeToo to either all be wins or all be losses to mimic the extremal counterfactuals in which excess court cases instead went to investigation. Under this exercise, the estimate of our main effect, which we describe shortly, of 0.101 would change to 0.100 (excess court cases would all have been losses) and 0.102 (excess court cases would all have been wins), suggesting that changes to the likelihood of exiting to court only minimally affects our results.

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

effects of #MeToo for reporters are concentrated in liberal places in the US (Levy and Mattsson 2025), and with poll evidence showing greater support for #MeToo among left-wing voters.[21]

6 Theoretical Framework

Our goal in this section is to decompose the combined effect of #MeToo into a selection effect—how the foundedness 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 framework. In it, each individual considers whether to make a report of sexual harassment with the pre-existing knowledge of whether her potential accusation is founded or unfounded, where a founded 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 a positive payoff if her accusation is "believed" by the adjudicator. We use the model to study the optimal decision to report instances of (potential) sexual harassment and to understand adjudicators' decisions to believe the accusations or not.

We then consider how changes after #MeToo affect these decisions. Our framework makes no assumptions about, and imposes no structure on, how adjudicator and individual complainant decisions evolve as a result of #MeToo; instead, our model takes as given data (which is estimable from our empirical environment) and provides a decomposition into selection and treatment effects.

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 each individual knows whether or not her potential accusation is founded before she decides to report, our findings are essentially unchanged if instead individuals do not know the foundedness of their claims, i.e., they are unsure 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 $\theta_i = (c_i, \tau_i) \in \{L, H\} \times \{F, U\}$, where $0 = L < H < \infty$ is the cost of making an accusation, and F and U stand for whether a potential accusation would be "founded" or "unfounded," respectively. We assume that in each time period, there exists a positive measure of agents of each type $\theta \in \{L, H\} \times \{F, U\}$.

The timing of the stage game is as follows. Upon observing her type, each individual chooses to

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

either *report* or *not report*. If an individual does not report, then her game ends immediately and she receives utility normalized to zero. If she reports, her case enters the review process. Conditional on reporting, one of three mutually exclusive outcomes occurs to the case: (i) it is settled with probability p^t_{settle}, (ii) it is routed to court with probability p^t_{court}, or (iii) it proceeds to the agency investigation stage with the remaining probability $p^t_{inv} := 1 - p^t_{settle} - p^t_{court}$. We assume p^t_{settle} and p^t_{court} (and hence p^t_{inv}) are independent of τ_i and other case features. Moreover, we assume that $p^t_{inv} > 0$ for all t.

For cases that enter the investigation stage, two mutually exclusive outcomes occur. The investigation can either be "won" by the complainant, in which case she receives a monetary award π^t_{win}, or it can be "lost" by the complainant, in which case no payment is made. The award π^t_{win} is potentially random, and the distribution of awards can differ by the foundedness of the claim, so long as the award payment has finite expectation for each level of foundedness. "Winning" is a stochastic outcome that occurs with probability $p^t_{win}(\tau_i)$ in period t.

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 founded; each complaint generates a level of evidence that is randomly and independently distributed, where the distribution depends on the foundedness of the complaint. To formalize these notions regarding evidence, let G_F and G_U be distributions of evidence generated for founded and unfounded claims, respectively. We assume that G_F and G_U have full support over $[0,1]$ and are continuously differentiable with probability distribution functions g_F and g_U, respectively. We assume that the likelihood ratio $g_F(e)/g_U(e)$ is strictly increasing in e, that is, higher values of evidence are more likely to be uncovered if the complaint is founded, 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. Given that reaching the investigation stage is independent of the case foundedness, the probability that case i wins conditional on reaching the investigation stage is $1 - G_{\tau_i}(\underline{e}^t)$. Because the level of evidence generated is a noisy signal of the foundedness of a complaint, for any $\underline{e}^t \in (0,1)$, there is positive probability of both type I error (unfounded complaints that are won, which occurs for unfounded complaints with probability $1 - G_U(\underline{e}^t)$) and type II error (founded complaints that are lost, which occurs for founded complaints with probability $G_F(\underline{e}^t)$).[22]

Each individual's payoff from reporting is equal to any monetary award she receives minus the

22. 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 of type I and type II errors induces a threshold rule as being optimal. We do not explicitly model these error 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.

cost of reporting, and her payoff from not reporting is normalized to zero.

Let

$$u_i^t := p_{settle}^t \, \mathbb{E}_t[\pi_{settle}^t|\tau_i] + p_{court}^t \, \mathbb{E}_t[\pi_{court}^t|\tau_i] + p_{inv}^t \, p_{win}^t(\tau_i) \, \mathbb{E}_t[\pi_{win}^t|\tau_i] - c_i \tag{3}$$

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. Note that in our empirical analysis, our main outcome corresponds to p_{win}^t, i.e., the probability of a favorable finding conditional on reaching the investigation stage, rather than the unconditional probability $p_{inv}^t \cdot p_{win}^t$.

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 whether they 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 "foundedness." While we find it reasonable, based on the literature, to dichotomize the foundedness 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 founded and unfounded 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 founded and unfounded cases ensure the desired outcome. Moreover, our assumption that each individual observes her type prior to reporting implies that accusers know ex-ante whether or not their complaints are in good faith. This is not necessary, and we could have instead complicated our model by assuming individuals receive a (potentially uninformative) signal of the foundedness 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 her type via the second component $\tau \in \{F, U\}$. 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.

Fourth, we assume that the probabilities of going to court or settling are independent of other features of the model. This implies that cases that go to investigation are neither positively nor negatively selected for foundedness.[23] While this is a limiting assumption, it is common in the literature[24] and our theoretical 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 foundedness. Reassuringly, as discussed in Footnote 19, we do not view the selection of filed cases into the investigation stage to be first order.

Finally, we do not include any notion of demographic groups in our model, while our empirical analysis separately considers the differential effects of #MeToo on male versus female complainants. Our model can straightforwardly be extended to consider different demographic groups (e.g. men and women), and all of our findings below will hold within group. Moreover, the types and type distributions in each of $t = -1$ and $t = 0$ can be different across groups. This flexibility allows us to consider the case in which, for example, the type distributions of male and female reporters differ pre-#MeToo, and where #MeToo differentially affects the type distribution—by deterring acts of sexual wrongdoing, or changing the cost of reporting—for each gender.

In Appendix E, we study an extension of our model in which adjudicator signal precision can also change (as in Ronchi and Salvestrini 2025). The forces leading to our empirical decomposition are unchanged, i.e., our broader framework nests both leniency and precision changes.

6.3 Theoretical Tests

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 $\tau \in \{F, U\}$, let R^t_τ represent the set of reporters with foundedness 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., $|R^t_\tau| > 0$.

23. 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.

24. 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' estimates assume that judges' choices do not 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..." Our assumption similarly attributes the effects of changes in the win rate after #MeToo to adjudicators, however, as discussed in Section 6.2, our framework allows #MeToo to change the foundedness distribution, potentially through deterrence of wrongdoing.

Therefore,

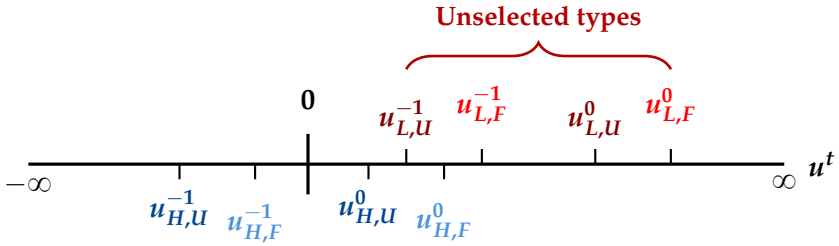
$$\mathcal{T}_t := \frac{|R_F^t|}{|R_F^t| + |R_U^t|} \in (0,1)$$

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

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

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

In words, a set of reporters r^0 in the post-#MeToo period is said to be unselected if the share of founded accusations within it is equal to the share of founded accusations 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,U) and (L,F) 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,U) and (H,F) 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 likely to be founded) or negatively selected (i.e. less likely to be founded).



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)*. The definition of always reporters is well defined, that is, a maximal unselected set of reporters necessarily exists.[25] If there are no composi-

25. The following argument demonstrates that a set of always reporters must exist. An unselected post-period subset of

tional changes across periods and the same types report in both periods, all reporters post-#MeToo are always reporters and the set of induced reporters is empty. While there are, in general, many possible sets of always reporters, they each will have the same composition of foundedness as the entire set of reporting agents pre-#MeToo, and our upcoming results will rely only on the composition of the set of always reporters, not their identities.

The following proposition relates changes in the win rates 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.

Proposition 1.

1. *If the expected win rate increases (decreases) for always reporters after #MeToo, then the evidentiary threshold is lower (higher) after #MeToo.*
2. *Complaints by induced reporters are more likely to be founded than those of always reporters if and only if induced reporters have higher expected win rates than always reporters after #MeToo.*

Part 1 discusses how a change to the win rate of always reporters reveals changes to the evidentiary threshold: an increase in win rates for always reporters corresponds to a lowering of the evidentiary threshold. Similarly, a decrease in win rates for always reporters corresponds to a raising of the evidentiary threshold. Part 2 discusses how selection into reporting affects outcomes of interest. If induced reporters are positively selected (their complaints are more likely to be founded), 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.

Remark 1. *Proposition 1 is stated in terms of always reporters and induced reporters. However, its findings and the 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.*

---

total measure m must contain $\mathcal{T}_{-1} \cdot m$ measure of founded reporters and $(1 - \mathcal{T}_{-1}) \cdot m$ measure of unfounded reporters. Hence m is feasible as the measure of an unselected set if and only if

$$\mathcal{T}_{-1} \cdot m \leqslant |R_F^0| \qquad \text{and} \qquad (1 - \mathcal{T}_{-1}) \cdot m \leqslant |R_U^0|.$$

Therefore, the maximal feasible measure is

$$m^{AR} = \min \left\{ \frac{|R_F^0|}{\mathcal{T}_{-1}}, \frac{|R_U^0|}{1 - \mathcal{T}_{-1}} \right\}.$$

Since the reporter sets are measured by atomless Lebesgue measure, we can choose subsets

$$A_F \subseteq R_F^0, \qquad A_U \subseteq R_U^0$$

with

$$|A_F| = \mathcal{T}_{-1}\, m^{AR}, \qquad |A_U| = (1 - \mathcal{T}_{-1}) m^{AR}.$$

Then $A_F \bigcup A_U$ is unselected by construction and maximal, because any larger unselected subset would require more founded or unfounded post-period reports than exist.

7 Empirical Decomposition

We now turn to empirically decomposing our main difference-in-differences effect into a direct treatment effect on adjudicators, and into a selection into reporting effect. This decomposition is based on our theoretical framework, which partitions reporters post-#MeToo into always and induced reporters. In this section, we demonstrate an empirical method for estimating treatment effects on each of these groups. Then, in Section 8, we use these estimates, along with the findings of Proposition 1, to make conclusions about selection into reporting and changes to adjudicators' evidentiary threshold.

Again considering a two-period model for simplicity of exposition, recall that the "combined" treatment effect is given by $ATT^{CE} = \mathbb{E}[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1] - \mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1]$, which includes the outcomes of both ARs and IRs. To aid in our decomposition, define the ATT for always reporters and induced reporters, respectively, as $ATT^{AR} := \mathbb{E}[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, ARs] - \mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, ARs]$ and $ATT^{IR} := \mathbb{E}[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, IRs]$. Each of these two terms is similar to ATT^{CE} conditioning on always/induced reporters, respectively.

The Law of Iterated Expectations yields the following decomposition of the combined effect of #MeToo, ATT^{CE}, into the causal effects of #MeToo on ARs and IRs, where $\omega \in (0, 1)$ represents the share of always reporters in the post period:

$$ATT^{CE} = \omega ATT^{AR} + (1 - \omega) ATT^{IR}. \tag{4}$$

As a reminder, the definition of always (and induced) reporters depends on the composition of case foundedness: always reporters form a *maximal subset* of post-#MeToo reporters with the same foundedness distribution as the set of pre-#MeToo reporters. This contrasts with instrumental variable settings, in which an always reporter is an *individual* who reports regardless of treatment. In our setting, the foundedness of a case (and whether it resolves pre- or post-#MeToo) determines the likelihood that it wins, which is why our definition of always reporters matches not on the identity of reporters, but on the distribution of foundedness. Thus, ω can be thought of as a "similarity index" of the foundedness distributions before and after #MeToo, with larger values of ω representing more similarity. $\omega \approx 0$ corresponds to the case in which the vast majority of cases in the pre-period are founded and the vast majority of cases in the post-period are not founded (or vice versa), and $\omega = 1$ corresponds to the case in which the share of founded accusations is identical pre- and post-#MeToo. However, because the foundedness of any given case is not directly observable, the share of always reporters (ω) is a latent variable. As a result, novel identification and estimation exercises are required to discern the terms of Equation 4:

- In Section 4 we showed that ATT^{CE} is identified by β^{CE} (see Equation 1a) under the *standard parallel trends assumption*, and presented empirical estimates.

- In Section 7.1, we will show that ATT^{AR} is identified (under plausible assumptions)[26] by restricting our regression analysis to cases filed before, but resolved after, #MeToo.
- In Section 7.2, we will present a conservative calibrated value of ω based on an estimate from the literature, and discuss a range of potential calibrations.
- With these terms in hand, we identify ATT^{IR} as the unique solution to Equation 4.

Section 8 relates these individual estimates to our economic questions of interest: do adjudicators believe complainants more after #MeToo, and are the complaints of induced reporters more founded on average than the complaints of always reporters?

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.

7.1 Estimating ATT of always reporters

In this section, we show how to empirically estimate ATT^{AR} by exploiting a feature of discrimination complaints shared with many other settings (such as permit approvals or grant applications); namely, that there is a delay between filing and resolution.

Specifically, we consider complaints filed *before* but resolved *after* #MeToo. We call these *overlap* cases. Since overlap cases are filed before #MeToo, their selection into reporting could not have been affected by #MeToo. Under plausible assumptions, we show that the treatment effect for overlap complaints identifies the treatment effect for always reporters, i.e., it reflects the change in leniency on the side of the institutional adjudication process. We often refer to this effect as the direct treatment effect on adjudicators, while recognizing that other institutional changes affecting leniency of the investigation may contribute to this direct effect (e.g., more favorable treatment of complainants by staffers or witnesses).

To operationalize the overlap analysis, we run the same difference-in-differences specifications as our main regressions (Equations 1a and 1b), but restrict the sample to cases filed before #MeToo. Therefore, this restricted regression compares overlap and pre-period sexual harassment cases to overlap and pre-period control cases, and we refer to the coefficient of interest as β^O (where the superscript denotes overlap cases). Figure 4 illustrates this comparison.

To identify β^O as ATT^{AR}, we make two assumptions, A1 and A2, which we describe below:

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

Note that A1 is equivalent to the *standard parallel trends assumption* presented in Section 4, except

26. Because of our difference-in-differences empirical strategy, the primary assumption required is a refinement of the standard parallel trends assumption.

that it applies only to the outcomes of always reporters in the treated group as opposed to all reporters in the treated group. Intuitively, this difference in assumptions mirrors the direct treatment and selection effects we study. The standard parallel trends assumption, along with ATT^{CE}, allow for selection into reporting; the standard parallel trends assumption assumes that in the absence of the direct treatment effect of adjudicators, the expected win rate of *the entire set of sexual harassment cases post #MeToo* trends the same as the win rate of non-sexual harassment cases. Assumption A1, along with ATT^{AR}, are unrelated to selection into reporting; because always reporters are definitionally unselected, A1 states that in the absence of the direct treatment and selection effects, the expected win rate of sexual harassment cases trends as does the win rate of non-sexual harassment cases. Since A1 is a parallel trends assumption, plotting and examining pre-trends can provide evidence consistent or inconsistent with this assumption. The absence of pre-trends, which we verify below, is consistent with A1.

Recalling terminology from our theoretical model, overlap cases are definitionally unselected, but practically, they account for only a subset of always reporters. This is because there are many cases (both founded and unfounded) filed after #MeToo. The following assumption is necessary to extrapolate the treatment effect from overlap cases to the entire set of always reporters, regardless of filing timing:

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.

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). Applied to our setting, A2 implies that #MeToo treats all adjudicators immediately upon onset and that treatment effects for always reporters do not change over time. Using the notation of our theoretical model, this assumption states that there is a constant evidentiary threshold $\underline{e}^0$ after October 2017. A2 is plausible if adjudicators do not observe case filing time, or are disallowed from considering it when making their determination. We provide corroborating evidence for this assumption after presenting our estimates.

The following proposition states our identification result.

Proposition 2. *Under assumptions A1 and A2,* $\beta^O = ATT^{AR}$.

As previously described, A1 allows β^O to identify the ATT for complainants in the overlap group in a similar way that the standard parallel trends assumption allows β^{CE} to identify ATT^{CE}. A2 allows for extrapolation to the entire set of always reporters, i.e., including those who file after #MeToo and therefore do not directly contribute to β^O.[27] A formal proof of Proposition 2 is straightforward, and therefore omitted. As described in Section 4, we can similarly identify

27. 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.

gender-specific treatment effects, ATT_M^{AR} and ATT_F^{AR}, under analogues of assumptions A1 and A2 that apply specifically to men and women, as the coefficients of interest from our main gender regressions in Equations 2a and 2b applied to the overlap sample. We refer to these coefficients as β_M^O and β_F^O, respectively.

Remark 2. *One point of possible independent interest is the economic interpretation of ATT^{IR}. While its definition, $\mathbb{E}[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, IRs]$, is mathematically coherent, (potential) outcomes for induced reporters are not observed in the pre-period. However, the standard parallel trends assumption and A1 combine to give the following characterization of ATT^{IR}, and its proof is contained in Appendix A.*

Observation. Under the standard parallel trends assumption and A1, the untreated potential outcomes of IRs equals the untreated potential outcomes of the control group, yielding $ATT^{IR} = \mathbb{E}[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0]$.

First, this observation allows us to interpret ATT^{IR} similarly to the other treatment effects we study: how would the case outcomes of induced reporters have changed compared to that of control-group reporters who were unaffected by #MeToo? Second, this observation allows for direct estimation of ATT^{IR} in an alternative setting and empirical strategy where induced reporters, as opposed to always reporters, can be isolated.

Having established β^O identifies ATT^{AR}, we turn to estimation. 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 $\hat{\beta}^O$, and the third column presents $\hat{\beta}_M^O$ (top row) and $\hat{\beta}^{DDD} = \hat{\beta}_W^O - \hat{\beta}_M^O$ (bottom row). The estimate for overlap cases is $\hat{\beta}^O = 0.087$ (std. err. = 0.013), which is positive and significant at the 1% level. There is gender heterogeneity: $\hat{\beta}_M^O = 0.153$ is significantly larger than $\hat{\beta}_W^O = 0.086$. Figure 5a presents a visual lack of pretrends and a positive effect post-#MeToo; Figure 5b presents a visual lack of pretrends for both genders, and a larger increase in the win rate for men than for women.[28] We show heterogeneity by state political majority in Appendix B.3.

We next conduct several robustness checks that validate our use of overlap cases in this exercise. These exercises provide empirical evidence that overlap cases do not systematically differ from the broader set of always reporter cases, i.e., that overlap complaints are plausibly unselected on foundedness. The final exercise corroborates the filing invariance (A2) assumption.

The first potential threat to overlap representativeness is selection on resolution timing. If long complaints are mechanically more likely to resolve after #MeToo (thereby being classified as overlap) than short complaints, and if duration is correlated with outcome, is our overlap sample biased by over-representation of long cases? Our analysis suggests that this concern is likely to be small. If long versus short (and founded versus unfounded) cases are filed at a given rate in the pre-period, then given a sufficiently long time window before #MeToo, the distribution of time

28. In both panels, estimated effects are larger in $t = 1$ than in $t = 0$. Notice that the probability of case resolution time in any period $t \geqslant 0$ for overlap complaints need not be independent of case characteristics (either foundedness or time of filing). The observed dynamic pattern post-#MeToo is plausibly caused by non-random resolution timing of cases, and thus, there is no observable violation of assumption A2.

since filing for pending cases in each pre-period should be roughly the same.[29] To support this claim, we plot the average duration of open cases in the months leading up to #MeToo in Appendix B.4. The average case duration is relatively consistent, for both sexual harassment and non-sexual harassment cases, suggesting that overlap cases are unlikely to be selected on duration.

The second potential threat to overlap representativeness is if 2017—which all overlap cases necessarily "straddle"—importantly differs from other years for reasons other than #MeToo. To address this, we compare actual 2017 overlap cases to placebo 2016 overlap cases (i.e., cases that would have overlapped had #MeToo occurred in 2016). We find few meaningful differences in case characteristics such as filing season or gender composition between the real and placebo treatment year, adding credibility to the claim that actual overlap cases are not systematically different. For more discussion, see Appendix B.4.

The third potential threat is if treatment effects are not the same for overlap cases as always reporter cases filed after #MeToo (violating A2, the filing invariance assumption). This may be a concern if treatment effects depend on how much of a case's lifecycle occurs before vs after #MeToo (i.e., treatment effects depend on a case's "exposure" to the pre-#MeToo regime), or case filings are seasonal, thus making overlap complaints systematically different from other always reporter cases. To corroborate assumption A2, we re-estimate our overlap specification excluding cases filed in progressively earlier time intervals immediately before October 2017. Because earlier overlap cases mechanically accumulate more exposure to the pre-#MeToo regime by the time of treatment, we would expect a significant change in the estimated coefficient $\hat{\beta}^O$ if A2 were violated. Figure 6 presents these estimates using progressively earlier filing cutoffs. The estimated overlap effect remains close to the baseline across cutoffs, with no evidence of a systematic exposure gradient.[30] This supports the assumption that the overlap estimate is not driven by filing timing or differential exposure to the post-#MeToo adjudication environment. Appendix B.5 provides additional support for this assumption by showing that the win rate for overlap cases remains stable in the period immediately after #MeToo.

7.2 Estimating the share of always reporters, ω

Having estimated ATT^{CE} and ATT^{AR}, in this section we describe the role of ω in evaluating the selection and direct treatment effects.

ω is a latent variable that depends on the number of cases reported before and after #MeToo, and the unobserved distribution of foundedness for reporters both before and after #MeToo (which itself is potentially affected by the rate of realized incidence of sexual harassment). As the second

29. Note that this claim applies only to the pre-period, i.e., we do not require #MeToo to have no effect on case duration.

30. A challenge with considering earlier and earlier cutoffs is that quick-to-resolve overlap complaints are mechanically more likely to be excluded by doing so. For example, only 3.2% of cases are resolved within one month, but 9.5% of cases are resolved within three months. This both means that earlier cutoffs are subject to more selection-on-duration concerns, and that the number of observations increases will be smaller for earlier cutoffs (and drives the larger confidence intervals for our estimates in Figure 6).

component is difficult to observe directly, estimating ω precisely is difficult in our context.

Our qualitative takeaways on the directions of the treatment and selection effect are not sensitive to the value of ω, and depend only on the signs of ATT^{AR} and ATT^{IR}. If $ATT^{AR} > 0$, then the direct treatment effect is positive, and if $ATT^{IR} > ATT^{AR}$ then the selection effect is positive. First, notice that ATT^{AR} mechanically does not depend on ω, meaning that the direct treatment effect is not affected by the share of always reporters. Second, the following ordinal comparisons of ATT^{AR} and ATT^{IR} hold by inspection of Equation 4, meaning that the sign of the selection effect will not depend on ω:[31]

Remark 3. *Fix ATT^{CE} and ATT^{AR}. Then regardless of ω:*

- $ATT^{AR} > ATT^{IR}$ *if* $ATT^{AR} > ATT^{CE}$,
- $ATT^{AR} = ATT^{IR}$ *if* $ATT^{AR} = ATT^{CE}$, *and*
- $ATT^{AR} < ATT^{IR}$ *if* $ATT^{AR} < ATT^{CE}$.

However, the value of ω reveals the relative sizes of the selection and direct effects. ω plays a similar role as the "first stage" in an instrumental variables design that scales the size of the selection effect. A large value of ω—corresponding to a large share of always reporters—"scales up" the magnitude of ATT^{IR} because this effect is being driven by a small fraction $(1-\omega)$ of post-#MeToo cases. The following definition formalizes which effect is larger, and is based on the observation that always reporters are only impacted by the direct effect while induced reporters are impacted by both the direct and selection effects.

Definition 1. *Define the induced-reporter selection component as $ATT^{IR}(\omega) - ATT^{AR}$. We say that the selection effect* dominates *the direct effect at ω if $\left|ATT^{IR}(\omega) - ATT^{AR}\right| \geqslant \left|ATT^{AR}\right|$. Otherwise, we say that the direct effect* dominates *the selection effect at ω.*[32]

Since we do not directly observe ω, we calibrate plausible values using estimates of sexual harassment incidence from Levy and Mattsson 2025. Our preferred, and conservative, calibrated value is $\omega_1 = 0.801$; the derivation of this calibrated value—and a more involved calibration exercise—is detailed in Appendix D.

8 Selection versus Treatment Decomposition Results

We now synthesize the various theoretical and empirical results presented above to answer our two questions of interest: do adjudicators believe complainants more as a result of #MeToo (direct treatment effect), and does #MeToo increase the share of founded accusations (selection effect)? To ensure identification of the relevant treatment effects (see Sections 4 and 7), we assume that the

31. As in Remark 1, the findings of the current remark are unchanged if we replace ATT^{AR} with the ATT for any unselected subset of complainants, and ATT^{IR} with the ATT of the complement of the unselected subset.

32. Equivalently, when $ATT^{IR}(\omega)$ and ATT^{AR} have the same sign, selection dominates direct treatment if $|ATT^{IR}(\omega)| \geqslant 2|ATT^{AR}|$. When $ATT^{IR}(\omega)$ and ATT^{AR} have opposite signs, selection dominates when it overturns the direct effect, i.e., $|ATT^{IR}| - |ATT^{AR}| \geqslant 0$.

standard parallel trends assumption, A1, and A2 hold throughout this section.

8.1 Direct Treatment Effect

In Proposition 1, we proved that the direct treatment effect is revealed by the change in the win rate for always reporters after #MeToo, or ATT^{AR}. That is, Proposition 1 states that adjudicators lower their evidentiary threshold if and only if the always reporter win rate increases. As shown in the previous section, ATT^{AR} is identified by β^O, and represents the change in the win rate of sexual harassment always reporters compared to that of always reporters for control group complaints under our maintained assumptions.

We find evidence that adjudicators lower their evidentiary thresholds in response to #MeToo, i.e., that they "believe" more accusations. This follows because our estimate of ATT^{AR}, $\hat{\beta}^O$, is positive and significant ($\hat{\beta}^O = 0.087$ and is statistically different from zero at the 1% level, see col. 1 of Table 3). In Figure 7a we plot ATT^{AR} across values of ω.

We find adjudicators "believe" more complainants of both genders. For women $\hat{\beta}^O_W = 0.153 - 0.067 = 0.086$, which is again statistically different from zero at the 1% level (see col. 3 of Table 3). For men, the increase is even greater. $\hat{\beta}^O_M = 0.153$ which is both statistically significant at the 1% level and larger than the analogous effect for women. Figure 7b plots these effects by gender.

Interpreting these results in light of Proposition 1 yields the following conclusions:

Finding 1. *#MeToo induced adjudicators to lower the evidentiary threshold for believing sexual harassment complainants. The effect is larger by roughly 50% for men than for women, i.e., adjudicators "believe" sexual harassment complainants of both sexes more after #MeToo, but especially those of male complainants.*

8.2 Selection Effect

Proposition 1 finds that the selection effect is revealed by comparing the win rates of always versus induced reports after #MeToo. Induced reporters are positively selected (for foundedness) if and only if the win rate is higher for induced reporters than always reporters. However, two complications arise when attempting to measure these quantities directly. First, as previously discussed, we do not observe the relative size ω of the always reporters set—we know only that overlap complainants are contained in a set of always reporters. Second, due to the endogeneity of reporting decisions and the mechanical timing bias of overlap cases, directly comparing win rates may misrepresent the direction or magnitude of the effect.[33] The generality of our model—recall that

33. There are a number of complications when using the win rates in the post-#MeToo period to directly answer this question. First, our unselected sample of complaints, the overlap complaints, happen immediately post-#MeToo by construction. Second, our theoretical model does not speak to the following reality: positively versus negatively selected cases may not be filed uniformly at random after October 2017 because the timing of filing a report is endogenously chosen by the complainant. If filing time and case foundedness are correlated, we may naively infer that induced reporters were positively selected for foundedness if we looked at the immediate post-#MeToo period, but that they were negatively selected if we looked farther into the future (or vice versa). For example, consider two potential reporters who decide they want to file complaints immediately after #MeToo. Reporter 1 experienced a founded occurrence of sexual

the selection effect also nests the effect of deterrence (and other factors that change the foundedness distribution), which may take time to affect the actual incidence of sexual harassment—leads to even more complications when trying to directly compare win rates across time.

To sidestep these timing issues, we proceed to study the selection effect by comparing treatment effects for always and induced reporters. Of course, the sign of the difference in *treatment effects* ($ATT^{IR} - ATT^{AR}$) may not be the same as the sign of the difference in *win rates* ($\mathbb{E}[Y_{i0}(1)|D_{i0} = 1, IR] - \mathbb{E}[Y_{i0}(1)|D_{i0} = 1, AR]$). To address this possibility, the following proposition states an equivalence between the ATT and post-period win rates of always versus induced reporters: under an additional assumption, ATT^{IR} is higher than ATT^{AR} if and only if post-period win rate of induced reporters is higher than the post-period win rate of always reporters.

Proposition 3. *Suppose standard parallel trends, A1, and the following* No Extreme Baseline Gap in Potential Outcomes *condition hold:* $\left|\mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, IR] - \mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, AR]\right| \leqslant \left|\mathbb{E}[Y_{i0}(1)|D_{i0} = 1, IR] - \mathbb{E}[Y_{i0}(1)|D_{i0} = 1, AR]\right|$*, or that the gap in (untreated) potential outcomes between IRs and ARs is magnified by #MeToo. Then* $ATT^{IR} \geqslant ATT^{AR}$ *if and only if* $\mathbb{E}[Y_{i0}(1)|D_{i0} = 1, IR] \geqslant \mathbb{E}[Y_{i0}(1)|D_{i0} = 1, AR]$*, or that the post-#MeToo win rate of IR sexual harassment complaints exceeds that of the post-#MeToo win rate of AR sexual harassment complaints.*

The no extreme baseline gap condition is satisfied when the evidentiary bar for winning is sufficiently high pre-#MeToo and is lowered as a result of #MeToo.[34] Naturally, the conclusions of Proposition 3 hold within gender as well, that is, conditioning the no extreme baseline gap restriction on a gender g implies that $ATT_g^{IR} \geqslant ATT_g^{AR}$ if and only if the post-#MeToo win rate of IR sexual harassment complaints by gender g exceeds that of the post-#MeToo win rate of AR sexual harassment complaints by gender g. Recall that the benefit of considering treatment effects rather than raw win rates is that our estimates are valid regardless of the endogeneity of filing timing after #MeToo. However, in Appendix B.6 we conduct regression discontinuity analyses on raw win rates immediately in the wake of #MeToo that corroborate our gender-specific findings below.

To address the issue of ω being a latent variable, we consider the point estimates of treatment effects $\hat{\beta}^O$ (which identifies ATT^{AR}) and $\hat{\beta}^{CE}$ (which identifies the combined ATT) as a function of ω to reveal the ATT of induced reporters by using Equation 4.

We find evidence that the selection effect of #MeToo is positive, that is, induced reporters bring forth founded claims of sexual harassment. However, there is significant gender heterogeneity.

Pooling across genders, we see that $\hat{\beta}^{CE} = 0.101$ exceeds $\hat{\beta}^O = 0.087$ (see col. 1 of Tables 2 and 3), i.e., that ATT^{IR} exceeds ATT^{AR} (see Remark 1 and Proposition 2). In Figure 7a, we

harassment in the pre-period, but chose not to make a report at the time. However, given the increase in (perceived) win probability post #MeToo, she is incentivized to do so immediately. By contrast, Reporter 2 never experienced sexual harassment, but decides he wants to file an unfounded report now that he perceives a higher chance of winning, and files his accusation several months or years after #MeToo.

34. This claim follows from our theoretical framework presented in Section 6. The smoothness conditions on distributions G_F and G_U imply that for a sufficiently high evidentiary threshold in the pre-period ($\underline{e}^{-1} \approx 1$), the win rate for both founded and unfounded cases is $1 - G_F(\underline{e}^{-1}) \approx 1 - G_U(\underline{e}^{-1}) \approx 0$, implying that the baseline gap in potential outcomes in the pre-period converges to 0 as the evidentiary threshold converges to 1.

trace out the treatment effects for induced reporters for different values of ω using Equation 4. The treatment effects for induced reporters exceed those for always reporters under every value of ω (as predicted by Remark 3). For women, the pattern is the same as the pooled effects: $\hat{\beta}_W^{CE} = 0.119 + 0.004 = 0.123$ is larger than $\hat{\beta}_W^{O} = 0.153 - 0.067 = 0.086$ (see col. 3 of Tables 2 and 3). Figure 7b shows that treatment effect for female induced reporters exceeds the treatment effect for female always reporters for all values of ω (orange lines). For men, $\hat{\beta}_M^{CE} = 0.119$ is smaller than $\hat{\beta}_M^{O} = 0.153$ (see col. 3 of Tables 2 and 3). The treatment effect for male induced reporters is *lower* than that for male always reporters (blue lines of Figure 7b) for all values of ω. Combining these observations with Propositions 1 and 3 yields the following conclusions:

Finding 2. *#MeToo induced more founded discrimination complaints to be filed. However, there is significant gender heterogeneity in the foundedness of induced reporters: women are positively selected for foundedness, while men are negatively selected for foundedness.*

Recalling that the share of always reporters ω acts as a "first stage" that scales the selection effect, our calibrated values offer guidance on which of the selection or direct treatment effects dominates the other. For our calibrated value of ω (and for any larger values), we find that the selection effect dominates the direct treatment effect for both men and women. The treatment effect for male induced reporters is *negative*: the negative selection effect for men is so large that it overturns the positive adjudicator treatment effect. For women, the selection effect and direct treatment effect reinforce one another to increase the win rate, but the selection effect contributes more to the aggregate effect:

Finding 3. *If the share of always reporters ω exceeds 0.778, then the selection effect dominates for men. Male induced reporters have a lower win rate than male always reporters in a counterfactual world where #MeToo never happened. In other words, men induced to report by #MeToo won less often than men who reported before #MeToo, despite benefiting from more lenient adjudicators. Moreover, if ω exceeds 0.570, then the selection effect dominates for women. Women induced to report by #MeToo had a treatment effect more than twice as large as women always reporters.*

Note, as previously discussed, we investigate the robustness of our results to alternative data samples, alternative specifications, and alternative control groups in Appendix C. Broadly speaking, our main empirical takeaways are qualitatively and quantitatively similar across these alternatives.

9 Discussion and Conclusion

Are marginal reporters of sexual harassment more likely than inframarginal reporters to bring forth founded accusations? This question drives much of the public debate surrounding the #MeToo movement. However, reduced-form estimates alone are insufficient to answer it, since campaigns that affect reporting decisions may simultaneously shift how adjudicators weigh evidence.

In a newly constructed dataset of US sexual harassment complaints, we show that #MeToo made adjudicators more inclined to "believe" accusers, and while women induced to report were more

likely to bring forth founded accusations, men induced to report were less likely to bring forth founded accusations. More broadly, we contribute a framework for disentangling treatment and selection effects in settings where there is a delay between filing and resolution.

What might explain these gendered patterns in selection? Recall that a complaint's foundedness captures both whether it is true *and* whether it meets the legal definition of sexual harassment. Research suggests sexual wrongdoing is rarely falsely reported (Lisak et al. 2010; Heenan and Murray 2006); in light of this, one interpretation is that our results primarily reflect gender differences in complainants' ability to meet this threshold after #MeToo. For example, if women have a better understanding than men of what constitutes sexual harassment, women induced to report by #MeToo may bring more successful cases relative to their male counterparts. A second interpretation is that these selection patterns could also reflect different correlations between reporting costs and foundedness. If foundedness and reporting cost are positively correlated for women but negatively correlated for men (perhaps because of different stigma costs and threats of retaliation), then the marginal male complaint prior to #MeToo would likely be less founded. Due to the relatively sparse information we observe about each individual case, we depend on future researchers to address these questions.

However, we can perhaps shed light on mechanisms by examining how our estimates change with complainants' work environments. In Appendix C, we explore firm-level and industry-level differences. We classify employers as Fortune 500 companies (versus not) and as operating in male-dominated industries (versus not) based on their name. Effects are larger for complainants employed by Fortune 500 companies than our main estimates, suggesting that adjudicators' responses and complainant behavior are "scaled up" in large firms. However, we find no significant differential effect for complainants employed in male-dominated industries. This suggests that #MeToo's influence on reporting and believing operated more through firm-level differences rather than industry-level norms.

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Tables & Figures

FIGURE 1 — GOOGLE TRENDS TIMESERIES OF #METOO SEARCHES

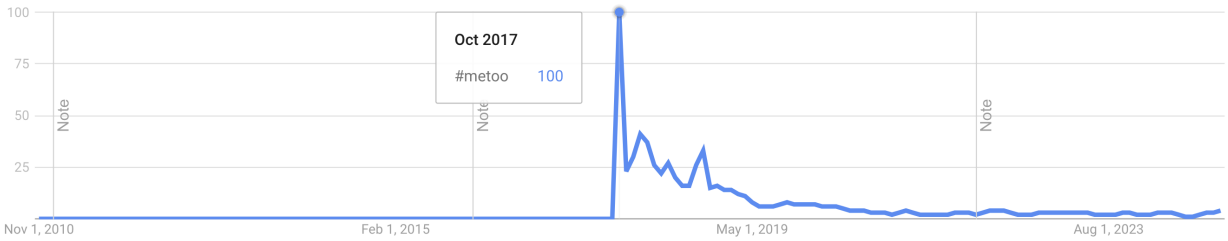


Figure 1: Google Trends timeseries of #MeToo searches from 2010 to 2024, showing a peak in October 2017. The y-axis is Google's measure of the relative popularity of the search term over time, with 100 being peak popularity.

FIGURE 2 — DIAGRAM OF COMPLAINT PROCESS

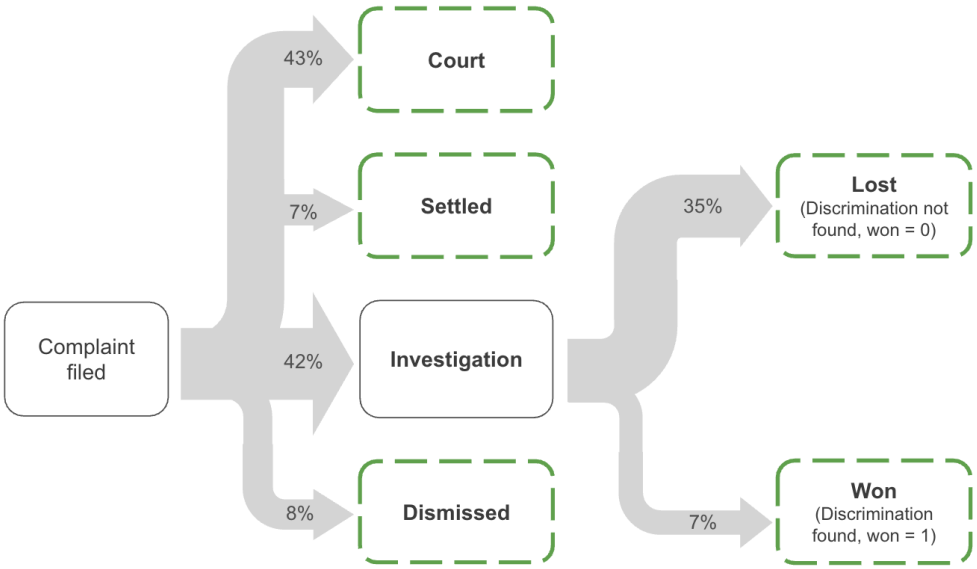


Figure 2: Complaint flow with terminal outcomes in boxes with green dashed lines. All terminal boxes add to 100%. *Won* and *Lost* are unconditional shares of all filed complaints. The conditional win rate among investigated cases equals #Won/(#Won+#Lost).

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: 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. *Won* and *Lost* are unconditional shares of all filed complaints. The conditional win rate among investigated cases equals #Won/(#Won+#Lost). Columns 3 and 4 present the difference in means of each variable after minus before #MeToo, with the standard error of the difference-in-means test in parentheses. Sexual harassment can be classified as Retaliation in the data, so Retaliation is non-zero in the "SH only" column.

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

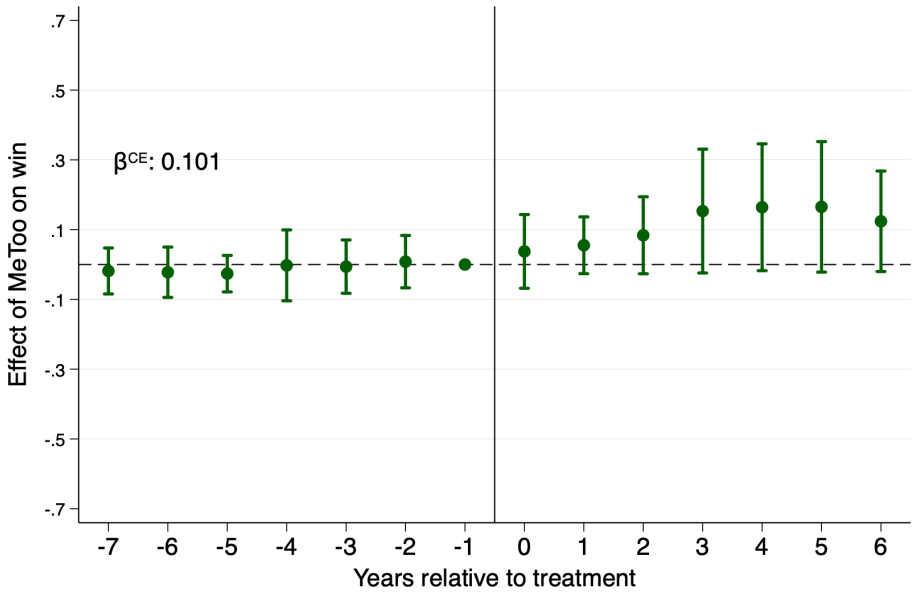


FIGURE 3b — IMPACT OF #METOO ON PROBABILITY COMPLAINANT WINS, BY GENDER

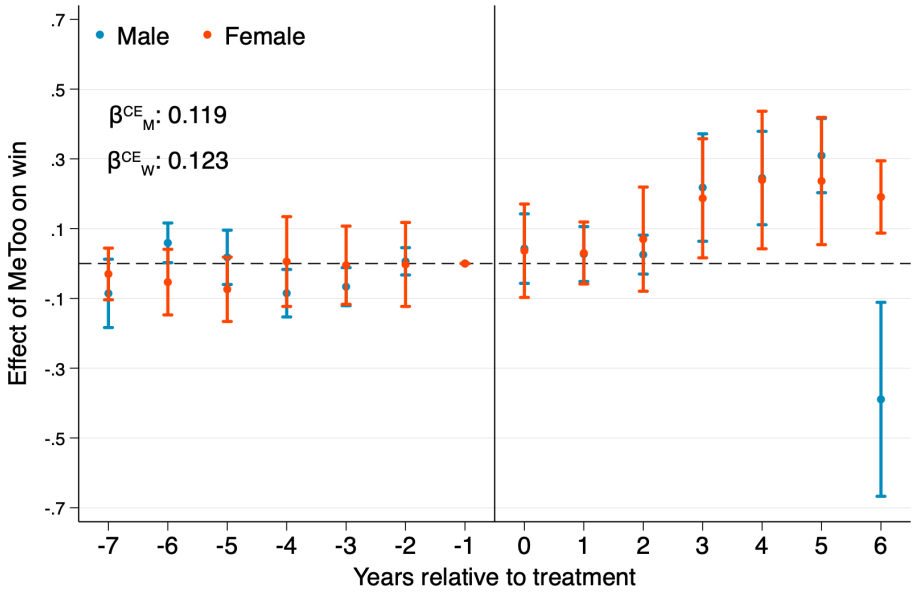


Figure 3: These figures provide estimates of Equations 1b and 2b. The x-axis in all subfigures represents 12-month intervals centered around the #MeToo start date. $t = -1$ is the 12 months preceding October 15, 2017, $t = 0$ is the 12-month period following October 15, 2017, and so on. In the top panel, we control for complaint type-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. 95% confidence intervals are reported using standard errors clustered at the complaint type by state level.

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***
	(0.029)	(0.027)	(0.026)
SH × Post × Female			0.004
			(0.022)
{Unit, Time} × State FE	✓	✓	✓
{Unit, 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: Columns 1 and 2 present estimates of Equation 1a. Column 3 presents estimates of Equation 2a. Fixed effects are complaint type by state and year-month of case resolution by state. For the triple differences specification, the fixed effects are complaint type by state by female complainant and year-month of case resolution by state by female complainant. Standard errors clustered at the state by complaint type level. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

FIGURE 4 — ILLUSTRATION OF 2×2 COMPARISON IN OVERLAP REGRESSIONS

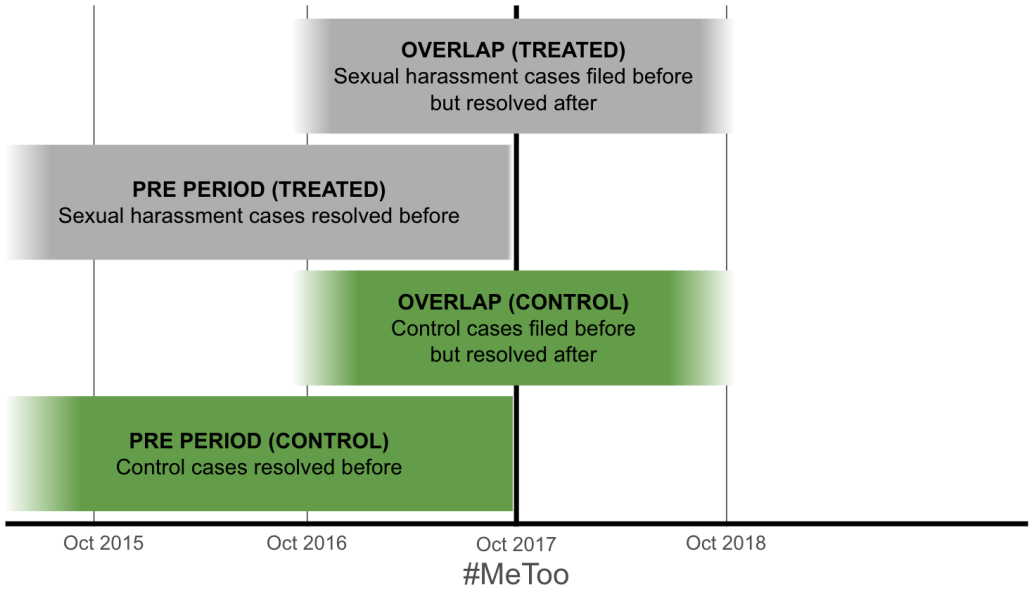


Figure 4: Stylized illustration of the two-by-two comparison that our difference-in-differences regression for overlap cases is making.

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***
	(0.013)	(0.011)	(0.014)
SH × Post × Female			-0.067**
			(0.027)
{Unit, Time} × State FE	✓	✓	✓
{Unit, 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: 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 state by complaint type (unit) level. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

FIGURE 5a — IMPACT OF #METOO ON PROBABILITY COMPLAINANT WINS, OVERLAP

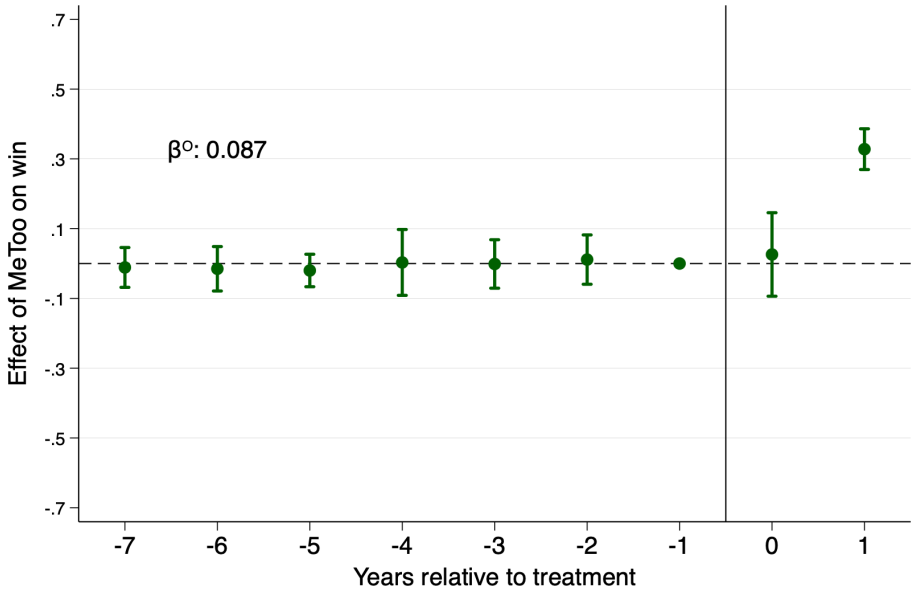


FIGURE 5b — IMPACT OF #METOO ON PROBABILITY COMPLAINANT WINS BY GENDER, OVERLAP

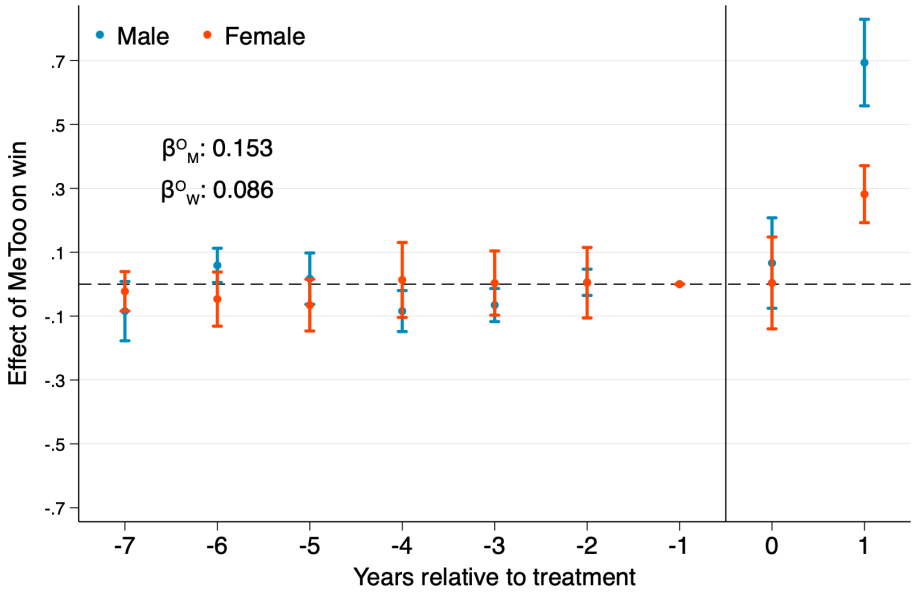


Figure 5: These figures provide estimates of Equations 1b and 2b for the subset of cases that are filed before #MeToo (the overlap sample from Figure 4). The x-axis in all subfigures represents 12-month intervals centered around the #MeToo start date. $t = -1$ is the 12 months preceding October 15, 2017, $t = 0$ 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. 95% confidence intervals are reported using standard errors clustered at the complaint type by state level.

FIGURE 6 — TREATMENT EFFECTS FOR OVERLAP CASES WITH DIFFERENTIAL EXPOSURE TO PRE-#METOO REGIME

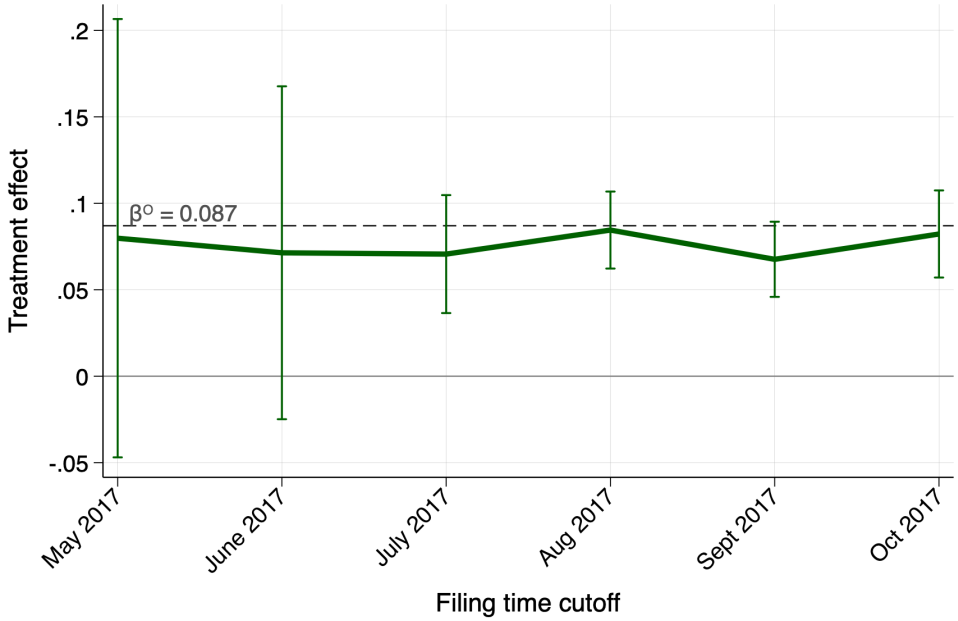


Figure 6: This figure presents estimates of β^O under various filing time cutoffs. For each month presented along the horizontal axis, we re-estimate Equation 1a, restricting to cases filed before that month. Thus, the final estimate corresponds to our baseline overlap specification, and each earlier cutoff represents estimates dropping cases that accumulated less time before treatment. 95% confidence intervals are indicated. $\hat{\beta}^O$ is stable across cutoffs, providing corroboration for the filing invariance assumption (A2).

FIGURE 7a — ATT^{AR} AND ATT^{IR} BY SHARE OF ALWAYS REPORTERS

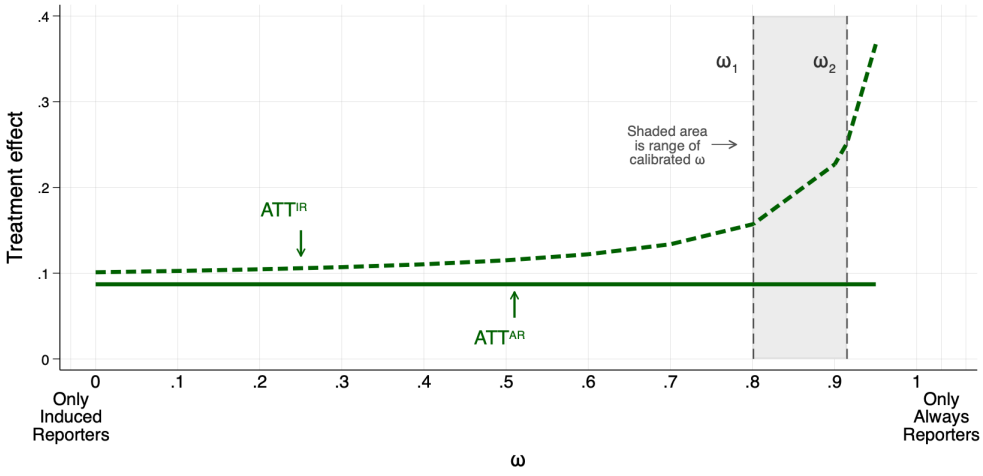


FIGURE 7b — ATT^{AR} AND ATT^{IR} BY GENDER

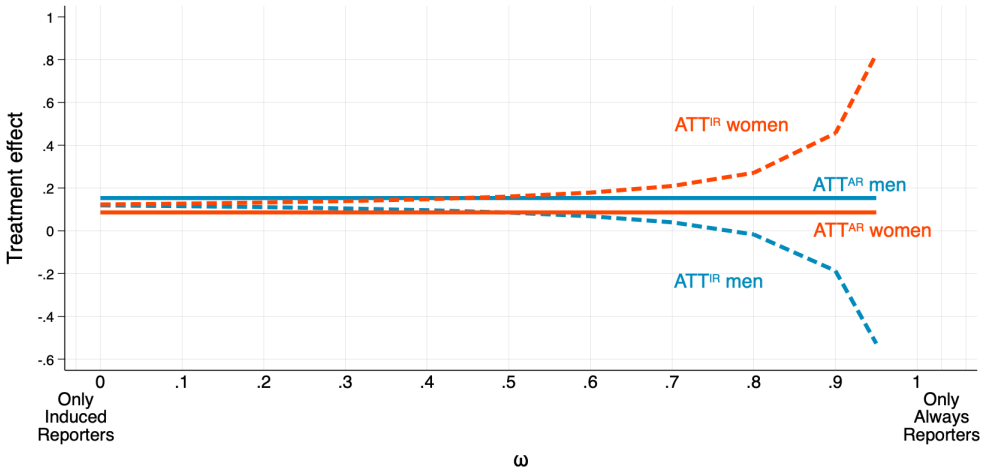


Figure 7: This figure presents the treatment effects for always and induced reporters as a function of the share of always reporters ω. The treatment effect for always reporters is constant across all values of ω. The treatment effect for induced reporters asymptotes at $\omega = 1$, so the graphs are bounded at $\omega = 0.95$. The top panel presents our pooled results, and indicates our calibrated estimate ω_1 (and an additional calibration ω_2, derived in Appendix D). The bottom panel disaggregates results by complainant gender.

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 a case's foundedness. Therefore, the win rate among always reporters (in each period t) is given by:

$$W_{AR}^t := \frac{|r_F^0| \cdot Pr(e \geqslant \underline{e}^t | \tau = F) + |r_U^0| \cdot Pr(e \geqslant \underline{e}^t | \tau = U)}{|r_F^0| + |r_U^0|} = \frac{|R_F^{-1}| \cdot Pr(e \geqslant \underline{e}^t | \tau = F) + |R_U^{-1}| \cdot Pr(e \geqslant \underline{e}^t | \tau = U)}{|R_F^{-1}| + |R_U^{-1}|}$$

where the equality follows from the definition of always reporters. By the properties of probability distributions, $Pr(e \geqslant \underline{e}^0 | \tau = F) \geqslant Pr(e \geqslant \underline{e}^{-1} | \tau = F)$ and $Pr(e \geqslant \underline{e}^0 | \tau = U) \geqslant Pr(e \geqslant \underline{e}^{-1} | \tau = U)$ if and only if $\underline{e}^0 \leqslant \underline{e}^{-1}$. Therefore, $W_{AR}^0 \geqslant W_{AR}^{-1}$ 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_F dominates G_U in the likelihood ratio order, it follows that $Pr(e \geqslant \underline{e}^0 | \tau = F) \geqslant Pr(e \geqslant \underline{e}^0 | \tau = U)$ for any given $\underline{e}^0 \in [0, 1]$. □

Proof of Observation contained in Remark 2

Proof. Recall the *standard parallel trends assumption*:

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

and Assumption A1:

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

Because each reporter in the post period is either an always reporter (AR) or an induced reporter (IR) set, and ω is the share of ARs, the Law of Iterated Expectations implies that:

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

Then it holds that:

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

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 $\mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0] = \omega\mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 0] + (1-\omega)\mathbb{E}[Y_{i0}(0) - Y_{i-1}(0)|D_{i0} = 1, IRs]$. Combining the definition of ATT^{IR} and Equation 8 yields the desired conclusion.

□

Proof of Proposition 3

Proof. The standard parallel trends assumption and A1 implies the following, as derived in Equations 6 and 8:

$$ATT^{IR} - ATT^{AR} = \mathbb{E}[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i0}(1) - Y_{i-1}(0)|D_{i0} = 1, ARs]. \tag{9}$$

First, we show that the sign of $ATT^{IR} - ATT^{AR}$ reveals whether IRs or ARs have a higher expected win rate in the post period.

Suppose $ATT^{IR} - ATT^{AR} \geqslant 0$. Rearranging Equation 9:

$$\mathbb{E}[Y_{i0}(1)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i0}(1)|D_{i0} = 1, ARs] \geqslant \mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, ARs]. \tag{10}$$

But then Equation 10 and the no extreme baseline gaps condition are both satisfied only if $\mathbb{E}[Y_{i0}(1)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i0}(1)|D_{i0} = 1, ARs] \geqslant 0$, proving the desired claim.

Similarly, suppose $ATT^{IR} - ATT^{AR} \leqslant 0$. Rearranging Equation 9:

$$\mathbb{E}[Y_{i0}(1)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i0}(1)|D_{i0} = 1, ARs] \leqslant \mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, ARs]. \tag{11}$$

But then Equation 11 and the no extreme baseline gaps condition are both satisfied only if $\mathbb{E}[Y_{i0}(1)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i0}(1)|D_{i0} = 1, ARs] \leqslant 0$, proving the desired claim.

Second, we show that whether IRs or ARs have a higher expected win rate in the post period reveals the sign of $ATT^{IR} - ATT^{AR}$.

Suppose the win rate of IRs exceeds that of ARs in the post period, i.e., $\mathbb{E}[Y_{i0}(1)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i0}(1)|D_{i0} = 1, ARs] \geqslant 0$. Then the MLRP property (see the proof of Proposition 1) implies that the (potential) win rate of IRs exceeds that of ARs in the pre period as well, $\mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, ARs] \geqslant 0$. We wish to show that $ATT^{IR} - ATT^{AR} \geqslant 0$, which is equivalent to showing that Equation 10 holds. But note that $\mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, ARs] \geqslant 0$ and the no extreme baseline assumption together imply Equation 10 holds, as desired.

Similarly, suppose the win rate of ARs exceeds that of IRs in the post period, i.e., $\mathbb{E}[Y_{i0}(1)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i0}(1)|D_{i0} = 1, ARs] \leqslant 0$. Then $\mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, ARs] \leqslant 0$. We wish to show that $ATT^{IR} - ATT^{AR} \leqslant 0$, which is equivalent to showing that Equation 11 holds. But note that $\mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, IRs] - \mathbb{E}[Y_{i-1}(0)|D_{i0} = 1, ARs] \leqslant 0$ and the no extreme baseline assumption together imply Equation 11 holds, as desired.

□

Online Appendix

B Additional Tables and Figures

In this section, we summarize the additional analyses conducted as referenced in the body of the paper. All relevant tables and figures for Appendix sections B.1-B.6 are presented at the end of Appendix B.

B.1 Data from FOIA Requests

In both Section 3 and Appendix F, we describe the process of obtaining discrimination complaint data from US states. Here, we present a collection of tables and figures that visualize this data and provide clarification on how complaints were categorized as sexual harassment versus control complaints.

Table A1 summarizes data from states that provided a substantial amount of relevant discrimination complaints that met sample restrictions for our main analysis (i.e., employment discrimination only). For each of these states, we can observe what percentage of complaints provided were classified as sexual harassment, as well as other statistics—such as the win rate across supplied complaints—calculated from the data when possible.

Figure A1, reproduced from Raj et al. 2024, provides many examples of sexual harassment that are consistent with the EEOC's definition, as discussed in Section 2.2 of the main body, and would therefore be classified as sexual harassment cases in our analysis. More serious instances, such as the example in the Sexual Assault category, may be charged as criminal offenses and, therefore, would not be included in our dataset. More information on this can be found in Section 2.3.

Figure A2 plots the number of sexual harassment complaints (top) and other complaints (bottom) filed over time. Note the larger increase in sexual harassment complaints between 2017 (the year that #MeToo began) and 2019, compared to other complaints. This suggests that the increase in sexual harassment complaints was driven by the movement and was not just reflective of a broader trend.

B.2 Secondary Outcomes

While a main focus of our research is on how #MeToo impacts the win rate of sexual harassment complaints, regressions on secondary outcomes—rates of settlement and going to court—provide additional insights into the overall effects of this social movement. Section 5 of the main body presents conclusions from our win rate analysis; there is little evidence of a pre-trend, #MeToo has a positive and significant impact on complainant win probability, and female complainants do not benefit more from #MeToo than their male counterparts. The following exhibits allow for similar types of conclusions to be drawn regarding settlement rates and the likelihood of going to court.

Figure A3a shows an event study graph for settlement rates, and Figure A3b shows an event study for the probability of exiting to court. Both event studies demonstrate little evidence of a pre-trend. Unlike with win rate, #MeToo has a negligible effect (β^{CE} = 0.001) on settlement likelihood. The movement has a slight positive effect on the probability of proceeding to court (β^{CE} = 0.007), with a transient spike at time $t = 0$. However, this effect is not statistically significant.

Table A2 presents estimates of equations 1a and 2a on settlement rates and the likelihood of exiting to court. As stated previously, #MeToo has essentially no effect on complaint settlement probability (β^{CE} = 0.001). The outcomes for men and women do not notably differ, as both coefficients are near zero (β^{CE}_{M} = 0.001 and β^{CE}_{W} = -0.002). Similarly, #MeToo has a positive but insignificant impact on the probability of exiting to court (β^{CE} = 0.007), and outcomes across genders are consistent (β^{CE}_{M} = 0.002 and β^{CE}_{W} = 0.009).

B.3 State political majority

Prior literature has found that the positive effects of the #MeToo movement for victims are concentrated in politically liberal places (Levy and Mattsson 2025). In this appendix section, we examine the heterogeneity of our main results by state. We proceed by bisecting the states in our analysis based on their voting history in recent presidential elections (see Table A1 for details of the classification). We run our event study specification in Equation 1b separately, estimating both $\hat{\beta}^{CE}$ and $\hat{\beta}^{O}$.

We find that our results are driven entirely by blue (i.e., liberal) states, and that the combined effect of #MeToo in red and purple (respectively, conservative and contested) states is virtually zero. Figure A4 presents these event studies for blue states (top panel, $\hat{\beta}^{CE} = 0.112$), and for red and purple states (bottom panel, $\hat{\beta}^{CE} = -.004$). This pattern holds in the overlap sample as well, with overlap effects virtually zero in red and purple states but similar in magnitude and significance to our main results in the blue states. Figure A5 presents the overlap effect across these two groups. $\hat{\beta}^{O} = 0.097$ in blue states and $\hat{\beta}^{O} = -.007$ in red and purple states, compared to $\hat{\beta}^{O} = 0.087$ in our full sample, again highlighting that our results are being driven by blue states. Our findings are nearly unchanged if we instead group purple states with blue states.

B.4 Overlap and Always Reporter Cases

Section 7 discusses the sufficient conditions needed to effectively decompose β^{CE}. One of these is that overlap cases are representative of always reporter cases (see Section 7.1). Here, we demonstrate why we believe this is justified.

First, we claim in Section 7.1 that our overlap analysis is unlikely to be affected by selection on duration in a way that biases our results. Figure A6 plots the average duration of cases that are open in a given month in the year preceding #MeToo, for both sexual harassment and non-sexual harassment cases. The trend is relatively flat month-to-month suggesting that the distribution of

case durations is relatively stable before #MeToo.

Second, we present summary statistics for overlap and "near" overlap cases. Table A3 compares actual overlap cases to placebo cases that would have been classified as overlap if #MeToo had begun in 2016, based on six (exogenous-to-adjudicators) characteristics. The differences between the actual and placebo cases, while statistically significant in many outcomes, are likely economically small, lending support to overlap cases being broadly representative of always reporter cases.

B.5 Exposure and Filing Invariance Assumption

Another condition we need to effectively decompose β^{CE} is the filing invariance assumption (A2). This assumption requires treatment effects for always reporters to remain stable over time. We have already presented Figure 6 to verify that treatment effects for overlap cases filed at different time cutoffs before #MeToo are indistinguishable from our baseline estimate.

Here, we present additional descriptive evidence to support this assumption. Figure A7 shows the win rate for overlap cases, which represent always reporter cases, for the six months immediately following the start of #MeToo. The steady lines indicate that the win rate remained relatively constant, providing complementary evidence of post-#MeToo consistency.

B.6 Regression Discontinuity

In Section 8, we discuss how directly comparing win rates for cases filed (just) before and (just) after #MeToo may misrepresent the direction or magnitude of the selection effect. As previously discussed, our difference-in-difference analysis, using data from the entire pre- and post-periods, finds that female induced reporters are positively selected for foundedness while male induced reporters are negatively selected. However, the share of founded cases filed post-#MeToo may differ over time. In this section, we present regression discontinuity (RD) analyses that suggest that the "immediate" selection effect directionally matches the overall selection effect for both genders.

In each RD design, we restrict to sexual harassment cases resolved after #MeToo (but filed either before or after). The running variable is the time of case filing. Therefore, the RD effect is the difference in win rate between always reporters (left of the discontinuity) and always *plus* induced reporters (right of the discontinuity). Recalling Remark 1, the sign of the RD coefficient is the same as the sign of the hypothetical RD coefficient if we could restrict to only induced reporters in the post-period. A positive RD effect is therefore consistent with $ATT^{IR} > ATT^{AR}$, and vice versa. Recalling that we do not observe the share of induced reporters in the post period $(1 - \omega)$, the magnitude of these effects should not be literally interpreted, and indeed, are mechanically a lower bound on the actual effects—our effects are unbiased if and only if $\omega = 0$.

In Table A4, we present estimates of how the win rate changes around the #MeToo discontinuity for men, and for women. Each row represents a different specification, varying the fixed effects and the polynomial order. All estimates for men are negative—consistent with male IRs being

negatively selected—and all estimates for women are positive (consistent with female IRs being positively selected). In Figure A8, we plot our preferred RD specification using state by time fixed effects and polynomial of order two, to visually confirm the direction of the point estimates.

TABLE A1 — DATA SUMMARY BY STATE FOR EMPLOYMENT DISCRIMINATION COMPLAINTS

State	N	Sexual harassment	Complainant won	Female complainant
Panel A: Employment Sample (Main Analysis)				
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	.
FL	5,110	.005	.050	.
AK	2,028	.133	.067	.
KY	1,862	.028	.007	.
ND	1,801	.165	.024	.
HI	1,756	.100	.002	.
N	191,985	191,978	74,029	64,499
Panel B: Employment Sample + CT (Robustness Check)				
CA	90,220	.247	.000	.
NY	48,183	.144	.270	.491
CT	20,794	.101	.040	.548
MA	17,540	.073	.116	.522
WI	16,135	.064	.	.
MI	7,350	.043	.0004	.
FL	5,110	.005	.050	.
AK	2,028	.133	.067	.
KY	1,862	.028	.007	.
ND	1,801	.165	.024	.
HI	1,756	.100	.002	.
N	212,779	212,772	77,934	85,001
Panel C: All data (Robustness Check)				
CA	118,304	.192	.025	.
NY	73,156	.100	.271	.530
IL	33,936	.069	.092	.555
MA	25,683	.057	.131	.562
CT	22,837	.092	.047	.559
WI	21,348	.050	.	.
MI	15,998	.025	.0004	.
FL	9,094	.004	.081	.
WA	8,509	.046	.003	.
TX	4,658	.016	.000	.
MT	4,028	.084	.152	.760
HI	2,966	.064	.013	.
KY	2,935	.021	.068	.
AK	2,836	.105	.037	.
ND	2,695	.113	.111	.
SC	1,245	.031	.051	.684
NC	643	.	.189	.
DE	351	.020	.632	.
N	351,222	350,572	151,572	155,145

Notes: This table presents means of variables describing discrimination complaints across three samples. Panel A corresponds to our main estimation sample with 10 states and contains only employment discrimination. Panel B corresponds to the sample we use in a robustness check with 11 states, and this sample includes the main sample plus complaints from the state of Connecticut which include employment and public accommodation complaints. Panel C corresponds to all 18 states that provided data containing all types of discrimination, including employment, public accommodation, housing, and education. See Appendix F for more details on our FOIA request process. N indicates the number of non-missing observations. 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. We used Genderize.io 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.

FIGURE A1 — DEFINITIONS OF SEXUAL WRONGDOING

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 makes you feel unsafe.
- Someone calling you a sexist slur, like "Bitch," "Slut," "Cunt," or "Whore".
- Someone purposefully 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.

CYBER SEXUAL HARASSMENT

- Someone sending you sexual content without your permission, sexualizing you inappropriately, or threatening you with sex acts, in *private online spaces*, such as over e-mail or text.
- Someone sending you sexual content without your permission, sexualizing you inappropriately, or threatening you with sex acts, in *public online spaces*, such as on social media.
- Someone taking and/or sharing sexual pictures or videos of you without your permission.

DATING COERCION OR SEXUAL THREATS

- 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 texting or calling you in a harassing way.
- Someone repeatedly asking you for a date or your phone number when you've said no or ignored them.

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 didn't want to do (including while you were under the influence of alcohol or drugs).

Figure A1: Definitions of different types of sexual wrongdoing, taken from Raj et al. 2024.

FIGURE A2 — NUMBER OF COMPLAINTS FILED OVER TIME

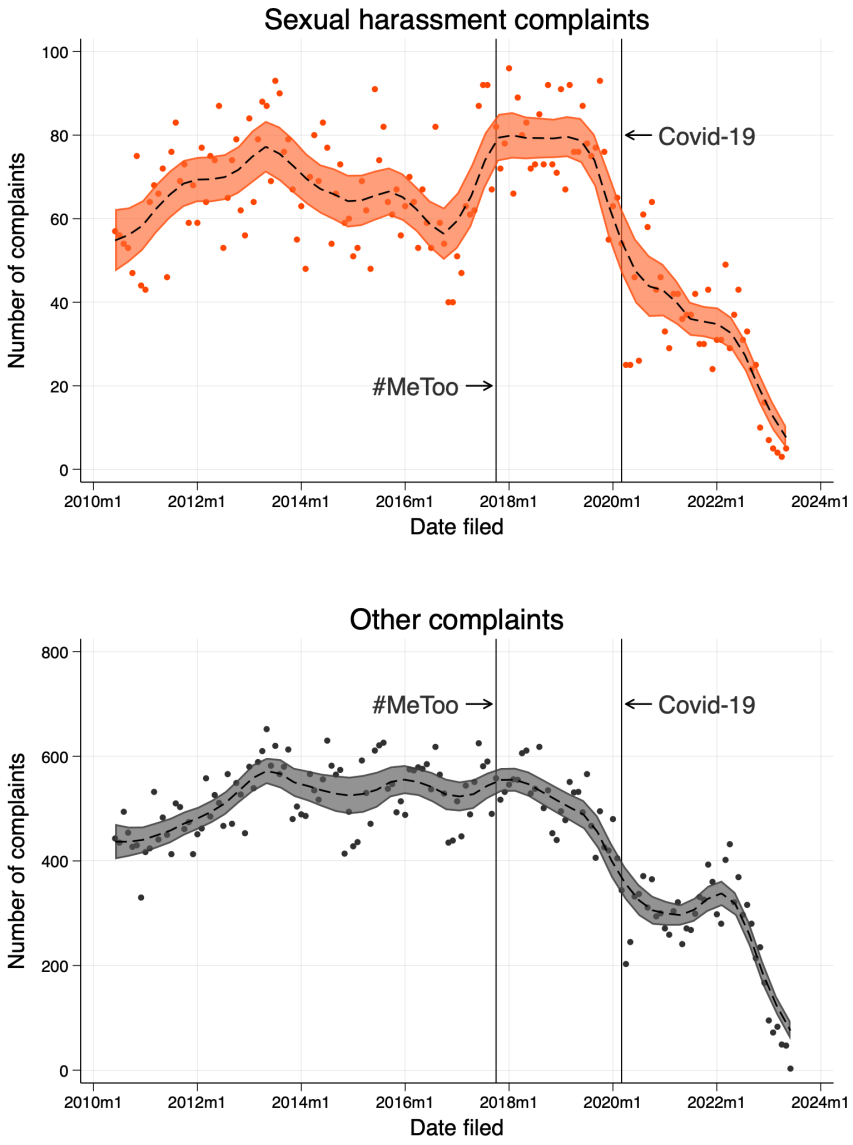


Figure A2: The figure shows the number of employment discrimination complaints filed over time, excluding California, Florida, and Wisconsin. These states are omitted because data for them begin only in early 2017, which would otherwise create an artificial increase in filings following the treatment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

FIGURE A3a — IMPACT ON SETTLEMENT

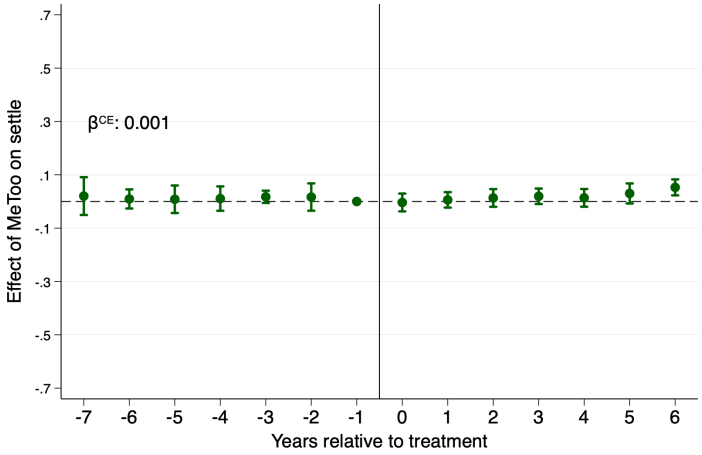


FIGURE A3b — IMPACT ON EXITING TO COURT

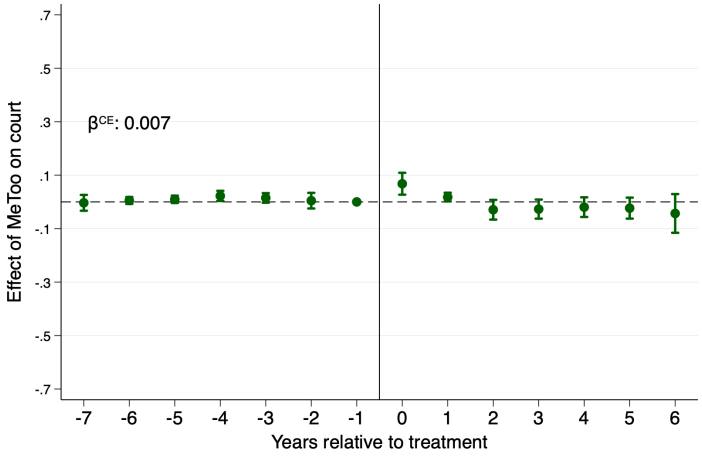


Figure A3: These figures provide estimates of Equation 1b. The x-axis in all subfigures represents 12-month intervals centered around the #MeToo start date. $t = -1$ is the 12 months preceding October 15, 2017, $t = 0$ is the 12-month period following October 15, 2017, and so on. We control for unit-by-state and year-month-by-state fixed effects, where the "year-month" specifies the date of the complaint resolution. 95% confidence intervals are reported using standard errors clustered at the complaint type by state level.

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)	(0.009)	(0.006)	(0.007)	(0.012)
SH × Post × Female			-0.003			0.007
			(0.009)			(0.009)
{Unit, Time} × State FE	✓	✓	✓	✓	✓	✓
{Unit, 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: This table replicates Table 2, but where the outcome variable is replaced by whether a case is settled (left panel) or goes to court (right panel). Time fixed effects are year-month of case resolution. Standard errors clustered at the state-by-complaint-type level. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

FIGURE A4 — HETEROGENEITY OF AGGREGATE RESULTS BY STATE POLITICAL MAJORITY

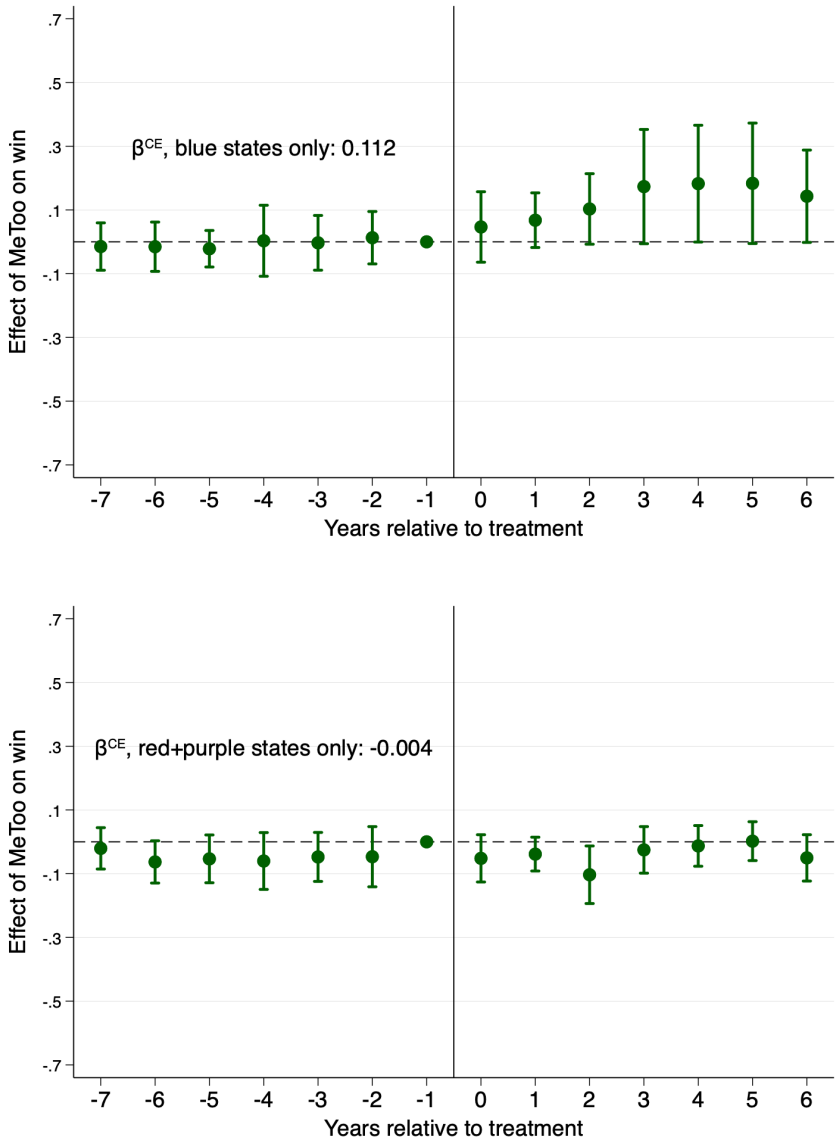


Figure A4: The figure plots the results of Equation 1b estimated on blue states (top panel), and red and purple states combined (bottom panel). Table A1 describes our classification of states into red, blue, and purple.

FIGURE A5 — HETEROGENEITY OF OVERLAP RESULTS BY STATE POLITICAL MAJORITY

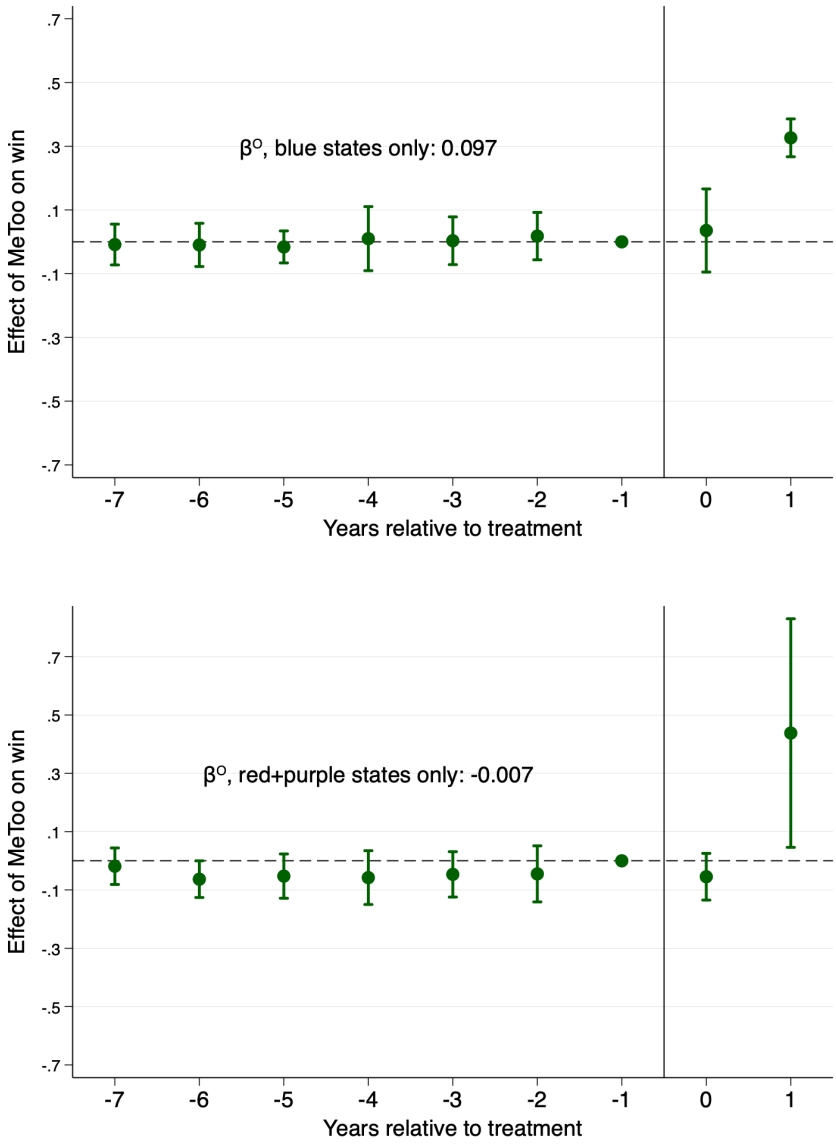


Figure A5: The figure plots the results of Equation 1b estimated on the group blue states (top panel), and red and purple states combined (bottom panel) for overlap complaints. Table A1 describes our classification of states into red, blue, and purple.

FIGURE A6 — AVERAGE DURATION OF CASES OPEN IN EACH MONTH BEFORE #METOO

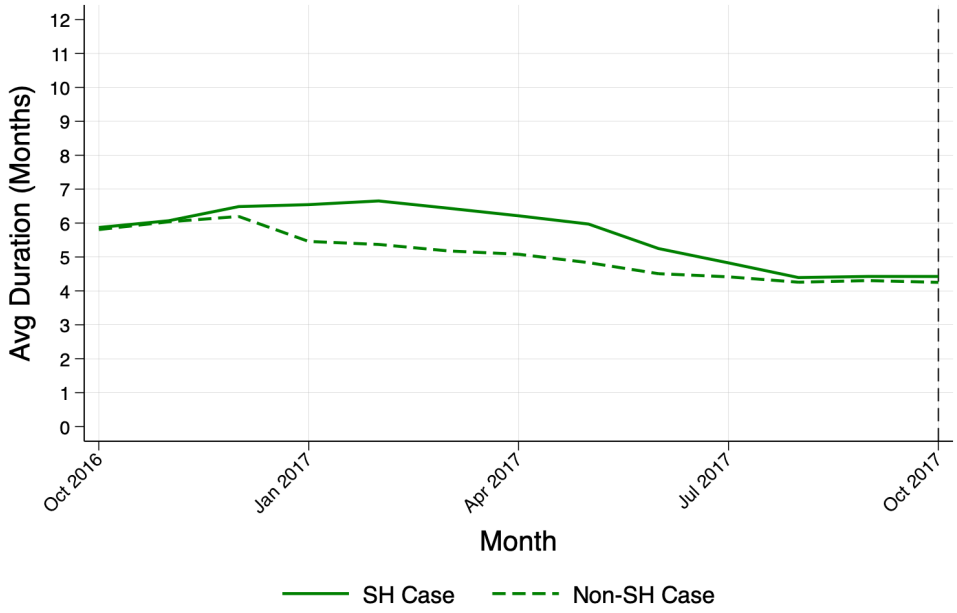


Figure A6: This figure displays the average duration of all open cases in a given month for the 12 months before #MeToo (indicated with grey dashed line) by sexual harassment and non-sexual harassment cases.

FIGURE A7 — WIN RATE OF OVERLAP CASES RESOLVED AFTER #METOO

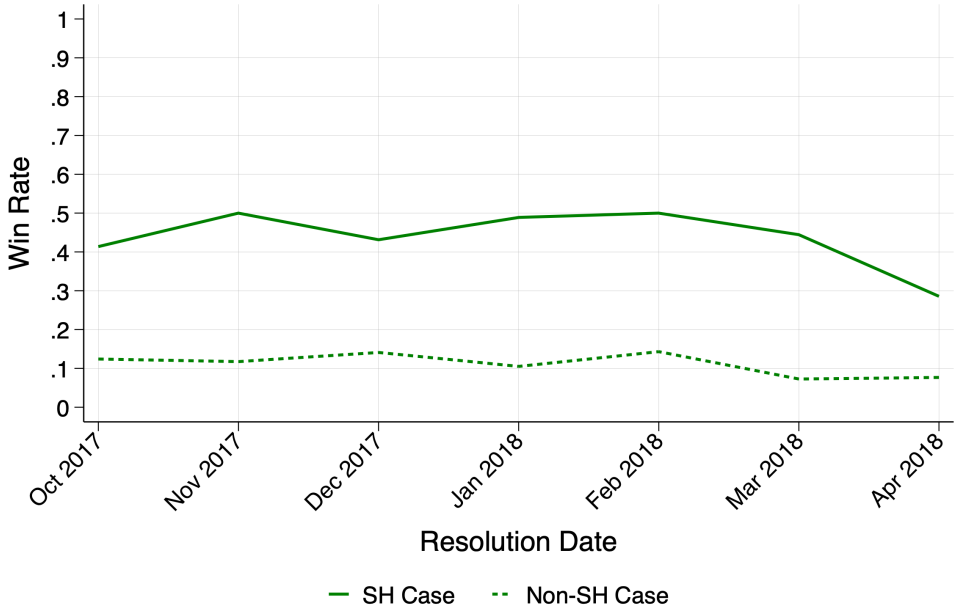


Figure A7: This figure shows the win rate of both sexual harassment and control (non-sexual harassment) cases over time, for cases resolved in the six months post-#MeToo.

TABLE A3 — COMPARISON OF ACTUAL OVERLAP CASES (2017) VERSUS PLACEBO OVERLAP CASES (2016)

Variable	(1) 2016	(2) 2017	(3) Diff	(4) p-value
Complainant is female	0.51	0.50	-0.01	0.28
Sexual harassment	0.10	0.08	-0.02	0.00***
Filed in Spring	0.24	0.21	-0.03	0.00***
Filed in Summer	0.39	0.44	0.05	0.00***
Filed in Fall	0.25	0.25	-0.00	1.00
Filed in Winter	0.12	0.10	-0.02	0.00***
Observations	5,315	6,230	11,545	

Table A3: From top to bottom, the variables measure the share of cases with a female complainant, the share classified as sexual harassment, and the share filed in each season. Col 1 presents these values for cases that would have been considered overlap if #MeToo had occurred in 2016, while Col 2 presents values for cases that are actually considered overlap. Col 3 shows the difference in values for each variable (Col 1 minus Col 2), and Col 4 displays the corresponding p-values. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

TABLE A4 — RD POINT ESTIMATES ACROSS VARIOUS SPECIFICATIONS

	Polynomial order	RD effect	p-value
Men			
No fixed effects	1	−0.180	0.300
No fixed effects	2	−0.275	0.431
State and time fixed effects	1	−0.137	0.460
State and time fixed effects	2	−0.199	0.787
State by time fixed effects	1	−0.120	0.498
State by time fixed effects	2	−0.190	0.871
Women			
No fixed effects	1	0.024	0.740
No fixed effects	2	0.048	0.841
State and time fixed effects	1	0.051	0.500
State and time fixed effects	2	0.096	0.618
State by time fixed effects	1	0.025	0.547
State by time fixed effects	2	0.085	0.649

Table A4: This table presents estimates of RD effects and associated p-values analogous to Figure A8, but across six different specifications, including the one shown in Figure A8. We first restrict the sample to sexual harassment complaints resolved after #MeToo filed by male (top panel) and female (bottom panel) complainants. Each row presents the result from a regression discontinuity design where the outcome is the win rate, and the specification changes row-by-row as indicated. We use an optimal bandwidth selection procedure (Calonico, Cattaneo, and Titiunik 2014) to select 200 days for men and 240 days for women as the optimal bandwidth.

FIGURE A8 — RD PLOT OF WIN RATE FOR CASES RESOLVED AFTER #METOO

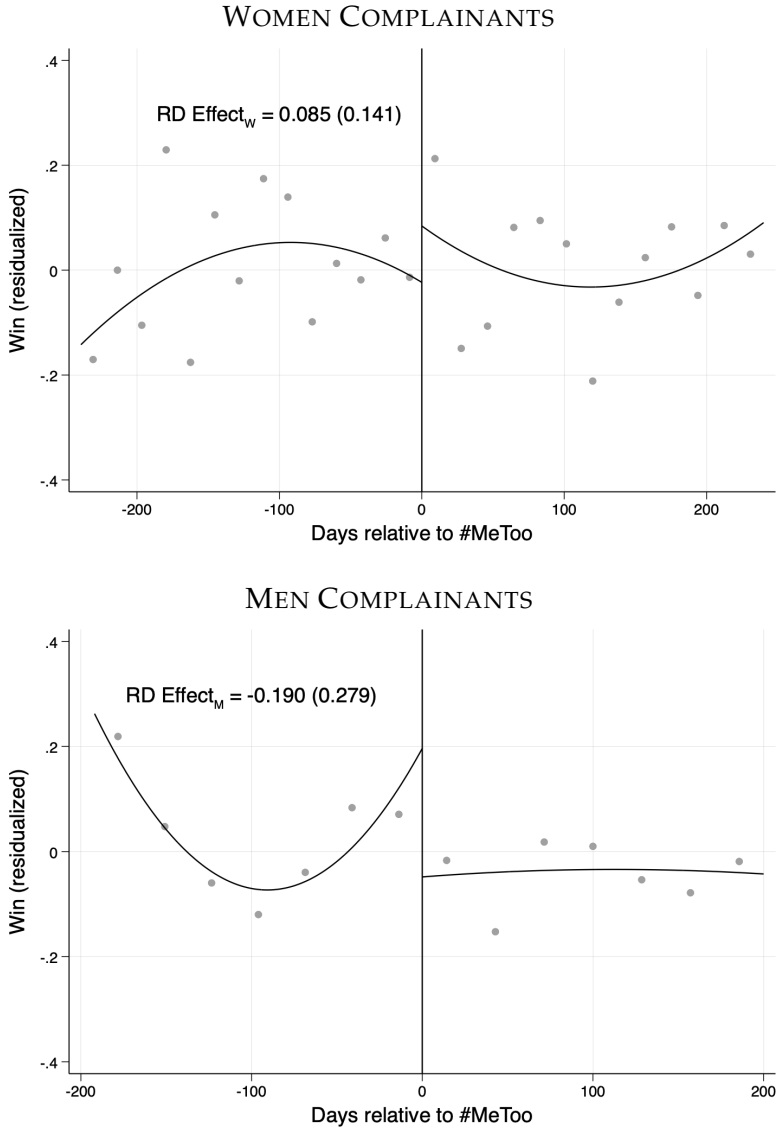


Figure A8: These figures estimate a regression discontinuity design using an optimal bandwidth selection method (Calonico, Cattaneo, and Titiunik 2014). Each figure shows the win rate change around the #MeToo discontinuity for sexual harassment cases that resolve after #MeToo, filed by women (top) versus men (bottom). The x-axis represents the day each case was filed, therefore, data points before #MeToo represent win rates for cases filed before but resolved after #MeToo, data points shown after #MeToo represent win rates for cases filed after and resolved after. The chosen bandwidth was 240 days around #MeToo for women and 200 days for men. The outcome is win rate, residualized on state fixed effects interacted with time fixed effects (representing the year-month of complaint resolution) with a polynomial fit of order two. The "RD Effect" label in each graph shows the point estimate and SE in parentheses, corresponding to the specification in the last row in each panel of Table A4.

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 β_M^O and β_W^O 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 foundedness compared to always reporters, which we investigate by comparing the ordinal rankings of the combined effect coefficients (β_M^{CE} and β_W^{CE}, respectively) to the overlap coefficients (β_M^O and β_W^O, respectively) (again, see Propositions 1 and 2).

As a summary of our findings, we first present Table A5, 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.[35] The estimated effect on the probability of winning remains positive and significant for both combined and overlap coefficients. $\hat{\beta}_M^{CE}$ is smaller than $\hat{\beta}_M^O$, and $\hat{\beta}_W^{CE}$ is larger than $\hat{\beta}_W^O$, 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 A6 and A7.

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 $\hat{\beta}_M^O$ and $\hat{\beta}_W^O$ 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 $\hat{\beta}_M^{CE}$ (0.113) is smaller than $\hat{\beta}_M^O$ (0.153), and the combined effect for women $\hat{\beta}_W^{CE}$ (0.119) is larger than $\hat{\beta}_W^O$ (0.082), both of which are directionally consistent with our main results. The estimates for this robustness check are presented in Tables A6 and A7.

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

35. 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.

are systematically different as a result of pandemic-related changes in workplace dynamics or agency operations. Because the start of the Covid-19 pandemic occurs after the start of #MeToo, $\hat{\beta}^{O}$, $\hat{\beta}^{O}_{M}$, and $\hat{\beta}^{O}_{W}$ are mechanically unchanged after this sample restriction. However, $\hat{\beta}^{CE}$, $\hat{\beta}^{CE}_{M}$, and $\hat{\beta}^{CE}_{W}$ are potentially affected by the restriction, and we compare the differences between the overlap and combined effects, respectively. $\hat{\beta}^{O}_{M}$ 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. 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 A6 and A7.

In the fourth robustness check we consider, we present estimates of Equation 1a using case filing rather than case resolution time fixed effects. Throughout the paper, we have defined time fixed effects at case resolution in order to capture the effect at the time of adjudicator decision. However, since overlap cases are filed and resolved on either side of treatment, the choice of time fixed effects may be consequential for the ordinal comparison of $\hat{\beta}^{CE}$ and $\hat{\beta}^{O}$. Table A8 shows that using filing fixed effects changes the point estimates of $\hat{\beta}^{CE}$, $\hat{\beta}^{O}$, and their gendered version only slightly, significance of all point estimates remains the same, and the main conclusions of the paper are unchanged to using case filing fixed effects.

In the fifth robustness check, we examine how sensitive our inference is to our choice of clustered standard errors. Throughout the paper we cluster standard errors at the state-by-discrimination type level to account for within-state and within-discrimination type correlation in how complaints are filed and adjudicated, yielding 57 clusters. A natural robustness check is wild bootstrap—however, since treatment is assigned at the discrimination type level $g(i)$ with a single treated discrimination type, cluster-robust inference would be unreliable even with wild bootstrap. We therefore conduct a permutation test by reassigning treatment to each of the other six (non-treated) discrimination types and re-run the regression described in Equation 1a. If the effect of treatment on sexual harassment is truly an outlier, we should expect small (in magnitude) and statistically insignificant coefficients on these placebo treatments. Moreover, we apply a pre-trends restriction to these placebo treatments to ensure that we are not comparing a plausibly causal estimate (our baseline) to non-causal estimates driven by pre-trends in the candidate placebo treatments. Specifically, we include only placebo treatments in which no more than one pre-#MeToo treatment estimate is statistically different from 0 at the $\alpha = 0.05$ level, as estimated in Equation 1b. The following placebos are retained: age, disability, nationality, and race. The results, presented in Table A9, are encouraging – the actual treatment effect (0.101) exceeds all placebo estimates in magnitude and has a smaller p-value (0.001).

In the sixth robustness check, we expand our main sample to include complaints from Connecticut. Connecticut provided us with 22,837 complaints spanning employment, public accommoda-

tion, and housing discrimination. We exclude Connecticut from our main sample because the state agency does not separately track employment and public accommodation complaints, making it impossible to isolate employment discrimination cases with certainty. However, 75% of all complaints across the rest of our data are related to employment, and only 3% are related to public accommodation discrimination. We therefore add both employment and public accommodation complaints in Connecticut to this sample. We re-estimate $\hat{\beta}^{CE}$ and $\hat{\beta}^{O}$ on the larger sample including CT, and our results are presented in Tables A10 and A11. The pattern of our findings mirrors that in our main sample. Both $\hat{\beta}^{CE}$ and $\hat{\beta}^{O}$ remain positive and significant (0.096, std. err. = 0.030 and 0.086, std. err. = 0.013), indicating that adjudicators lowered their evidentiary threshold after #MeToo. Similarly, the direction of selection for both genders is consistent with our main results. $\hat{\beta}^{O}_{M}$ is larger than $\hat{\beta}^{CE}_{M}$ (0.154 $>$ 0.116), while $\hat{\beta}^{CE}_{W}$ is larger than $\hat{\beta}^{O}_{W}$ (0.116 $>$ 0.084), indicating men are negatively selected while women are positively selected among all discrimination complaints in our sample.

The seventh robustness check presents the results of estimating our main regressions but on our third sample of complaints, i.e., discrimination complaints filed across the housing, public accommodation, education, and employment sectors. Doing so allows us to use data from 351,222 complaints filed across 18 states. Both $\hat{\beta}^{CE}$ and $\hat{\beta}^{O}$ remain positive and significant (0.129, std. err. = 0.068 and 0.184, std. err. = 0.033), 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 $\hat{\beta}^{CE}_{M}$ (0.212 $>$ 0.200), while $\hat{\beta}^{CE}_{W}$ is larger than $\hat{\beta}^{O}_{W}$ (0.238 $>$ 0.218), indicating men are negatively selected while women are positively selected among all discrimination complaints in our sample. These findings are presented in Tables A12 and A13.

Our final robustness check seeks to understand whether firm-level factors or industry-level factors are more relevant for determining the effect of #MeToo. While a full analysis is difficult given the limited case details included in our publicly-provided dataset, we utilize the name of the employer to investigate this question. We use Anthropic's Claude Pro LLM to classify whether a complainant likely worked for a Fortune 500 company or in a primarily male workforce industry.[36] In Table A14 we explore our combined effects regression separately for Fortune 500 employees (left panel), and in male-dominated-industry employees (right panel). The effect sizes, for both men and women, are larger than our baseline estimates at Fortune 500 companies, and are similar in male-dominated industries as in our baseline estimates.

36. We queried Claude Pro on April 17, 2026 to write a Python script to clean and classify employers. The script cleans employer names by stripping special characters and standardizing abbreviation and capitalization. Then, it matches cleaned names against a list of Fortune 500 companies. To classify the industry, the script matches the employer name to industry keywords (i.e., words like 'auto', 'delivery', 'truck' are matched to the transportation industry). Then, all firms in a given industry are labeled as male-dominated or not. Construction/Trades, Automotive, Manufacturing, Transportation/Logistics, Mining/Energy/Utilities, and Technology/Telecom employers are labeled as male-dominated. Healthcare, Education/Childcare, and Personal Services employers are labeled as female-dominated. Classifications are available on request.

TABLE A5 — COMPARISON OF $\hat{\beta}^{CE}$ AND $\hat{\beta}^{O}$ ACROSS ROBUSTNESS CHECKS FOR COMPLAINTS WITH GENDER

Main results	$\hat{\beta}^{CE}$	$\lessgtr$	$\hat{\beta}^{O}$
Triple Diff (Men)	0.119	$<$	0.153
Triple Diff (Women)	0.123	$>$	0.086
Robustness Check	$\hat{\beta}^{CE}$	$\lessgtr$	$\hat{\beta}^{O}$
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 A5: 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 A6 — ROBUSTNESS OF $\hat{\beta}^{CE}$ ACROSS DIFFERENT SAMPLES AND CONTROL GROUPS

	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***
	(0.036)	(0.038)	(0.023)	(0.027)	(0.026)	(0.027)	(0.016)	(0.016)	(0.013)
SH × Post × Female			0.001			0.006			0.017
			(0.052)			(0.021)			(0.020)
{Unit, Time} × State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
{Unit, Time} × State × Female 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 A6: 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. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

TABLE A7 — ROBUSTNESS OF $\hat{\beta}^O$ ACROSS DIFFERENT SAMPLES AND CONTROL GROUPS

	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***
	(0.011)	(0.010)	(0.041)	(0.014)	(0.012)	(0.010)	(0.013)	(0.011)	(0.014)
SH × Post × Female			-0.077			-0.071**			-0.067**
			(0.055)			(0.027)			(0.027)
{Unit, Time} × State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
{Unit, Time} × State × Female 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 A7: 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. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

TABLE A8 — ROBUSTNESS OF MAIN RESULTS TO USING COMPLAINT FILING FIXED EFFECTS

	Combined effect	Overlap
$\hat{\beta}$	0.100***	0.090***
	(0.029)	(0.011)
$\hat{\beta}_M$	0.118***	0.156***
	(0.025)	(0.014)
$\hat{\beta}_W$	0.120***	0.085***
	(0.033)	(0.021)

Table A8: This table reports estimates of Equation 1a for the entire sample and the overlap sample using complaint filing rather than complaint resolution time fixed effects $\mu_{m(i)s(i)}$. Standard errors are reported in parentheses. Coefficients in the table represent the combined and overlap effects for all complaints ($\hat{\beta}^{CE}$ and $\hat{\beta}^{O}$), for men ($\hat{\beta}_M^{CE}$ and $\hat{\beta}_M^{O}$), and women ($\hat{\beta}_W^{CE}$ and $\hat{\beta}_W^{O}$). Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

TABLE A9 — PERMUTATION TEST

	$\hat{\beta}$	p-value
Sexual harassment	**0.101**	**0.001**
Disability	0.001	0.945
Nationality	0.003	0.800
Age	−0.030	0.011
Race	0.025	0.148

Table A9: This table reports the results of a permutation test in which we re-ran our main specification reassigning treatment to each discrimination type $g(i)$ sequentially in each row (excluding sexual harassment from the control group). The top row, bolded, is our actual treatment. The columns show the coefficient on win and the p-value. Religion and Retaliation discrimination bases are excluded because they fail a pre-trends test (i.e., more than two pre-treatment year coefficient estimates are statistically different from zero at the $\alpha = 0.05$ level).

TABLE A10 — $\hat{\beta}^{CE}$ INCLUDING CONNECTICUT COMPLAINTS

	All complaints	**Complaints with gender**	
SH × Post	0.096***	0.110***	0.116***
	(0.030)	(0.028)	(0.025)
SH × Post × Female			-0.000
			(0.021)
{Unit, Time} × State FE	✓	✓	✓
{Unit, Time} × State × Female FE			✓
N	79,010	51,259	51,143
R^2	0.173	0.101	0.108
Control mean	0.147	0.213	0.216

Table A10: This table is equivalent to Table 2, but we expand the sample to include both employment and public accommodation complaints from Connecticut. Connecticut's data provider does not separate discrimination in employment from public accommodation. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

TABLE A11 — $\hat{\beta}^O$ INCLUDING CONNECTICUT COMPLAINTS

	All complaints	**Complaints with gender**	
SH × Post	0.086***	0.100***	0.154***
	(0.013)	(0.010)	(0.010)
SH × Post × Female			-0.070***
			(0.019)
{Unit, Time} × State FE	✓	✓	✓
{Unit, Time} × State × Female FE			✓
N	42,317	33,536	33,458
R^2	0.131	0.083	0.091
Control mean	0.147	0.213	0.216

Table A11: This table is equivalent to Table 3, but we expand the sample to include both employment and public accommodation complaints from Connecticut. Connecticut's data provider does not separate discrimination in employment from public accommodation. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

TABLE A12 — $\hat{\beta}^{CE}$ POOLING HOUSING, PUBLIC ACCOMMODATIONS, EDUCATION, AND EMPLOYMENT COMPLAINTS

	All complaints	**Complaints with gender**	
SH × Post	0.129*	0.227***	0.200***
	(0.068)	(0.034)	(0.029)
SH × Post × Female			0.038***
			(0.014)
{Unit, Time} × State FE	✓	✓	✓
{Unit, Time} × State × Female FE			✓
N	150,313	91,535	91,276
R^2	0.150	0.098	0.106
Control mean	0.136	0.197	0.199

Table A12: This table is equivalent to Table 2, but we expand the sample to include all types of discrimination allegations (employment, housing, public accommodation, and education) from all 18 states in our sample. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

TABLE A13 — $\hat{\beta}^{O}$ POOLING HOUSING, PUBLIC ACCOMMODATIONS, EDUCATION, AND EMPLOYMENT COMPLAINTS

	All complaints	**Complaints with gender**	
SH × Post	0.184***	0.214***	0.212***
	(0.033)	(0.027)	(0.047)
SH × Post × Female			0.006
			(0.038)
{Unit, Time} × State FE	✓	✓	✓
{Unit, Time} × State × Female FE			✓
N	80,963	58,867	58,708
R^2	0.133	0.081	0.089
Control mean	0.136	0.197	0.199

Table A13: This table is equivalent to Table 3, but we expand the sample to include all types of discrimination allegations (employment, housing, public accommodation, and education) from all 18 states in our sample. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

TABLE A14 — $\hat{\beta}^{CE}$ BY EMPLOYER CHARACTERISTICS

	Fortune 500 Company			Male-dominated Industry		
	All complaints	**Complaints with gender**		**All complaints**	**Complaints with gender**	
SH × Post	0.173***	0.186**	0.195***	0.077***	0.087***	0.125***
	(0.060)	(0.063)	(0.044)	(0.024)	(0.025)	(0.026)
SH × Post × Female			-0.017			-0.046
			(0.034)			(0.034)
{Unit, Time} × State FE	✓	✓	✓	✓	✓	✓
{Unit, Time} × State × Female FE			✓			✓
N	6,852	5,571	5,528	9,353	7,581	7,539
R^2	0.181	0.139	0.183	0.158	0.108	0.137
Control mean	0.180	0.219	0.223	0.202	0.248	0.251

Table A14: Each panel is equivalent to Table 2, except that the sample is restricted to complaints filed against an employer that is a Fortune 500 company (columns 1-3) and complaints filed against an employer that is predicted to be male-dominated by an AI model (columns 4-6). Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

D ω calibrations

Although we show in Section 8 that the share of always reporters post #MeToo, ω, does not affect our main qualitative predictions, it may nevertheless be illustrative to consider benchmark values of ω. In this section, we provide two potential such benchmarks.

The share of always reporters can be inferred from changes in the number of cases reported before and after #MeToo, under assumptions about the shares of founded versus unfounded cases before and after #MeToo.[37] We make two sample restrictions to increase consistency. First, we consider only cases in the states present for the entirety of our sample. Second, to avoid the mechanical drop in cases following the Covid-19 pandemic, we consider reporting counts from our data restricting to one year pre- and post-#MeToo. In this subsample, there are 767 sexual harassment cases prior to #MeToo and 957 sexual harassment cases after #MeToo, yielding $957 - 767 = 190$ additional reports in the post period.

Our first calibration method equates the set of induced reporters to precisely be the 190 additional cases. In order for this to hold given our definitions of always and induced reporters, one of the following two limit cases must hold:

- All 767 cases pre-#MeToo were founded, 767 cases post-#MeToo were founded, and 190 cases post-#MeToo were unfounded, or
- All 767 cases pre-#MeToo were unfounded, 767 cases post-#MeToo were unfounded, and 190 cases post-#MeToo were founded.

In either of these cases, the share of always reporters is $\omega_1 := \frac{767}{957} = 0.801$ and the share of induced reporters is $1 - \omega_1 = \frac{190}{957} = 0.199$.

Our second calibration method dispenses with the extreme assumption either that all cases in the pre-period are founded or that all cases in the pre-period are unfounded. Instead, we assume that the share of founded accusations in the pre-period is equal to the share of "won" investigations in the pre-period. This value in our data is 0.165. To account for potential changes caused by #MeToo, we assume that #MeToo lowers the share of founded accusations by 0.014. This value is inspired by Levy and Mattsson (2025), who find that #MeToo decreased sexual *assault* incidence by 1.4 percentage points.[38] Based on these numbers, the share of always reporters is $\omega_2 := \frac{\frac{957(0.165-0.014)}{0.165}}{957} = \frac{0.165-0.014}{0.165} = 0.915$, where $\frac{957(0.165-0.014)}{0.165}$ is the number of always reporters in the post-period, i.e., the maximal number of cases such that precisely a 0.165 share are founded. The share of induced reporters is $1 - \omega_2 = 1 - 0.915 = 0.085$.

Our calibrated values are therefore $\omega \in \{0.801, 0.915\}$. We reiterate that these are illustrative, and our qualitative conclusions do not depend on the specific value of ω.

37. Throughout, we ignore integer constraints implied by data finiteness, and allow for a fractional case to be founded.

38. Levy and Mattsson's analysis is based on the Campus Climate Survey, which measures the prevalence of sexual misconduct using self-reported data from U.S. university students. Sexual assault and sexual harassment are different, and college students are a selected sample, however, their estimate is possibly the best existing proxy for our setting.

E Robustness of the Theoretical Framework: Signal Precision

The model in Section 6 represents the direct effect of #MeToo as a change in the evidentiary threshold $\underline{e}^t$, holding fixed the evidence-generating process. This appendix shows that the selection-versus-direct-treatment decomposition does not rely on this particular interpretation. The same decomposition remains valid if #MeToo changes the precision of the investigative signal, either instead of or in addition to changing the evidentiary threshold.

Recall the framework proposed within period in the main text. Let G_F^t and G_U^t denote the distributions of evidence in period $t \in \{-1, 0\}$ for founded and unfounded complaints, respectively, with densities g_F^t and g_U^t. A case with foundedness level $\tau \in \{F, U\}$ wins in period t if the realized evidence exceeds the period-specific evidentiary threshold $\underline{e}^t$. Thus, the probability that a complaint of type τ wins is

$$q_\tau^t := \Pr(e \geqslant \underline{e}^t \mid \tau) = 1 - G_\tau^t(\underline{e}^t).$$

The main text focuses on the case in which $G_F^t = G_F$ and $G_U^t = G_U$ for both periods, so that direct treatment operates only through a change in $\underline{e}^t$. We now allow both $\underline{e}^t$ and the signal structure (G_U^t, G_F^t) to vary across periods.

Definition 2 (More precise signal structure)**.** *Suppose G_F, G_U, G_F', G_U' have strictly positive densities on $(0, 1)$. We say that the signal structure (G_U', G_F') is more precise than (G_U, G_F) if*

$$\frac{g_F'(e)}{g_F(e)} \quad \textit{is weakly increasing in } e$$

and

$$\frac{g_U(e)}{g_U'(e)} \quad \textit{is weakly increasing in } e.$$

The first condition says that the evidence distribution for founded complaints shifts toward higher values of evidence. The second condition says that the evidence distribution for unfounded complaints shifts toward lower values of evidence. Thus, greater precision separates founded from unfounded complaints: it makes founded complaints easier to verify and unfounded complaints easier to reject.

The following lemma makes two points. First, it relates the "more evidence for founded cases" and "less evidence for unfounded cases" likelihood ratio statements above to win probabilities: founded cases benefit from more precision, and unfounded cases benefit from less precision given any evidentiary threshold. The second point is a technical one, and states that increasing precision maintains the underlying likelihood ratio assumption.

Lemma 1. *If (G_U', G_F') is more precise than (G_U, G_F), then for every $\underline{e} \in (0, 1)$,*

$$1 - G_F'(\underline{e}) \geqslant 1 - G_F(\underline{e})$$

and

$$1 - G'_U(\underline{e}) \leqslant 1 - G_U(\underline{e}).$$

Moreover, if the original signal structure satisfies the monotone likelihood ratio property, so that $g_F(e)/g_U(e)$ is weakly increasing in e, then the more precise signal structure also satisfies the monotone likelihood ratio property.

Proof. The first two inequalities follow from the standard implication that likelihood ratio dominance implies first-order stochastic dominance. Since g'_F/g_F is weakly increasing, G'_F first-order stochastically dominates G_F, implying $1 - G'_F(\underline{e}) \geqslant 1 - G_F(\underline{e})$ for every threshold $\underline{e}$. Similarly, since g_U/g'_U is weakly increasing, G_U first-order stochastically dominates G'_U, implying $1 - G'_U(\underline{e}) \leqslant 1 - G_U(\underline{e})$.

For the final claim, note that

$$\frac{g'_F(e)}{g'_U(e)} = \frac{g'_F(e)}{g_F(e)} \cdot \frac{g_F(e)}{g_U(e)} \cdot \frac{g_U(e)}{g'_U(e)}.$$

Each term on the right-hand side is weakly increasing in e, so their product is weakly increasing in e. □

This distinction matters for interpretation. A decrease in the evidentiary threshold $\underline{e}^t$ raises the probability that both founded and unfounded complaints win. By contrast, an increase in signal precision raises the probability that founded complaints win and lowers the probability that unfounded complaints win, holding fixed the threshold. If both leniency and precision change, the direct treatment effect for always reporters is the net effect of both forces.

To see this formally, recall that $\mathcal{T}_{-1}$ denotes the share of founded complaints among pre-#MeToo reporters. By definition, always reporters have the same share of founded complaints. Therefore, the expected win rate for always reporters in period t is

$$W^t_{AR} = \mathcal{T}_{-1} q^t_F + (1 - \mathcal{T}_{-1}) q^t_U.$$

The direct treatment effect is

$$ATT^{AR} = W^0_{AR} - W^{-1}_{AR} = \mathcal{T}_{-1}(q^0_F - q^{-1}_F) + (1 - \mathcal{T}_{-1})(q^0_U - q^{-1}_U).$$

When only the threshold changes, the sign of ATT^{AR} reveals whether the evidentiary threshold rose or fell. When signal precision can also change, the sign of ATT^{AR} no longer separately identifies the direction of precision or leniency. A positive ATT^{AR} may reflect a lower evidentiary threshold, a precision change that sufficiently benefits founded complaints, or a combination of the two.

The empirical decomposition, however, is unchanged. The decomposition in Equation (4) follows from the law of iterated expectations and does not require the direct treatment effect to operate

only through the threshold. Similarly, the overlap design continues to identify ATT^{AR} under the same logic as in the main text, provided that Assumption A2 is interpreted as invariance of the entire adjudication environment $\left(\underline{e}^0, G_F^0, G_U^0\right)$ rather than invariance only of the threshold.

Proposition 4. *Allow #MeToo to change both the evidentiary threshold and the precision of the investigative signal. Under the parallel trends assumptions in the main text and the modified filing invariance assumption, the overlap coefficient identifies ATT^{AR}, and the combined effect decomposes as*

$$ATT^{CE} = \omega ATT^{AR} + (1-\omega) ATT^{IR}.$$

Moreover, induced reporters are positively selected for foundedness if and only if induced reporters have a higher post-#MeToo win rate than always reporters.

Proof. The identification of ATT^{AR} from overlap cases follows exactly as in the main text. The proof uses the fact that overlap cases are filed before #MeToo and therefore are not selected into reporting by #MeToo, together with filing invariance of the direct treatment effect. It does not require the direct effect to operate only through the evidentiary threshold.

The decomposition

$$ATT^{CE} = \omega ATT^{AR} + (1-\omega) ATT^{IR}$$

follows from the law of iterated expectations, since post-#MeToo reporters are partitioned into always reporters and induced reporters.

It remains only to show that post-period win-rate comparisons continue to reveal selection. Let $\mathcal{T}^{IR}$ denote the share of founded complaints among induced reporters. Since always reporters have foundedness share $\mathcal{T}_{-1}$, the post-period win-rate difference between induced reporters and always reporters is

$$W_{IR}^0 - W_{AR}^0 = \left[\mathcal{T}^{IR} q_F^0 + (1-\mathcal{T}^{IR}) q_U^0\right] - \left[\mathcal{T}_{-1} q_F^0 + (1-\mathcal{T}_{-1}) q_U^0\right].$$

Rearranging,

$$W_{IR}^0 - W_{AR}^0 = (\mathcal{T}^{IR} - \mathcal{T}_{-1})(q_F^0 - q_U^0).$$

The post-#MeToo monotone likelihood ratio property implies $q_F^0 > q_U^0$ for any interior threshold. Therefore,

$$W_{IR}^0 \geqslant W_{AR}^0 \quad \Longleftrightarrow \quad \mathcal{T}^{IR} \geqslant \mathcal{T}_{-1}.$$

Thus, induced reporters have higher post-#MeToo win rates than always reporters if and only if they are positively selected for foundedness. □

The augmented model also clarifies what can and cannot be learned from the sign of ATT^{AR}. In the threshold-only model, $ATT^{AR} > 0$ implies that the evidentiary threshold fell. In the augmented model, $ATT^{AR} > 0$ implies only that the net direct effect of the post-#MeToo adjudication environment raised the win probability of always reporters. This direct effect may combine greater

leniency or precision changes.

Evidence and interpretation

Because the sign of ATT^{AR} does not reveal whether precision rose or fell, we turn to auxiliary evidence on case duration. If investigative effort is approximately uniform over time, and if greater effort produces a more precise signal of foundedness, then longer case duration (for overlap complainants) is suggestive of greater signal precision. We therefore estimate our overlap specification using case duration—the number of days between filing and resolution—as the outcome. The results are presented in Table A15.

The duration results provide some suggestive evidence that case duration *falls* after #MeToo for both men and women. Under the auxiliary assumption that longer duration corresponds to greater investigative effort, this pattern suggests that adjudicators generated less precise signals of foundedness after #MeToo.

Does a change in adjudicator signal precision alone drive our results on the effect of #MeToo on winning a filed case? To answer this question, we define a win-rate-neutral precision benchmark. This benchmark captures the idea that precision changes redistribute wins across founded and unfounded complaints, but do not change the unconditional probability that an unselected complaint wins when the evidentiary threshold is held fixed.

Definition 3. *Suppose the evidentiary threshold is unchanged, $\underline{e} := \underline{e}^0 = \underline{e}^{-1}$. We say that the change in the signal structure satisfies the* win-rate-neutral precision benchmark *if*

$$W^t_{AR}(\underline{e}) := \mathcal{T}_{-1}\left[1 - G^t_F(\underline{e})\right] + (1 - \mathcal{T}_{-1})\left[1 - G^t_U(\underline{e})\right]$$

is constant in t.

Under this benchmark,

$$ATT^{AR} = W^0_{AR}(\underline{e}^0) - W^{-1}_{AR}(\underline{e}^{-1}) = W^0_{AR}(\underline{e}) - W^{-1}_{AR}(\underline{e}) = 0.$$

(for the win outcome variable). The strictly positive, and statistically significant, overlap estimate in Table 3 (for both men and women) is therefore inconsistent with the precision-only benchmark. This does not rule out precision changes as part of the direct treatment effect. Rather, it implies that precision changes alone, in the narrow sense defined above, cannot explain the observed increase in always reporters' win rates. The direct treatment effect we estimate should therefore be interpreted as a net change in the adjudication environment, with the evidence pointing toward a meaningful increase in adjudicator leniency rather than a pure increase in investigative precision.

TABLE A15 — $\hat{\beta}^O$ FOR CASE DURATION

	All complaints	**Complaints with gender**	
SH × Post	-18.270***	-21.411***	-25.572***
	(6.849)	(6.494)	(6.817)
SH × Post × Female			9.478
			(7.517)
{Unit, Time} × State FE	✓	✓	✓
{Unit, Time} × State × Female FE			✓
N	60,711	41,628	41,627
R^2	0.419	0.277	0.281
Control mean	174.224	240.384	240.602

Table A15: This table is equivalent to Table 3, but where the outcome variable is case duration. Single asterisk denotes statistical significance at the 90% level of confidence; double, 95%; triple, 99%.

F Data Appendix

F.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 March 2026, we have received usable data containing employment, public accommodation, housing, or education discrimination complaints from 18 states. Our main analysis sample consists of the 10 states that provided employment discrimination data (listed in Table A1, Panel A). One state (Connecticut) provided data but did not distinguish employment from public accommodation discrimination. We therefore do not use CT in our main analysis, but add CT data to our analysis in a robustness check (Table A1, Panel B). The remaining 7 states provided data on other types of discrimination but not employment discrimination (Table A1, Panel C).

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. Connecticut: Received a spreadsheet after submitting a request via the online portal.
4. Delaware: Received a spreadsheet after submitting a request via email to dos.foia@delaware.gov.
5. Florida: Received a spreadsheet in PDF format after submitting a request via email to records@fchr.myflorida.com.
6. Hawaii: Received a spreadsheet after submitting a request via email to DLIR.HCRC.INFOR@hawaii.gov.
7. Illinois: Received a spreadsheet in PDF format after submitting a request via email to IDHR.FOIA@illinois.gov.
8. Kentucky: Received a spreadsheet after submitting a request via email to kchr.mail@ky.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. North Carolina: Received a spreadsheet after submitting a request via email to oah.postmaster@oah.nc.gov.
13. North Dakota: Received a spreadsheet after submitting a request via email to labor@nd.gov.
14. New York: Received a spreadsheet after submitting a request via the online portal.
15. South Carolina: Received a spreadsheet after submitting a request via email to information@schac.sc.gov.
16. Texas: Received a spreadsheet in PDF format after submitting a request via the online portal.
17. Washington: Received a spreadsheet after submitting a request via email to records@hum.wa.gov.
18. Wisconsin: Received a spreadsheet after submitting a request via email to OpenRecords@dwd.wisconsin.gov.

We have **received unusable data** from 7 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. Maine: Compiled cases from the website after submitting a request via email to foaa@mhrc.maine.gov. Data are unusable because they were provided in unstructured PDF format.
3. 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.
4. Minnesota: Received a spreadsheet after submitting a request via the online portal. Data are unusable because there is no data from the pre-period.
5. 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.
6. Pennsylvania: Received 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.
7. Rhode Island: Received PDFs of case files after submitting a request via email to John.Bogue@richr.ri.gov. Data consisted of 12 observations which were excluded after meeting various sample restrictions.

Our request was **closed/denied by/not responded to by the circulation date of this draft** the following 23 states:

1. Alabama: Request submitted via the online portal and denied due to lack of state citizenship.

2. Arizona: Request submitted via the online portal.
3. Colorado: Request submitted via email to dora_ccrd@state.co.us and denied on the basis of state law.
4. Idaho: Request was closed after the state said that they do not release any files on the basis of confidentiality.
5. Indiana: Request submitted via the online portal and denied due to the range of data requested.
6. Iowa: Request submitted via email to icrc@iowa.gov and denied on the basis of state law.
7. Kansas: Request submitted via email to khrc@ks.gov.
8. Louisiana: Request submitted via email to GovPublicRecords@la.gov and denied on the basis of state law.
9. Missouri: Request submitted via email to mchr@labor.mo.gov and denied on the basis of state law.
10. Nebraska: Request submitted via email to neoc.response@nebraska.gov and denied on the basis of state law.
11. Nevada: Request submitted via email to detrmedia@detr.nv.gov and denied on the basis of state law.
12. New Hampshire: Request submitted via email to humanrights@hrc.nh.gov and denied based on NH Administrative Rule Part 219.04.
13. New Jersey: Request submitted via the online portal and denied on the basis of state law.
14. 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.
15. Ohio: Request submitted via telephone and closed after the state shared that their retention period was only 5 years.
16. Oregon: Request submitted via email to public.records@boli.oregon.gov.
17. South Dakota: Request submitted via the online portal and denied on the basis of state law.
18. Tennessee: Request submitted via email to ask.thrc@tn.gov and denied due to lack of state citizenship.
19. 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.
20. Vermont: Request submitted via email to human.rights@vermont.gov and denied on the basis of state law.
21. 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.

22. West Virginia: Request submitted via contact us page.
23. Wyoming: Request submitted via the online portal and decided not to pursue after the state asked for payment of $440 to process the request.

F.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 seven categories: age, disability, nationality, race, religion, retaliation, and sex. The original basis of discrimination as provided by states is much more specific than these categories. For example, we classify "Sexual harassment" 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: age, retaliation, nationality, religion, 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.

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 resolved 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. If the outcome was not self-evident from the label, we followed up with state agencies over email to clarify their terminology.

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
ADMIN CLSR	dismissed == 1
Adjusted and Withdrawn	settle == 1
Adjusted/Terms of Settlement	settle == 1
Admin Closure	dismissed == 1
Admin Closure Outside of Regular Meeting	dismissed == 1
Admin. Closure	dismissed == 1
Administrative Closure	dismissed == 1
Administrative Closure DWOP	dismissed == 1
Administrative Closure/ DWOP	dismissed == 1
Administrative Closure; DWOP	dismissed == 1
Administrative Dismissal	dismissed == 1
Administratively closed	dismissed == 1
Annulment Issued	court == 1
Annulment Issued (no paper file)	court == 1
B1 - Successful Settlement	settle == 1
B2 - Predetermination Settlement (PDS)	settle == 1
B3 - Not Substantial Evidence (Exception)	win == 0
B4 - Conciliation Finalized	win == 1
C01 - Signed/notarized complaint not returned	dismissed == 1
C1 - Hearing Decision for Complainant	win == 1
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

Value	Variable
COURT-DISMISSED BY JUDGE	court == 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 Consent Decree	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 - 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 - Withdrawn	dismissed == 1
Closed - Withdrawn With Settlement	settle == 1
Closed Because Trial Has Begun	court == 1
Complainant Elected Court Action	court == 1
Complainant Failed To Cooperate	dismissed == 1
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

Value	Variable
Conc	win == 1
Conc by ENFORCEMENT	win == 1
Conciliated	win == 1
Conciliated Settlement	win == 1
Conciliated/Dismissal w/Pred.	win == 1
Conciliation	win == 1
Conciliation Closing Issued	win == 1
Conciliation Closing Issued (no paper file)	win == 1
Conciliation Failure	win == 1
Conciliation/settlement successful	settle == 1
Cp failed to co-operate	dismissed == 1
Cp not available	dismissed == 1
D3 - Appeal - Affirmed	win == 0
Determination Dismissing Complaint Issued	dismissed == 1
Determination by Another Agency	court == 0; dismissed == 0
DF	win == 1
DWOP	dismissed == 1
Decline to File: Exercise of Discretion	court == 0; dismissed == 0
Dismissal	dismissed == 1
Dismissal without Pred.	dismissed == 1
Dismissal w/out prejudice	dismissed == 1
Dismissal w/Pred.	dismissed == 1
Dismissal w/Settlement	dismissed == 1; settle == 1
Dismissal without Pred./NPC	win == 0
Dismissal without Prejudice/Admin Closure	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
FHAP judicial consent order	settle == 1
FHAP judicial dismissal	dismissed == 1
Failure to Cooperate	dismissed == 1
Failure to Locate	dismissed == 1
Failure to Proceed	dismissed == 1

Value	Variable
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
I18 - Decided by Court – no adjustment	court == 1
INVESTIGATED BY HUD	win == 0
INVESTIGATED- NO COURT	court == 0
Insufficient Evidence	win == 0
Intake Closure	dismissed == 1
Investigated and Dismissed	dismissed == 1
Investigated and Dismissed - Accusation Withdrawn	dismissed == 1
Investigated and Dismissed - Insufficient Evidence	dismissed == 1
Investigated and Dismissed - Insufficient Evidence and NJI	dismissed == 1
Investigated and Dismissed - Insufficient Evidence and NJI - CP Failed to Cooperate	dismissed == 1
Investigated and Dismissed - Insufficient Evidence and NJI - CP Failed to Respond to 30-Day Letter	dismissed == 1
Investigated and Dismissed - Insufficient Evidence and NJI - CP Filed Suit	court == 1
Investigated and Dismissed - Insufficient Evidence and NJI - Closed Due To Court Decision	court == 1
Investigated and Dismissed - Insufficient Evidence and NJI - Respondent unavailable	dismissed == 1
Investigated and Dismissed - No Basis to Proceed	dismissed == 1
Investigated and Dismissed - Withdrawn - Intend to file a lawsuit	court == 1
Investigated and Dismissed - Withdrawn - Other reasons	dismissed == 1

Value	Variable
Investigated and Dismissed - Withdrawn - Rescind	dismissed == 1
Investigated and Dismissed - Withdrawn - Resolved between parties	dismissed == 1
Investigation not Completed within Statutory Time Limit	dismissed == 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 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
MEDIATED BY COUNSEL	win == 1
Mediation/Settlement	settle == 1
NPC	win == 0
NPC Reactivated by HUD over 120 days old	win == 0
NPC W/OUT PREJUDICE	win == 0
NPC ; NPC on Recon	win == 0
NPC ; Recon DENIED	win == 0
NPC ; See Notes	win == 0
NPC Dismissal with Prejudice	win == 0
NPC w/out prejudice	win == 0
NPC/ Withdrawal	win == 0
NPC/ Withdrawal WITHOUT Settlement	win == 0
NPC/Order to Vacate & Set Aside & to Grant Withdrawal/Withdrawal	win == 0
NPC/Recon Pending	win == 0
NPC/Reconsideration	win == 0
Negotiated Settlement	settle == 1
No Basis to Proceed (For Filing Purposes Only)	dismissed == 1

Value	Variable
No Cause	win == 0
No Cause Determination	win == 0
No Cause Finding	win == 0
NO CAUSE - insufficient evidence	win == 0
No Follow-Up Info Rcvd from Requestor	dismissed == 1
No Probable Cause	win == 0
No Probable Cause Determination Issued	win == 0
No Reasonable Cause	win == 0
No cause determination	win == 0
Notice of Right to Sue	court == 1
Notice of Rights	win == 0
PC Determination Issued / Post Cause Conciliation	win == 1
PC Determination Issued / Conciliated	win == 1
PC Determination Issued / Conciliation	win == 1
PC Determination Issued / Dismissed by Circuit Court	win == 1
PC Determination Issued / Withdrawal WITH Settlement	win == 1
Post Cause Conciliation	win == 1
Pre-finding Settlement	settle == 1
Probable Cause	win == 1
Probable Cause Determination Issued	win == 1
Probable Cause Found	win == 1
Processing Waived to Another Agency	court == 0; dismissed == 0
RC - Resolution Conference Closure	settle == 1
Reasonable Cause Recommendation - Settled	win == 1
Reasonable Cause Recommendation - Withdrawn	win == 1
Reasonable remedy refused	court == 0; dismissed == 0
Relief covered by Ct order/consent decree	settle == 1
Resolved between Parties	settle == 1
Right to Sue	court == 1
Right to Sue Issued	court == 1
Right to Sue—Intake Closure	dismissed == 1

Value	Variable
Serve Annulment (Post-Investigation)	court == 1
Serve Final Order Dismissing Complaint	dismissed == 1
Serve Order After Hearing: Dismissing Complai	win == 0
Serve Order After Hearing: Sustaining Complain	win == 1
Serve Order After Stipulation of Settlement	settle == 1
Settled after Charge Issued	win == 1
Settled by Appeals	settle == 1
Settled by DRD and Legal: Mandatory Mediation	settle == 1
Settled by DRD and Legal: Post-civil Complaint	settle == 1
Settled by DRD: Post Appeal	settle == 1
Settled by DRD: Voluntary Mediation	settle == 1
Settled by Enforcement	settle == 1
Settled by Legal: Post- civil Complaint	settle == 1
Settled by Legal: Pre- Filing	settle == 1
Settled by Mediation: Voluntary	settle == 1
Settled with Respondent (specific complaint not sustained; other inequities remedied)	settle == 1
Settlement - Settled by Enforcement	settle == 1
Settlement - Settled by Mediation - Mandatory 1038	settle == 1
Settlement - Settled by Mediation - Voluntary	settle == 1
Settlement - Settled with Respondent - Pre- Civil	settle == 1
Split Decision: PC/LOPC or PC/LOJ	win == 1
Stay Substantial Evidence	win == 1
Successful Conciliation	win == 1
Transfer to EEOC (Closed at Commission)	dismissed == 1
Transferred to Circuit Court	court == 1
Unable to Locate Respondent	dismissed == 1
Unable to locate complainant	dismissed == 1
Unable to Serve	dismissed == 1
Untimely filed	dismissed == 1
VRA	settle == 1
WD	dismissed == 1
WDB	settle == 1

Value	Variable
Withdrawal	dismissed == 1
Withdrawal with Benefits	settle == 1
Withdrawal With Settlement	settle == 1
Withdrawal Without Settlement	dismissed == 1
Withdrawal w/o Predjudice	dismissed == 1
Withdrawal with Resolution/Settlement	settle == 1
Withdrawal without Resolution/Settlement	dismissed == 1
Withdrawn	dismissed == 1
Withdrawn With Resolution	settle == 1
Withdrawn Without Resolution	dismissed == 1
Withdrawn Request â Housing Only	dismissed == 1
Withdrawn Request â Housing Only - Intend to file a lawsuit	court == 1

Note: We do not include duplicate outcomes, including those differing only by spelling errors or capitalization. Double-mapped outcomes (e.g., court == 0; dismissed == 0) represent cases where administrative records allow us to rule out specific outcomes but are insufficient to classify the case further.