Why is Workplace Sexual Harassment Underreported? The Value of Outside Options Amid the Threat of Retaliation

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Why is workplace sexual harassment chronically underreported? We hypothesize that employers coerce victims into silence through the threat of a retaliatory firing. To test this, we estimate how two external shocks which reduce workers' outside options—unemployment rate increases and sharp cuts to unemployment insurance benefits—affect the selectivity of sexual harassment charges filed with the Equal Employment Opportunity Commission. We find that both shocks increase selectivity, which implies an increase in underreporting. Bolstering these findings, anonymous Google searches for "sexual harassment in the workplace" (total prevalence) spike relative to charges filed (reported prevalence) during the Great Recession.

Key Words: sexual harassment, unemployment, unemployment insurance

JEL Codes: J71, J78

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Virtually all of the people I spoke with told me that they were frightened of retaliation. "If Harvey were to discover my identity, I'm worried that he could ruin my life," one former employee said.

-"From Aggressive Overtures to Sexual Assault: Harvey Weinstein's Accusers Tell Their Stories," *The New Yorker*, October 23, 2017

1 Introduction

Based on anonymous survey responses, no fewer than 1 in 28 U.S. workers report having been victimized by workplace sexual harassment annually.[1] Yet only 1 in 13,000 workers file a formal sexual harassment charge with the Equal Employment Opportunity Commission (EEOC), the agency tasked with enforcing all federal anti-discrimination laws. Even in the aftermath of the #MeToo-induced reckoning, harassment charges are up only 10%, not nearly sufficient to close the underreporting gap. A major concern with underreporting is that it exacerbates the behavior: faced with a low probability of punishment, firms may respond by pushing the boundaries of their misconduct further.

The costs of this hidden sexual harassment, which affect an estimated 5 million workers per year, are substantial. Prior evidence suggests that victimization leads to a steep loss in job satisfaction, elevated emotional and mental stress, declines in productivity, increases in absenteeism, and job turnover.[2] Hersch (2018) estimates the value of statistical harassment (VSH) to be the same order of magnitude as the value of a statistical life (VSL), which while larger than one might have expected, highlights its seriousness. There are also pre-market costs of avoiding sexual harassment, which one study estimates to be 20% of the present discounted value of lifetime earnings in India (Borker, 2017).

Why is it, then, that underreporting is so common? One candidate explanation is that firms and managers coerce their employees into silence through the threat of retali-

[1]These calculations come from the 2015 American Working Conditions Survey conducted by RAND (see Maestas et al., 2017). The General Social Survey (see McCann et al., 2018) and the 2018 National Study on Sexual Harassment and Assault yield similar findings.

[2]See, for example, Crull (1982), Acken et al. (1991), Frierson (1989), Gutek and Koss (1993), Faley et al. (1994), Laband and Lentz (1998).

ation—through a firing, wage cut, or assignment to undesirable tasks or schedules—for engaging in a "protected activity," such as alerting HR or filing a charge with the EEOC. Figure 1 demonstrates that not only is employer retaliation a common response to an employee's decision to file a sexual harassment charge, but that this behavior has become increasingly prevalent over time, rising from 38% of cases in 1995 to 69% in 2016. At the same time, the number of annually filed sexual harassment charges has fallen by roughly 50%. Viewed in isolation, this latter pattern could naively be construed as a sign that charges have decreased solely because the problem of workplace sexual harassment has diminished over time.

A more cynical interpretation is that firms have also become more adept at credibly wielding the threat of retaliation to suppress worker reports. While the fear of employer retaliation has been hypothesized as a possible explanation for underreporting of sexual harassment and explored using retrospective surveys (Near and Miceli, 1986, Berrey et al., 2017, Knapp et al., 1997, Bergman et al., 2002, Cortina and Magley, 2003, Cortina and Berdahl, 2008), we are the first to formally test it using labor market variation.[3]

Empirically measuring underreporting is challenging because it is difficult to measure what is not reported. Incidents of sexual harassment filed with the EEOC capture reported prevalence, rather than total prevalence (the sum of reported and unreported sexual harassment). Anonymous surveys have the potential to capture total prevalence, but these are scarce and only infrequently conducted. This has made it difficult to analyze how underreporting varies with the potential costs of retaliation.

In this paper, we examine whether underreporting of sexual harassment rises as the value of outside options falls.[4] In a simple model, we theorize that external forces which reduce the value of becoming unemployed should increase the employee's cost of a retaliatory firing, thus

[3]Consistent with this explanation, but for a different outcome, Johnson et al. (2020) show that serious injuries fall by 12% in states that enacted whistleblower protections in the 1970s and 1980s. Likewise, Sockin and Sojourner (2020) find that fears of employer retaliation discourage negative company reviews and make employees more likely to hide aspects of their identity. Boone et al. (2011) find that workers who report workplace accidents are subsequently more likely to be fired.

[4]This hypothesis is consistent with Lazear et al. (2016), who show that worker effort is positively correlated with local unemployment rates during the Great Recession.

raising the threshold above which a victim is willing to file a charge. The idea is that workers will respond to a reduction in their outside options by only reporting more severe or clear-cut forms of harassment. We show that if workers become more selective about the degree of sexual harassment they are willing to endure before breaking their silence, this implies that underreporting must be rising. Empirically, we make use of the EEOC's determination of whether a case has merit as a proxy for the selectivity or quality of a charge.[5]

Our first analysis of merit exploits variation in unemployment over time at the county level. Using data from 1995-2016, we find that each 1 percentage point rise in the unemployment rate increases the probability a charge is determined to have merit by roughly 0.3%. In contrast, there is less of an increase in EEOC charge quality for other harassment cases (such as racial harassment) and no increase in quality among all other discrimination cases. This highlights that sexual harassment victims' willingness to report is uniquely responsive to economic conditions.

We extend this analysis by documenting facts and providing insights into possible mechanisms. To do this, we link EEOC charge data with information on the gender mix of managers and co-workers within establishments. We build on the work of Folke and Rickne (2022), who show that anonymous reporting increases in the fraction of male colleagues. We find that underreporting, as proxied by selectivity, increases in both the fraction of men occupying supervisory roles and in the fraction of male co-workers. The male-manager underreporting gradient is amplified by a higher unemployment rate. These results highlight the prominent role of power disparities that run along gender lines. This finding is also consistent with Adams-Prassl et al. (2024), who find that when workplace violence is reported to the police, male perpetrators experience less negative labor market outcomes than female victims, and that this differential is almost entirely explained by men being in managerial positions, while female victims are lower in the firm's hierarchy.

[5]The merit variable captures the quality of a case as long as the EEOC applies a consistent standard for determining merit (after netting out geography and time fixed effects). We note that the merit or quality of a case depends not only on the seriousness of harassment, but also how difficult it is to prove.

Our second analysis of merit takes advantage of a plausibly exogeneous change in the generosity of North Carolina's unemployment insurance (UI) program. Since UI partially protects workers from income loss in the event of a retaliatory firing, it follows that UI reductions may stifle reporting behavior. In the aftermath of the Great Recession, North Carolina addressed the looming insolvency of its state UI fund by slashing the maximum weekly benefits available by 35% (from $535 to $350), reducing the duration over which recipients could receive benefits from 26 to 20 or fewer weeks, and imposing additional eligibility restrictions. Consistent with our hypothesis, we find that the selectivity of sexual harassment charges filed in North Carolina subsequently rose by 7 percentage points, representing more than a 30% increase over control states. These effect sizes are three times as large compared to other harassment charges and five times as large compared to other discrimination charges.

While no other states reduced UI benefit levels, six other states also cut benefit durations. For precision, we combine these moderate reform states into a single analysis in a stacked design (Borusyak et al., 2021). We estimate that these UI reforms increased merit by 10% on average, consistent with the cuts being smaller than in North Carolina. We also explore whether victims become more hesitant to report, as measured by filing delays, as UI benefits contract. We find that victims of sexual harassment delay filing with the EEOC by an additional 11% (27 days) following all 7 UI reforms.

Our paper contributes to a nascent literature in economics inspired by the #MeToo movement. Cheng and Hsiaw (2022) develop a theoretical model of reporting sexual harassment in the workplace, where underreporting occurs due to uncertainty about whether other victims within the firm will corroborate patterns of sexual misconduct. Folke and Rickne's (2022) study of sexual harassment and firm gender ratios uses information collected via an anonymous survey, which means there are no labor market consequences for the respondents. We add to their work by underscoring how the incentives of employees to report sexual harassment to authorities depend on potential economic costs.[6] Finally, Sockin et al. (2021)

[6]Consistent with this idea, Grittner and Johnson (2020) show that the Secure Communities immigration enforcement program reduces Hispanic workers' willingness to file complaints about poor working conditions

study changes in non-disclosure agreement (NDA) laws designed to protect trade secrets, but which have also been used to restrict workers from disclosing negative information about the firm. They find that less restrictive laws decrease employee ratings of the firm, and increase the fraction of reviews mentioning words related to bullying or harassment (but not necessarily sexual harassment). Our paper provides a novel way to demonstrate that reductions in outside options suppress the willingness of sexual harassment victims to speak out, and that this silencing effect is amplified by gendered power disparities within the workplace.

We also contribute to the literature on the effects of unemployment insurance. Whereas previous research focuses primarily on the effects of UI generosity on job search behavior and unemployment spells for those who have already lost their jobs, less research has concerned itself with ex-ante moral hazard effects on those who are currently employed.[7] One exception is Lusher et al. (2022), which finds that UI expansions during the Great Recession reduced productivity among currently employed supermarket cashiers and increased shirking behavior more broadly. The effects we find are consistent with such ex-ante moral hazard effects, which materialize in our setting as a reduction in the willingness to report sexual harassment when the generosity of UI benefits falls. While Lusher et al. (2022) shows a negative consequence of UI extensions, we document a positive impact of more generous UI benefits.

The remainder of the paper proceeds as follows. We first provide motivating evidence using Google searches, followed by information on the EEOC's role in curbing sexual harassment and North Carolina's cuts to its UI program. Section 4 describes the datasets and Section 5 provides a simple threshold model of sexual harassment reporting. In Section 6 we detail the identification strategies for our two empirical analyses of merit. Section 7 presents results using unemployment shocks and the impact of gendered power disparities, while Section 8 presents results for the reductions in UI generosity. Section 9 concludes.

and subsequently raises injury rates. In a non-workplace setting, Levy and Mattsson (2019) show that sexual harassment reporting to the police increases in response to the #MeToo social movement.

[7]See Moffitt (1985), Katz and Meyer (1990), Christofides and McKenna (1996), Baker and Rea Jr (1998), Card and Levine (2000), Rothstein (2011), Marinescu (2017), Johnston and Mas (2018), Farber and Valletta (2015), Farber et al. (2015), Schmieder and Von Wachter (2016), Marinescu and Skandalis (2021).

2 Unreported versus Reported Sexual Harassment

Perhaps the most obvious barrier to studying how the underreporting of workplace sexual harassment responds to outside options is that we usually do not observe behavior that is unreported, let alone how it responds to reporting costs. To motivate our analysis, we first look at the volume of Google searches (Google Trends, 2016) for "sexual harassment in the workplace", which are private and anonymous, and hence should track the actual prevalence of sexual harassment. We find that while these anonymous reports increase when unemployment is high, formal charges filed with the EEOC do not mirror this rise.

For each month beginning in January 2004, the Google Search Index (GSI) provides a normalized measure of the number of searches for a particular term over the search period. While the normalization precludes us from learning about the level of sexual harassment, we can use the GSI to measure changes over time and to calculate an elasticity of the prevalence of workplace sexual harassment to macroeconomic conditions. This metric has been deployed in prior work, for example, to estimate how job search intensity responds to both extensions to unemployment insurance (Baker and Fradkin, 2017) and to rollbacks of public health insurance programs (Garthwaite et al., 2014).

Figure 2 shows how the nationally aggregated volume of Google searches varies with the unemployment rate. Strikingly, it spikes by 150% (relative to the January 2004 baseline) just as the national unemployment rate reaches its apex at the height of the Great Recession, after which it declines proportionately with the unemployment rate. In contrast, a measure of the reported prevalence of workplace sexual harassment–the volume of charges filed with the EEOC–exhibits far less sensitivity to macroeconomic conditions. The divergence of these measures of total and reported prevalence indicates that while workplace sexual harassment rose substantially during the Great Recession, underreporting masked much of the increase. This is consistent with the idea that workers become reluctant to report when the economic consequences of retaliation are high, and that firms respond by escalating the degree of sexual harassment to which they subject their employees.

Table 1 reports regression results corresponding to Figure 2. Columns (1) and (3) indicate that each 1 percentage point increase in the unemployment rate raises the value of the Google Search Index by 1.22 percentage points, but increases reported EEOC charges by just 0.05 percentage points. Adding in controls for time trends in seasonal searching or reporting patterns does not materially alter this result, as seen in columns (2) and (4).[8]

Of course, EEOC charges likely reflect a different level of severity relative to Google searches. And we do not know whether the searches were done by the perpetrators or the victims, or the extent to which repeat searches reflect double-counting. For these reasons, the underreporting elasticity should be interpreted with appropriate caution. Nonetheless, the striking departure in the two time series during the Great Recession provides a first indication that underreporting changes with outside options.

3 Institutional and Policy Background

3.1 The EEOC's Enforcement of Illegal Sexual Harassment

The EEOC defines illegal sexual harassment in the workplace as "...unwelcome sexual advances, requests for sexual favors, and other verbal or physical harassment of a sexual nature. Harassment does not have to be of a sexual nature, however, and can include offensive remarks about a person's sex. For example, it is illegal to harass a woman by making offensive comments about women in general."

This definition corresponds to the four types of sexual harassment previously identified as standard in the literature (Fitzgerald et al., 1999, Till, 1980, Folke and Rickne, 2022). These include *sexist hostility*, the expression of insulting or degrading attitudes about men or women, *sexual hostility*, defined as the use of non-physical behavior such as lewd sexual

[8]It is not the case that people simply have more free time during recessions to conduct Google searches. For example, the elasticity of Google searches for "seasonal allergies" with respect to the unemployment rate is 0.01. The result is likewise not driven by two landmark Supreme Court sexual harassment cases (Astra USA v. Bildman in 2009 and Reeves v. C.H. Robinson Worldwide, Inc. in 2010), as removing search terms related to these two cases has little effect on the elasticity.

comments or revealing body parts, *sexual coercion*, the implicit or explicit trades of sexual favors in exchange for rewards or to avoid punishments, and *unwanted sexual attention*, which covers inappropriate touching all the way through groping and sexual assault.

The EEOC considers harassment to rise to the level of illegality if it is "so frequent or severe that it creates a hostile or offensive work environment or when it results in an adverse employment decision (such as the victim being fired or demoted)."[9] Of course, charges may still involve some level of harassment without having exceeded this standard.

The agency which handles the charge may be either the EEOC or a state Fair Employment Practice Agency (FEPA). Our data include charges filed with either agency. Their job is to attempt to conciliate the claim. For simplicity, throughout the remainder of the paper, we refer to an agency as the "EEOC", regardless of whether the agency was the EEOC or a FEPA. If conciliation efforts fail (i.e., no settlement agreement is reached), the agency is responsible for issuing a cause determination. The agency assigns a merit designation to a case if either the company settles with the employee, the claimant withdraws their case upon the receipt of benefits, or the agency makes a determination that the case has reasonable cause following a lengthy legal investigation. A visual depiction of the charge process appears in Figure A1; further details can be found in Dahl and Knepper (2023).

3.2 Cuts to North Carolina's UI Program

From the late 1960s through early 2011, all 50 states and DC paid regular UI benefits to eligible recipients for at least 26 weeks (McHugh and Kimball, 2015). However, in 2011, unemployment trust funds neared insolvency in a number of states amid record benefit pay-outs during the protracted recovery from the Great Recession. In response to this deepening crisis, several states passed restrictive legislation that permanently cut the number of weeks available through regular UI below this long-established norm of 26 weeks (GAO, 2015).

In no state were cuts more draconian than in North Carolina, which was the only one to

[9]See https://www.eeoc.gov/sexual-harassment.

contemporaneously reduce its maximum weekly benefits (by nearly 35% from $535 to $350 per week) while also reducing the maximum allowable benefit weeks available from 26 to 20 (and by even more if the state unemployment rate fell below specified thresholds).[10] The combined effect of these cutbacks was substantial. The maximum level of regular state-level UI benefits available to North Carolinians fell by at least 50%, from approximately $14,000 to $7,000. These changes violated the Congressional non-reduction rule, which made individuals ineligible for the additional 47 weeks of federally-provided UI benefits.

This policy environment is ideal for four related reasons. First, the North Carolina cuts were made on the basis of political forces and insolvency issues surrounding its state UI fund, rather than on local labor market conditions. This is in contrast to state Extended Benefits (EB) and federal Emergency Unemployment Compensation (EUC) programs, which use thresholds of the state unemployment rate or insured unemployment rate to determine the duration of eligibility for UI benefits (Marinescu, 2017, Farber and Valletta, 2015). Unlike many previous studies, we are able to estimate discrete changes in UI generosity when the labor market was recovering rather than languishing.

Second, these cuts were unique in that they were implemented at a time of historically high unemployment. Thus, the strength of the social safety net was a particularly relevant consideration for those considering actions that might jeopardize current employment.

Third, North Carolina's cuts were large and affected both the intensive and extensive margin of benefits. The cuts were so draconian that between April and July of 2013, thousands of protesters organized at the state capitol in Raleigh each Monday (referred to as "Moral Mondays") to voice their disapprobation.[11] Unlike the EUC and EB programs analyzed in past work, the North Carolina cuts affected not just duration and generosity (i.e., the intensive margin) but also reduced eligibility (the extensive margin). Extensive margin

[10]Following the reform, maximum duration drops to 12 weeks when the state UR rate is less than 5.5%, and increases by 1 week for each 0.5% increase until reaching 20 weeks once the UR reaches 9%. North Carolina also restricted access to the program by disqualifying individuals who had lost a job for "good cause," such as providing family caregiving. The waiting period was also increased.

[11]See https://www.usatoday.com/story/news/nation/2013/07/21/north-carolina-unemployment/2571889/.

cuts are likely to be particularly salient for women worried about a retaliatory firing.[12]

A fourth reason to study North Carolina's UI experiment is that the UI reductions were permanent and affected the first source of benefits which claimants draw from. Only after exhausting regular UI benefits are individuals eligible for EB and EUC. Thus, access to regular state UI benefits is relatively more valuable as they are discounted less heavily.

Figure A2 provides empirical evidence that North Carolina's reform was highly salient. The figure displays the normalized number of Google searches for the phrase "unemployment benefits" in North Carolina and in other Southern States. First, searches in North Carolina differentially rise soon after February 2013 when the North Carolina legislature passed the bill. Searches in North Carolina spike again in July 2013 when the law becomes effective, a phenomenon not mirrored in control states. Lastly, searches increase more in control states than in North Carolina when the Federal EUC program expires in December 2013. This is sensible, as the UI reform had already rendered North Carolinians ineligible for federally extended benefits. While the Federal EUC's expiration was still a prominent national news story, it was less relevant for North Carolina.

One caveat is that North Carolina also reduced corporate and personal income taxes soon after the UI reform. However, this tax reform should have increased both labor demand and labor supply (as long as substitution effects dominate income effects), and so we interpret our estimate as a lower bound of the effect of the UI reform. Six other states–Florida, Georgia, Kansas, Michigan, Missouri, and South Carolina–also cut potential benefit durations from 26 to 20 weeks, but did not reduce weekly benefits. To assess robustness, we also study how the selectivity of sexual harassment charges changes in these moderate reform states.

In spite of the many advantages of studying changes to UI generosity generated from legislative rollbacks, there is limited research leveraging this type of variation. A notable exception is Johnston and Mas (2018), which finds a benefit cut in Missouri reduced unem-

[12]Prior research has shown that currently employed workers' labor supply is responsive to changes in UI generosity (Light and Omori, 2004). Christofides and McKenna (1996), Baker and Rea Jr (1998) document employment responses on the extensive margin of UI eligibility.

ployment spells and moderately reduced the state's overall unemployment rate.

4 Data and Sample Construction

This paper leverages several novel data sources. We combine data on the quality (and volume) of individual sexual harassment reports filed with the EEOC (Equal Opportunity Employment Commission, 2016) with three data sources to study how economic conditions, workplace characteristics, and social safety net programs each influence incentives for victims to report. First, we use contemporaneous estimates of local labor market conditions with data produced by the Bureau of Labor Statistics (BLS) (Bureau of Labor Statistics, 2016b). We also link the charge data to the EEO-1 files collected by the EEOC (Equal Opportunity Employment Commission, 2015), which detail the racial, gender, and occupational distribution of workers in private establishments. We additionally link the charge data with publicly available information on the timing and severity of the cut to North Carolina and other states' UI programs. Various crosswalks are used to to conduct analyses at different levels of aggregation (U.S. Department of Agriculture Economic Research Service, 2000, U.S. Department of Housing and Urban Development, 2010, U.S. Census Bureau, 2019).

4.1 EEOC Charge Data

Our study draws on the universe of all sexual harassment charges filed with the EEOC between January 1995 and September 2016.[13] We restrict our sample to the 194,879 charges (out of 245,975 total) that include the victim's demographic information given that harassment risk varies substantially by sex and age (Hersch, 2011, 2018). These data provide an alternative measure of discrimination compared to what is commonly used in the economics literature, such as wage and employment gaps.

Moreover, these EEOC data include the resolution associated with each charge, which

[13]The data provided to us by the EEOC exclude charges between October 1995 and September 1996, and between October 2010 and September 2011.

reveals whether the EEOC determined that the charge had reasonable cause. This merit variable then serves as a proxy for the quality of each sexual harassment charge filed, allowing us to test whether individuals become more selective about reporting. While a few others have used these or the nationally aggregated version of these data to study sexual harassment in the United States (Hersch, 2011, 2018, McCann et al., 2018, Cassino and Besen-Cassino, 2019), they have focused on the volume of charges, whereas we focus on merit.

Table 2 provides several pieces of evidence that highlight just how widespread the underreporting problem is, and a hint for the mechanism. Based on all charges filed between 1995 and 2016, we find that, compared to other discrimination charges, sexual harassment charges are 40% more likely to have been meritorious (25.4% versus 18.1%) and more than two-and-a-half times as likely to have involved employer retaliation (57.6% versus 22.4%). These differences accord with earlier work from a survey of public employees showing that 75% of those who report sexual harassment face retaliation (Cortina and Magley, 2003). As further evidence that sexual harassment cases are more egregious than other discrimination filings on average, they are nearly twice as likely to have private legal representation (13.1% compared to 6.8%).

An alternative explanation for why merit and retaliation rates are higher for sexual harassment than for other discrimination charges is that making an internal report before filing a formal charge with the EEOC increases the odds of success in harassment cases. Specifically, the two 1998 Supreme Court cases of *Faragher v. City of Boca Raton* and *Burlington Industries, Inc. v. Ellerth* established the "Faragher-Ellerth defense," which requires that an employer first be made aware of the harassment through an internal report if they are to be held liable for not having taken corrective actions.[14] If charges that cannot be resolved internally are more likely to be meritorious, then the selection of which types of cases make it to the EEOC could also explain the difference in merit rates.

However, if this were the primary explanation, we would expect to see similarly high

[14]See https://www.eeoc.gov/harassment.

merit rates for other forms of harassment (for example, race-based harassment), which are also subject to the internal reporting precedent. But as column (3) shows, sexual harassment merit rates are still approximately 33% higher than for other types of harassment. Likewise, retaliation is 23% higher for sexual harassment. In Appendix A.2, we discuss in more detail the reasons why sexual harassment is likely to be different from both other harassment cases as well as non-harassment discrimination cases, including the role of power disparities and the difficulty in proving wrongdoing.

Table 2 also shows that 84% of sexual harassment complainants are female (compared to 52% of non-harassment victims and 56% of other harassment victims), and they are younger than victims of other forms of discrimination.

We provide additional context and statistics on EEOC retaliation charges in Table A1. Retaliation occurs when an employer punishes an employee in response to engaging in an EEO "protected activity."[15] Each complainant indicates whether the employer took any retaliatory actions in her EEOC charge. The employee is fired in 70% of sexual harassment charges involving retaliation. Other less common types of retaliation include disciplining the employee, reducing or withholding wages, suspending the employee, and not promoting or demoting the employee.

4.2 County Unemployment Data

We measure local variation in the number of unemployed individuals at the county-month level using county-level BLS Local Area Unemployment Statistics (LAUS) (Bureau of Labor Statistics, 2016c). Our time period spans 1995 through 2016. This period provides rich variation in exposure to local labor demand shocks, as these years include two distinct recessions with strikingly different heterogeneity across geography (Hershbein and Stuart, 2020).

[15] See https://www.eeoc.gov/retaliation for more details.

4.3 EEO-1 Employment Files

We further link EEOC charge data with EEO-1 files, which provide a rich census of all private establishments in the United States for firms with at least 100 employees, and account for 8.5 million establishment-year observations. The EEO-1 data cover approximately 40% of all employees in the country and detail the gender and occupational distribution of workers within an establishment. We are able to link roughly one quarter of the EEOC sexual harassment charges to this data.[16]

Table 3 compares establishments which had a sexual harassment charge filed against them by a female, some other discrimination charge filed against them by a female, or did not have any charges filed against them in a given year. Establishments with sexual harassment charges versus other charges have 8.4 percentage point more male employees and 9.2 percentage point more male managers. Comparing the first two columns to column (3) where no charges are filed, there are differences by race, firm size, and geography, with the geographic distribution matching those of labor rights violations (Marinescu et al., 2020).

4.4 Unemployment Insurance Data

To estimate the impact of North Carolina's UI reform, we combine Department of Labor administrative data (U.S. Department of Labor, 2016) with monthly Current Population Survey data (IPUMS-CPS, 2016) to calculate short-term UI recipiency rates. Our measure divides the monthly number of individuals receiving weekly UI benefits by the number of short-term unemployed–defined as those unemployed for 26 weeks or fewer–as in Schaefer and Evangelist (2014). This serves as a proxy for the ex-ante probability of receiving short-term unemployment benefits in the event that a worker files a sexual harassment charge and is subsequently dismissed.

[16]We cannot match all charges for two primary reasons. First, EEOC federal anti-discrimination laws cover all employees at establishments with 15 or more employees, but requires EEO-1 surveys only from those establishments whose firm has 100 or more employees or federal contractors who have 50 or more employees. Second, the EEO-1 files cover only private establishments, whereas approximately 15% of discrimination charges are leveled against public-sector firms.

In Figure 3, we compare the short-term UI recipiency rate in North Carolina to other Southern states that had no changes to their UI programs. While the two rates track one another closely prior to North Carolina's reform implementation date, they diverge sharply thereafter. The fraction of short-term unemployed receiving UI benefits in North Carolina drops from 33% to 10%, a 59% decrease relative to control states. We leverage this shock to study how the value of outside options influences the willingness to report sexual harassment.

5 Model

This section outlines a simple two-stage threshold model of sexual harassment reporting behavior. The model predicts that when the cost of becoming unemployed is higher, the harassment threshold above which a worker will report sexual harassment to the EEOC rises. The intuition is that victims will remain silent rather than report and potentially jeopardize their employment at a time when alternate labor market opportunities are limited. Our model builds on ideas in Boone and Van Ours (2006), which explores the willingness of workers to report occupational injuries.

To start, consider the internal reporting decision of a worker. In cases of workplace harassment, before filing an EEOC claim, the worker will often first make a report to the firm (usually the HR department) so the firm has an opportunity to remedy any possible wrongdoing. If the worker does not follow internal reporting procedures, the firm will often be able to avoid liability. We therefore model all workers as first filing internally, although the predictions continue to hold if individuals file directly with the EEOC. In what follows, we use the subscript "1" to refer to the internal reporting stage and the subscript "2" for the EEOC reporting stage.

We define $\alpha \geq 0$ as the level of sexual harassment a firm engages in against an employee. The net psychological cost of reporting c_1 is a random variable which differs across workers. Given α, the worker decides whether to report internally. Should the worker endure the

harassment and not report, she receives a payoff of w, her wage, minus α.

If the worker instead reports, she incurs costs c_1 and with probability $p_1(\alpha)$ the hostile work environment is eliminated, with $\delta p_1(\alpha)/\delta\alpha > 0$. However, such a worker also faces the possibility of a retaliatory firing with probability θ_1. If the worker is fired, she finds a new job with probability $q(u)$ which depends negatively on the current unemployment rate u. If she fails to find a job, she receives unemployment benefits. The value of becoming unemployed is the weighted average of the wage at a new job, w', and the unemployment benefit, b: $V_1^u = q(u)w' + (1 - q(u))b$. The value of remaining employed is the worker's wage at her current job, w, minus the amount of harassment she endures, which will be eliminated if her case is successful: $V_1^e = w - (1 - p_1(\alpha))\alpha$.

The worker also takes into account the option value of being able to file an EEOC claim after an internal report if the firm does not eliminate the harassment. Thus, a worker will file an internal report if the expected value of filing exceeds the payoff of remaining silent:

$$\theta_1 V_1^u + (1 - \theta_1)V_1^e - c_1 + \textit{option value} \geq w - \alpha \tag{1}$$

The second stage of the model considers whether the worker files a formal claim with the EEOC.[17] The probability of winning an EEOC case is allowed to depend on whether there was a retaliatory firing in the first stage, $p_2^{nr}(\alpha)$ or $p_2^r(\alpha)$, where the superscripts nr and r denote no retaliation and retaliation, respectively. Likewise, the monetary payout translated into utility terms is $\gamma^{nr}(\alpha)$ or $\gamma^r(\alpha)$. All of these functions are assumed to be increasing in α. There are two cases to consider.

Case i: First, consider a worker who retained her job after filing an internal report, but did not have the harassment eliminated. If they report to the EEOC, they face a second threat of a retaliatory firing. They will choose to file a case with the EEOC if the expected net benefit exceeds the payoff of remaining silent:

$$\theta_2 V_2^u + (1 - \theta_2)V_2^e - c_2 + \psi^{nr}(\alpha) \geq w - \alpha \tag{2}$$

[17]For simplicity, the firm makes a binary choice of whether to eliminate sexual harassment. Allowing the firm to eliminate part of the harassment complicates the model without yielding additional insights.

where $\psi^{nr}(\alpha) = p_2^{nr}(\alpha)\gamma^{nr}(\alpha)$. Notice the similarity between this equation and equation 1. The key difference is that instead of having an option value term, the equation includes the expected EEOC financial payout from filing a case. V_2^u and V_2^e are defined as in the first stage, but with $p_2^{nr}(\alpha)$ instead of $p_1(\alpha)$ appearing in V_2^e.

Case ii. Now consider the other case, where the worker experiences a retaliatory firing after an internal report. An individual will file a case with the EEOC if:

$$\psi^r(\alpha) - c_2 > 0 \tag{3}$$

where $\psi^r(\alpha) = p_2^r(\alpha)\gamma^r(\alpha)$.

The comparative statics of this model are worked out in Appendix A.1. The testable implication is that merit rates should be higher when unemployment increases or UI benefits are reduced. The intuition is that the severity of EEOC reported cases (and hence merit) increases since (i) there is positive selection of cases which make it to stage 2 and (ii) conditional on making it to stage 2, the threat of a retaliatory firing makes filing more costly in a high unemployment rate environment.

The appendix also presents a model for firm behavior which is consistent with these comparative static predictions (Appendix A.2). We assume a piecewise profit function which is concave in the severity of workplace sexual harassment and convex in the costs of being reported.[18]

In our model, the threat of a retaliatory firing in each stage (θ_1 and θ_2), serves as a worker discipline device to discourage reporting. This follows the logic in Chassang and Padró I Miquel (2019) and Shapiro and Stiglitz (1984). For simplicity, we do not model this idea formally. However, we do note that this device is likely to be more relevant for disciplining the reporting of sexual harassment compared to other forms of discrimination.

One reason is that sexual harassment is arguably more likely to be perpetrated by managers against subordinates. If the perpetrator is more important to firm profits, the firm has

[18]Profit could depend on the level of harassment for more than one reason. It could harm productivity of directly harassed workers and their co-workers. However, if highly productive managers or coworkers gain utility from committing harassment, it could also be costly to eliminate.

an incentive to protect the perpetrator and fire the worker. Knowing this, the worker is less willing to report sexual harassment, since they will be more likely to become unemployed. Moreover, a rising unemployment rate is likely to shift power from workers to managers. Another reason is that sexual harassment is more difficult to verify. Sexual harassment often happens when no one else is looking, and hence cases can devolve to arguments of "she-said, he-said". The fact that sexual harassment is hard to verify makes it more risky to pursue a claim when unemployment is high, which could embolden the firm to retaliate more often.[19]

Both of these reasons imply that retaliation will be used more often in cases of sexual harassment. Indeed, as Table 2 shows, retaliation is more common in sexual harassment claims (58%) versus non-harassment claims (22%) filed with the EEOC. Sexual harassment is also 23% more likely to involve retaliation compared to other forms of harassment, such as racial harassment. When the value of outside options falls, sexual harassment victims will be relatively less willing to report because they face a higher retaliation rate. A testable implication is that merit rates for sexual harassment should be more responsive than for other harassment, with non-harassment charges being the least responsive. Absent any firm response, reported volume should fall most for sexual harassment, followed by harassment, and then other forms of discrimination.

6 Empirical Framework for Merit Analyses

6.1 Labor Demand Shocks and the Selectivity of Charges

We now discuss how we use EEOC charge data to better understand underreporting. We estimate the effect of local labor market slackness on the selectivity/quality of sexual harassment charges filed with the EEOC much in the spirit of Maestas et al. (2021), who study the relationship between labor market conditions and disability insurance claims at the state-

[19]Two other reasons sexual harassment could be different is that the psychological costs of reporting for the worker or the reputational costs for the firm could be higher. However, it is not clear why these factors should vary with the business cycle.

month level. Building on this approach, we exploit heterogeneity in unemployment across counties at the monthly level. As we show in robustness checks, re-defining the relevant labor market to be either the commuting zone or state-industry yields similar results.

Using county variation offers several advantages over state-industry variation. First, relying on county variation allows us to use the full sample of nearly 195,000 sexual harassment charges rather than the 91,000 observations for which industry codes are available. Second, this approach sidesteps any selection issues which may arise if women avoid or exit certain industries due to elevated harassment risk (Batut et al., 2021). Relatedly, using counties does not assume that the set of relevant outside options must be in the same industry.

Our primary hypothesis concerns how the selectivity of sexual harassment reports responds to the health of the local labor market. We regress an indicator for whether charge i in county c in month t receives a merit designation by the EEOC, $merit_{ict}$, on the number unemployed in a county:

$$merit_{ict} = \beta U_{ct} + \tau_c + \phi_t + \delta X_i + \epsilon_{ict} \tag{4}$$

U_{ct} represents the number of unemployed individuals in county c. X_i measures the race, age, and sex of the charging party, though our results are invariant to the inclusion of these covariates. We control further for τ_c which capture fixed effects for county and ϕ_t, which capture fixed effects for month-year by agency (EEOC or FEPA). In doing so, we absorb heterogeneity in charge selectivity that is unrelated to negative labor demand shocks at the county level, or part of a broader national trend. In all specifications, we cluster our standard errors at the county-level.

Following Maestas et al. (2021) and Dahl and Knepper (2023), our main independent variable of interest is the number unemployed, rather than the unemployment rate. We do this to sidestep any potential confounding effects introduced by county-time differences in the size of the labor force. However, as we show, our results are robust to using rates.

6.2 North Carolina Reform and the Selectivity of Charges

We also estimate how the reductions in North Carolina's UI program affected the quality of sexual harassment charges filed. The reduced form regression is:

$$merit_{ist} = \beta \left(North\ Carolina_{is} \times post_{st} \right) + \tau_c + \phi_t + \epsilon_{ist} \tag{5}$$

The coefficient of interest is β, the effect of the North Carolina UI reform on the merit of sexual harassment charges. The dummy variable "post" indicates whether t is after July 2013, which is when the law became effective. As before, τ_c and ϕ_t are county and month-year by agency fixed effects. Because this is a difference-in-differences design, identification requires that the quality of sexual harassment charges filed in North Carolina in the absence of the reform would have evolved identically as in the control states (other Southern states). We also present yearly event study estimates and document parallel pre-trends. Our control group of other Southern states excludes four Southern states which changed their UI programs (AR, FL, GA, SC). It includes those from the South Atlantic (DE, DC, MD, VA, WV) and East South Central Census Divisions (AL, KY, MS, TN).

We estimate an instrumental variable (IV) regression to scale the magnitude of the reduced form effect. To do this, we run a first stage regression of the short-term UI recipiency rate in a state and year on the North Carolina post-reform dummy. The UI recipiency rate captures the likelihood an individual receives short-term support in the event she is fired. The parameter of interest is the elasticity of the quality of sexual harassment charges to the probability of receiving short-term UI following the NC reform. Our theoretical model predicts that this elasticity will be negative.

We also estimate the effect for the other six states which cut the maximum duration of regular UI benefits from 26 to 20 weeks, but which did not alter weekly benefit amounts (FL, GA, KS, MI, MO, and KS). Because these cuts were less severe, for precision, we combine all these states into a single regression. As these reforms were implemented in a staggered fashion, we estimate effects in event time using a difference-in-differences framework following

Borusyak et al. (2021).

7 Results Using Unemployment Shocks

This section reports our empirical findings for unemployment shocks. We first assess whether workers report sexual harassment more selectively during recessions. We then extend this analysis by documenting facts related to the gendered nature of workplace sexual harassment and providing insights into possible mechanisms.

7.1 Unemployment and the Selectivity of Charges

Figures 4a-4c depict graphically the relationship between the unemployment rate in a county-month cell and the fraction of EEOC charges with merit after residualizing out the controls and fixed effects included in equation 4. The binned scatterplots are constructed separately based on whether the charge includes a sexual harassment claim (red triangles) or not (blue squares). Panels b and c show the relationship between merit and unemployment rates for all other charges not involving harassment and for all harassment charges. As a reminder, our main prediction is that as outside options fall, the merit rate of sexual harassment should rise. This is due to the threat of a retaliatory firing in response to either an internal report or an EEOC filing. The response will be larger for sexual harassment cases compared to other charges if retaliatory firings are a more commonly deployed and effective worker discipline device due to power disparities.

The results indicate that among sexual harassment charges, there is a positive relationship between merit and unemployment. This accords with the prediction that a reduction in outside labor market opportunities causes workers to raise the threshold level of sexual harassment they will tolerate before filing a report. The relationships for all non-harassment charges and for other harassment charges are comparatively flat, indicating that the selectivity of sexual harassment charges is uniquely responsive to worsening economic conditions.

Column (1) of Table 4 reports regression estimates. To interpret magnitudes, we report both the percent change for a 1 percentage point increase in unemployment as well as an elasticity with respect to unemployment. To calculate the percent change, we first compute the effect of a 1 person increase in the number unemployed in a county-month on the fraction of sexual harassment charges with merit, controlling for county and time fixed-effects (and the victim's race, sex, national origin, and age). We then multiply β by 1% of the size of a labor force in a county-month cell, which is approximately 447,000. We find that each one percentage point increase in the local unemployment rate increases the fraction of charges with merit by 0.00074 percentage points. Finally, we divide by the mean merit rate. To calculate the elasticity, we multiply the regression coefficient by the average number unemployed in a county-month cell. We then divide this by the mean merit rate.

The estimate in column implies a 0.29 percent increase and an elasticity of 0.017. During the Great Recession unemployment rose from 4.5% to a 10%, so that from trough to peak, our estimates imply that merit increased by 1.6%. In column (3), we repeat this exercise for the set of charges that do not include any harassment claim. Consistent with Figure 4b, we find no effect on merit in this sample. This null effect rules out common changes in the labor market as the driver of our findings for sexual harassment, such as an expanded pool of potential replacement workers causing managers to treat workers worse in general.

In column (5), we find that the merit in other harassment charges increases in unemployment by half as much as it does for sexual harassment charges, consistent with the threat of a retaliatory firing causing workers to report more selectively than in non-harassment cases (where the likelihood of retaliation is lowest), but not as selectively as in sexual harassment cases (where the likelihood of retaliation is highest). This implies that the internal reporting precedent can explain at most half of our result for sexual harassment charges, as victims of other forms of harassment face equally strong incentives to submit an internal report to HR before filing an EEOC charge.[20]

[20]These results also suggest that our findings are not driven by firms taking internal complaints more seriously when workers have good outside options (e.g., Dube et al., 2022).

One important point on interpretation is that we cannot separately identify how much of the merit increase is directly due to victim underreporting, and how much arises from firms' engaging in more severe sexual harassment in response to this increased reluctance to report. That said, neither of these effects would arise in the absence of increased underreporting.

While our focus is on charges involving sexual harassment, other issues and bases may be raised simultaneously. The other most common charge issues include discharge, hiring, demotion, discipline, harassment, intimidation, promotion, reasonable accommodation, suspension, terms and conditions, and wages. Other top bases include disability, age, race, sex, national origin, and retaliation. Hence, it is possible that the increase in merit is not due to the sexual harassment issue, but rather due to other issues and bases filed simultaneously. In columns (2), (4), and (6), we control for these other case characteristics (which are potentially endogenous), and find that they leave the coefficient on the main independent variable virtually unchanged for sexual harassment, though it does shrink modestly for non-harassment and other harassment charges.

Particularly relevant for our setting, firms may be more inclined to fire (i.e., discharge) or otherwise retaliate against a worker who complains during a slack labor market, since she will be easier to replace. This, in turn, would give the employee an additional claim of liability, possibly increasing the merit rate. In such a scenario, the countercyclical increase in the selectivity of sexual harassment charges would be due to the employer's response to the report rather than the victim's reluctance to file. However, Table 5 shows that the estimated effect is positive even when the case does not involve a discharge (column 2), retaliation (column 4), or either discharge or retaliation (column 6).

In Figure 5, we perform a different exercise, comparing the merit response for sexual harassment relative to all other top issues and bases. We run separate models on the EEOC sample for each of the 18 most commonly raised issues and bases, adding fixed effects for the other issues and bases in each regression. The figure plots the scaled estimates, which can be interpreted as elasticities. The sexual harassment elasticity is among the largest, and

the only one which is greater than 0 at the 5% significance level. This is true whether the non-sexual harassment categories exclude (Figure 5) or are allowed to include (Figure A3) sexual harassment as an issue.

One alternative explanation for the merit result is that employers or workers are more likely to settle during a depressed economy to avoid costly litigation and a potential loss in court. Recall that a charge is categorized as meritorious by the EEOC either if the firm and claimant reach a settlement voluntarily or if the EEOC's investigation determines that the case has reasonable cause after a lengthy investigation. As another robustness exercise, in Table A2 we run separate models where we estimate the effect of unemployment on the likelihood of either a reasonable cause ruling or the likelihood that the firm and worker settle. We find a large and statistically significant increase in reasonable cause rulings by the EEOC (elasticity = .036) in column (1) but no statistically significant effect on settlements (elasticity = .010) in column (3). A reasonable cause determination occurs only if there is no voluntary settlement, so in column (2) we condition on no settlement and continue to find a sizeable elasticity (0.040). For charges involving discharge or retaliation (columns 4-6) as well as those excluding them (columns 7-9), we continue to find that the increase in merit we document is driven by reasonable cause rulings rather than an uptick in settlements.

The availability of prosecuting lawyers could also vary with the business cycle, but we find no evidence of this. Using our baseline specification but with the victim's use of a lawyer as the dependent variable, we find a precise zero: a 1% increase in unemployment is associated with a .07% decrease (p-value=.41) in the use of lawyers by victims.

7.2 Robustness to Different Measures of Labor Market Tightness

We now evaluate the sensitivity of our main results to the use of alternative measures of labor market tightness. First, we use the number unemployed at the commuting zone (CZ) level, U_{zt}, which can be extracted directly from the BLS LAUS data. There are 709 CZs,

which are comprised of contiguous counties.[21]

Second, we use monthly heterogeneity in exposure to downturns across states and industries, as in Dahl and Knepper (2023). We use sector as our measure of industry, given that this is the level for which national unemployment statistics are available. These 20 sectors map to specific 2-digit NAICS codes.[22] We combine data on state-month unemployment from the LAUS with national industry-month unemployment and state-industry-month employment from the Quarterly Census of Employment and Wages (QCEW) (Bureau of Labor Statistics, 2016d) to impute U_{jst}, the number of unemployed individuals in a state-industry-month.

To impute U_{jst} we first write the number of unemployed in each state s in time period t, U_{st}, as the weighted sum of industry-specific unemployment j within that state:

$$U_{st} = \sum_{j=1}^{J} w_{jst} U_{jst} \tag{6}$$

The weights, w_{jst}, equal each industry's share of total state employment in a month, and are calculated directly from the QCEW. We then impute U_{jst} by setting industry j's employment-weighted share of unemployment in state s in period t to the analogous employment-weighted share of unemployment at the national level n. This proportionality assumption can be expressed as:

$$w_{jst} \times \frac{U_{jst}}{U_{st}} = w_{jnt} \times \frac{U_{jnt}}{U_{nt}} \tag{7}$$

Finally, U_{jst} can be imputed by solving as a function of these known quantities, which are available from either the QCEW or BLS's Current Employment Statistics.

Using variation in unemployment measured at the CZ level, we find similar results compared to using county-level variation. Figure A4 visually shows how merit varies with CZ unemployment, separately for sexual harassment (panel a), non-harassment charges (panel

[21] We thank the editor and an anonymous referee for suggesting the use of CZ variation.

[22] In a few cases, sectors represent combinations across multiple 2-digit NAICS codes. Specifically, these are Manufacturing (codes 31-33), Retail Trade (codes 44-45), and Transportation and Warehousing (codes 48-49). For details, see https://www.census.gov/programs-surveys/economic-census/guidance/understanding-naics.html.

b), and non-sexual harassment charges (panel c). Similar to Figures 4a-4c which are based on county-level variation, merit increases sharply with local unemployment for sexual harassment, while the relationships for non-harassment and other harassment charges are comparatively flat. As Table A3 shows, a 1 percentage point increase in CZ unemployment results in a 0.29 percent increase for sexual harassment, which is very close to the effect found for counties. Figure A5 and columns (4)-(6) of Table A3 perform a similar exercise at the state-by-industry level. We again find a significant percent increase for sexual harassment cases (0.52). Using state-by-industry variation, we also find increases for the other types of discrimination cases.

As a further robustness exercise, in Table A4, we use unemployment rates instead of levels. Column (1) reveals that each one percentage point increase in the county unemployment rate increases the fraction of charges with merit by 0.40 off a baseline of 25.5%, which translates to a 1.6% increase. Results are roughly twice as large in column (2) when we use variation at the CZ level, and roughly half as large in column (3) when we use state-industry variation.

In Table A5, we re-estimate our main specifications, except clustering by the receiving EEOC office to allow for any arbitrary correlation in how merit decisions are made. The standard errors remain approximately the same. Lastly, Table A6 directly controls for the imputed number of layoffs and discharges from the experimental state-by-month Job Openings and Labor Turnover Survey (JOLTS) data (Bureau of Labor Statistics, 2016a). This accounts for the possibility that our results are driven not by a reduction in outside options but rather by a larger fraction of the workforce being laid off and simultaneously filing sexual harassment charges. However, the merit estimate is virtually identical to the baseline, though is imprecisely estimated.

7.3 Labor Demand Shocks and the Volume of Charges

We now turn to how the volume of charges changes with labor market conditions. The simple model we wrote up in Section 5 implies that charge volume would decrease when the

labor market is slack, absent any firm response. However, if firms endogenously respond to a higher threshold by raising the severity of sexual harassment, whether volume increases or decreases depends upon the distribution of reporting costs for the worker relative to the derivatives of expected firm benefits and costs for each charge type. Therefore, we do not have as clear a prediction for volume as we do for merit.

Figures 4d-4f graph the relationship between the volume of charges filed and county unemployment rates after residualizing out time and county fixed effects. The binned scatterplots are constructed separately based on whether the charge involves sexual harassment (red triangles) or not (blue squares). Panels e and f show the relationship between volume and unemployment rates for all other charges not involving harassment and for all harassment charges which do not involve sexual harassment, respectively. While we find that volume increases in the unemployment rate most strongly for other charges, the relationship is flatter for harassment charges, and the most flat for sexual harassment charges.

Table 6 uses our baseline regression to more precisely characterize the relationship between unemployment and charge volume by type of charge (SH, O, or H). The volume of SH charges decreases in unemployment, with an elasticity of -0.107.[23] As a reminder, the increase in merit for SH found in Table 4 provides evidence that victims are less willing to report when unemployment is high. The drop in SH volume reinforces this conclusion. For other discrimination charges not involving harassment, volume increases, with an elasticity of 0.054. Likewise, other harassment also increases, but is noisily estimated. The combined patterns for merit and volume in Tables 4 and 6 are consistent with more severe underreporting for sexual harassment relative to other forms of workplace discrimination as the labor market worsens.

Table A7 reports volume results when using CZ or state-by-industry variation. For sexual harassment, the estimate is negative and insignificant (using CZ variation in column 1 or

[23]Cassino and Besen-Cassino (2019) and Juban and Wallace (2005) analyze how the volume of sexual harassment charges responds to unemployment at the national level, reaching opposite conclusions from one another.

state-by-industry variation in column 4), and always has the lowest elasticity compared to other types of non-harassment charges and other harassment charges.

Sexual harassment cases are often co-filed with a discharge or retaliation claim. If discharges and retaliation are more common when the labor market is weak, ceteris paribus, co-filings should increase. In Table A8, we find that they are not more common in an absolute sense, although the volume of charges involving discharge, retaliation, or their union are not as negatively impacted by increased unemployment.[24]

7.4 The Impact of Gender in the Workplace Using Linked Data

We now document several facts about workplace sexual harassment and explore potential mechanisms. To do this, we link in data on the gender distribution of workers at the establishment level. We find evidence that the gender make-up of a workplace plays a prominent role in determining the selectivity of sexual harassment charges. Since 84% of all victims in our data are female, this subsection focuses on women.

Figure A6 shows that the selectivity of charges is monotonically increasing in the fraction of male workers at an establishment. This suggests that in male-dominated environments, female employees become increasingly reluctant to report despite an increase in volume. The volume result accords with Folke and Rickne (2022); a measure of selectivity is not contained in their dataset, so no comparison is possible on that margin. The figure also explores whether gendered power dynamics (Hesson-Mcinnis and Fitzgerald, 1997, MacKinnon, 1979) play a role. We find the selectivity of charges filed by females is increasing in the fraction of managers who are male. Combined, these patterns suggest that a lack of women in power contributes to an increased fear of reporting workplace sexual harassment due to a fear of retaliation. Adams-Prassl et al. (2024) offers a related explanation of why this might be so–that is, male managers tend to face less severe consequences than their female victims in

[24]Relatedly, the fraction of sexual harassment charges with a retaliation or termination is not responsive to unemployment. Using our baseline specification, a 1 percentage point increase in unemployment reduces the fraction of sexual harassment charges with a retaliation or discharge by 0.05% (p-value = 0.18).

cases of workplace violence.

Table 7 explores the extent to which our main results are driven by gendered power disparities within a workplace. It could be, for example, that rising unemployment further shifts power from workers to managers, which would make female victims more reluctant to report due to concerns of retaliation in either male-dominated environments or when men are more likely to have influence over their employment status. We re-run our merit regressions, interacting the number unemployed with (i) the percent of men in a workplace and (ii) the extent to which males dominate managerial roles within a workplace. A one standard deviation increase in these margins amplifies the underreporting effect due to one percentage point higher unemployment more than twofold–by 0.37% (column 1) and 0.33% (column 4), respectively. These results suggest that the threat of retaliation from male colleagues and male managers becomes an increasingly effective deterrent to reporting sexual harassment as outside labor market opportunities become scarcer.[25]

When restricting attention to discrimination charges involving non-harassment or other harassment charges, this effect is close to zero and insignificant for the % men interaction (columns 2 and 3) and the % male managers interaction (columns 5 and 6), though is consistently more positive for other harassment charges. The dramatic difference between the sexual harassment and other discrimination charge estimates is statistically significant at the 1% level. Taken together, it appears power dynamics play a uniquely large role in women's reluctance to report sexual harassment. In terms of volume, sexual harassment falls the most in response to a higher fraction of men or male managers when unemployment increases, although the estimates are relatively imprecise (Table A10).

What still remains to be determined, however, is the nature of how sexual harassment differs from other forms of harassment in a way that could explain this differential response. For example, one possibility is that sexual harassment may be more likely to be perpetrated against subordinates. Another is that the power gap between men and women in establish-

[25]Table 7 includes controls for % male managers in columns 1-3 and for % men in columns 4-6. As Table A9 shows, excluding these controls has little effect on the estimates.

ments where sexual harassment is occurring is larger than it is for other classes of workers subject to harassment (e.g., on the basis of race or age).

To disentangle these channels, we first replicate the preceding analysis for racial and ethnic discrimination. First, we document that sexual harassment charges are 26% more likely to involve retaliation than are racial or ethnic discrimination charges (58% versus 46%). This fact is consistent with the manager-subordinate relationship being more relevant among sexual harassment charges, as retaliation is a viable tool only when the perpetrator is more "important" to the firm in an economic sense. Second, Figure A7 shows that the merit rate for ethnic or racial discrimination charges is comparatively flat in percent coworkers white (panel a) and percent managers white (panel b), which provides additional evidence that sexual harassment is unique in that it is more likely to arise from power disparities. In fact, among establishments with a female sexual harassment charge, women are underrepresented in managerial positions by 27%, while among establishments with a racial or ethnic harassment charge, non-whites are underrepresented in managerial positions by 48%. Together, these observations suggest that the power gap between men and women is indeed lower than it is for whites and non-whites, but conditional on a power gap, sexual harassment appears to be more likely to be perpetrated against subordinates than other forms of harassment.

Lastly, we explore the extent to which the effect of unemployment on merit in cases of racial discrimination is mediated by the percent white or percent white managers. Unlike with sexual harassment, Table A11 shows that the unemployment effect, if anything, falls as the minority group becomes smaller or more likely to be supervised by a white manager. In Table A12, we find the volume response to higher unemployment decreases with a higher fraction of whites or white managers. These results are consistent with a story where the threat of retaliation is less prominent in cases of racial or ethnic harassment.

8 Results Using Unemployment Insurance Reforms

In this section we report empirical findings for North Carolina's UI reform, followed by results for states which reduced their benefits more modestly. We show results in event-study figures and estimate difference-in-differences regressions, as well as IV estimates which scale the underreporting effect of the reform by the change in the UI recipiency rate.

8.1 Reduction in UI Benefits and the Selectivity of Charges

In July 2013, the maximum level of regular state-level UI benefits available to North Carolinians fell by 50%, from $14,000 to $7,000. This was the result of both a reduction in the maximum benefit duration from 26 to 20 weeks, and a reduction in the maximum weekly benefit amount from $535 to $350. These curtailments sharply reduced the short-term UI recipiency rate relative to other Southern states which did not change their UI programs (see Section 3.2 and Figure 3). If outside options are pivotal in victims' decisions to report workplace sexual harassment, it follows that the UI reform could exacerbate underreporting by magnifying the loss of income after a retaliatory discharge (which occurs in at least 40% of charges). Wrongful terminations, such as retaliatory firings, are illegal and so the worker should in theory qualify for unemployment benefits. If the worker is simply let go, and not fired for cause, she will be eligible for benefits. If instead she is fired for cause, she must prove to the UI office that she was the victim of a wrongful termination.

In Figure 6a we plot the fraction of cases with merit in each year before and after the reform after residualizing out time by agency (FEPA or EEOC) FEs, for North Carolina versus other Southern states. We normalize the merit rate for the two groups to be the same in the year leading up to the reform.[26] There is no evidence for differential pre-trends. However, in the 12 months after the reform, the fraction of cases with merit spikes in North Carolina but not in other Southern states.

[26]North Carolina's pre-reform merit rate was 21.3%, while it was 26.5% in other Southern states.

In Figure 6b we plot regression-adjusted event-study coefficients for the difference between treatment and control. There is a 6 percentage point rise in merit in North Carolina relative to other Southern states in the year after the reform. This elevated rate persists and grows two to four years following the reform, consistent with the fact that North Carolina's reduction in UI benefits was permanent.

Instrumental variable estimates can be found in Table A13. The first stage regresses the short-term UI recipiency rate in a state and year on a North Carolina post-reform dummy. Panel A reveals that after the reform, there was a 20 percentage point drop in UI recipiency, relative to a pre-reform mean of 46%. Turning to Panel B, column (1) shows that, following North Carolina's reform, the selectivity of sexual harassment charges increases by 31.8%, or 7 percentage points off a baseline 21% merit rate. To scale the magnitude of the reduced form effect, we estimate an IV regression in column (2). The IV estimate indicates that each one percentage point increase in the UI recipiency rate causes a 0.34 percentage point decrease in selectivity. Since a higher UI recipiency rate mediates the effects of a retaliatory firing, this large response is consistent with increased underreporting in the absence of a generous social safety net.

How does this effect compare to our estimate using unemployment shocks? From the trough to the peak of the Great Recession, the national unemployment rate increased by 5.5 percentage points (from 4.5 to 10%). This implies a 1.6 percentage point increase in merit (calculated using the estimate from Table 4, column 1), which is smaller than the 7 percentage point increase due to the UI reform. The larger effect could partially be due to North Carolina passing their UI reform when unemployment was still 9.1%, as prior work suggests that UI is more highly valued when unemployment is high (Schmieder et al., 2012, McHugh and Kimball, 2015).

8.2 Robustness and Heterogeneity

As an alternative approach to our main UI specification, we estimate treatment effects using the synthetic difference-in-differences method (Arkhangelsky et al., 2021). This approach uses both time-period weights and unit weights to construct a counterfactual for how merit would have evolved based on a weighted average of the control states' merit over time. The unit weights can be found in Table A14. While the weights are fairly evenly distributed across all possible donor states, the states contributing the most weight are Indiana, Arizona, and Alabama. Note that there are 20 possible donor states as we exclude states that had changes to their UI program and many smaller states that had fewer than 15 sexual harassment charges in at least one year.

Figure A8 panel a plots the fraction of cases with merit for North Carolina as compared to our synthetic control sample. Time-period weights are represented in the bottom left corner of the graph. The trend for the outcome variable among synthetic North Carolina closely tracks that of North Carolina in the pre-period. In contrast, merit in North Carolina increases sharply following the reform, while there is no detectable deviation from trend for the synthetic controls. The estimated effect of the UI reform on merit is again approximately 7 percentage points, which matches the estimate from the standard difference-in-differences estimator which uses only other Southern states as controls. Figure A8 panel b constructs the analog of the placebo test in synthetic control designs whereby the estimated placebo effect in each of the 20 donor states is subtracted from the estimated effect in North Carolina. Note that all but 1 of the effects is positive, which indicates that the merit increase in North Carolina is the second-highest across all states in the sample.

Returning to our difference-in-differences design, in Table 8, we show that the effects of the reform are much larger for sexual harassment than for other non-harassment charges. As columns (2) reveals, merit increases by only 6% compared to 34% for sexual harassment. Column (3), which examines merit for other harassment cases, also has a smaller effect (11%) compared to sexual harassment. These differences mirror what we found in the unemploy-

ment analysis, and suggests that victims are more reluctant to report sexual harassment than other types of discrimination when outside options fall.

We next explore whether discharges or retaliations, and their impact on merit, drive our findings, rather than the victim's reluctance to file. In Table A15, we perform a similar exercise as we did for our unemployment shocks analysis (see Table 5). We find that the estimated effects are positive even when a case does not involve a discharge, a retaliation, or either a discharge or a retaliation, although the estimates are somewhat noisy.

To the extent that wages are a reasonable measure of outside options, the effect of the reform should be larger among low-wage workers (e.g., Caldwell and Danieli, 2020). This is because unemployment insurance covers a larger fraction of lost wages for low-wage workers. To evaluate this hypothesis, we split industries by whether their median wage is above or below the national median (Bureau of Labor Statistics, 2004). Columns (1) and (2) of Table 9 show that the increase in merit is driven primarily by workers operating in below-median wage industries.[27]

We next assess whether the North Carolina UI reform induced changes in the volume of reported charges. Figure A9 shows that the amount of reported sexual harassment in North Carolina did not change appreciably relative to other Southern States. Table A16 likewise shows that there is no volume response among non-harassment charges, and a small increase among harassment charges.

While no other state reduced the benefit levels of their regular UI programs, six other states also reduced benefit durations from 26 to 20 weeks. These six states, which we label the moderate reform states, are Florida, Georgia, Kansas, Michigan, Missouri, and South Carolina. These cutbacks were both less dramatic (the total nondiscounted reduction in maximum benefits averaged $2,000 in these other states, as compared to nearly $7,000 in North Carolina) and more heavily discounted (the benefit levels were unchanged for the 20 allowed weeks, while in North Carolina, benefit levels were 35% lower starting in week one).

[27] A similar analysis using unemployment shocks finds little difference across subsamples for either merit or volume. See Tables A18 and A19, respectively.

To gain precision, we combine these moderate reform states into a single analysis. As the reforms were implemented at different times, we use the difference-in-differences framework of Borusyak et al. (2021), using other Southern and Midwestern states which did not alter their UI programs as controls.

Figure A10 plots the estimates in event time. In the years leading up to the reform, there is no evidence for differential pre-trends. By two years after the reform, there is a statistically significant increase in merit of roughly 2 percentage points. This effect increases in year three to roughly 4 percentage points. For completeness, Figure A11 plots event-studies for each of the six states individually. In column (4) of Table 8, we estimate an average increase in merit of 2.4 percentage points over the three years following the reform (a 9.6% increase in merit relative to the mean).[28] As expected, this effect is smaller than the 33% increase found for the larger reform in North Carolina. In the moderate reform analysis, there is no statistical effect for other forms of non-harassment discrimination in column (5) or other types of harassment in column (6). Turning to Table 9, columns (3) and (4), shows that merit is more responsive to these reforms in low-wage industries. In terms of volume, there is no statistical evidence of a response, as can be seen in Figure A12 and Table A16.

We further assess the robustness of our event-study results to violations of smoothness restrictions in parallel pre-trends (Rambachan and Roth, 2023). Columns 1 and 2 of Table A17 show that we can rule out violations up to a 5 percentage point decline in merit for North Carolina, while columns 3 and 4 show that our estimated effects survive up to a 3 percentage point decline in merit for the moderate reform states. Both tests suggest that our results are not particularly sensitive to violations of parallel pre-trends.

Table 10 suggests one possible mechanism for why merit increases. In particular, we make use of the filing delay, which could increase if victims are more hesitant to report and hence take more time to deliberate. The filing delay is defined as the number of days between the

[28]The estimated effect in Table 8 is not directly comparable to Figure A10 because in the figure, we do not include data for Georgia for January-June 2012, as its reform did not occur until July. These observations are included in the table.

first incident and the date on which the EEOC charge is filed. Here we combine the NC UI reform and the moderate UI reform treatments into one analysis. Column (1) shows that following the reforms, victims of sexual harassment increase their filing delay by 11.5%, or an additional 27 days. Columns (2) and (3) indicate that this filing delay increase is larger for sexual harassment compared to non-harassment and other harassment charges, though the differences are not statistically significant.

From the analyses of North Carolina and the moderate reform states, we conclude that the willingness to report sexual harassment depends crucially on outside options. To quantify this relationship for North Carolina, we perform a back-of-the-envelope calculation for the share of underreporting attributable to the reform. We conservatively estimate the reform increased underreporting by 28.9%, or in other words, that each $1,000 reduction in maximum UI generosity increased underreporting by approximately 4.1%.[29]

9 Conclusion

This paper provides novel tests for the underreporting of workplace sexual harassment. We measure the quality of charges using the universe of charges filed with the EEOC to study how economic conditions, workplace characteristics, and UI generosity each influence the incentive for victims to report sexual harassment in the workplace. Our analysis of reporting incentives complements existing work which studies anonymous surveys of workplace sexual harassment, where victims are shielded from any economic consequences when describing their experiences. We view the two types of data and analyses as useful complements for a better understanding of workplace sexual harassment, and how best to combat it.

We conclude that reductions in the value of outside options make employees more vul-

[29] In the post-reform period, North Carolina workers filed 779 sexual harassment charges, 195 of which were meritorious, for a merit rate of 25.0%. Table 8 indicates the reform increased merit by 7.2 percentage points. The number of charges x that would have had to have been filed to return the merit rate to its pre-reform level (assuming that the volume of charges filed is unaffected by the reform) is $(195+p\times x)/(779+x) = 0.250-0.072$, where p is the average merit rate among charges averted due to the UI reform. The most conservative estimate uses $p = 0$, i.e., that every averted charge would have been non-meritorious. Setting p equal to 0, we estimate that 316 fewer sexual harassment charges were filed due to the cutback in North Carolina's UI program.

nerable to sexually hostile work environments. Using both unemployment rate increases and sharp cuts to unemployment insurance benefits, we estimate significant increases in the selectivity of sexual harassment charges filed with the EEOC, which implies an increase in underreporting. Consistent with these findings, Google searches for "sexual harassment in the workplace" (which capture both reported and unreported instances) spike relative to charges filed (reported instances) during the Great Recession. We also document the role of power disparities using linked employee-employer data that connects sexual harassment charges to the male share of co-workers and managers in the workplace.

One way to think about the underreporting problem is that it stems from an inefficient allocation of costs across firms and employees. If employees face reporting costs that are amplified by the prospect of retaliatory firings, they will underreport. A direct way to both curb firm misconduct and encourage reporting would be to raise the costs borne by employers in the event that they are found culpable, particularly when a charge involves retaliation (Hubbell, 1989, Bullock, 2020). These penalties could even be made countercyclical, to more aggressively deter illegal harassment when the costs of speaking out are highest. Moreover, the expansion of social safety net programs, such as UI, may efficiently increase ex-ante moral hazard by shrinking the penalty of a retaliatory firing and hence, implicitly encourage victims to report sexual harassment they might otherwise not.

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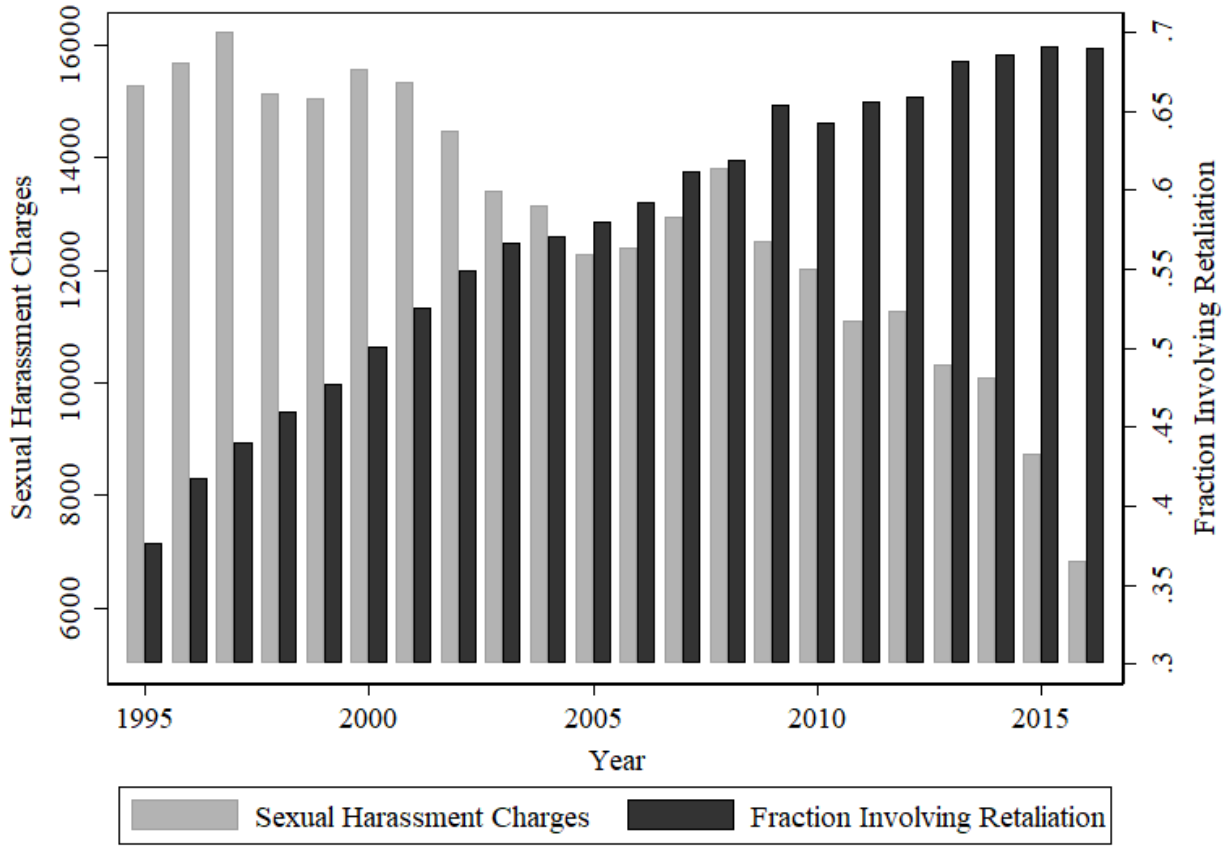
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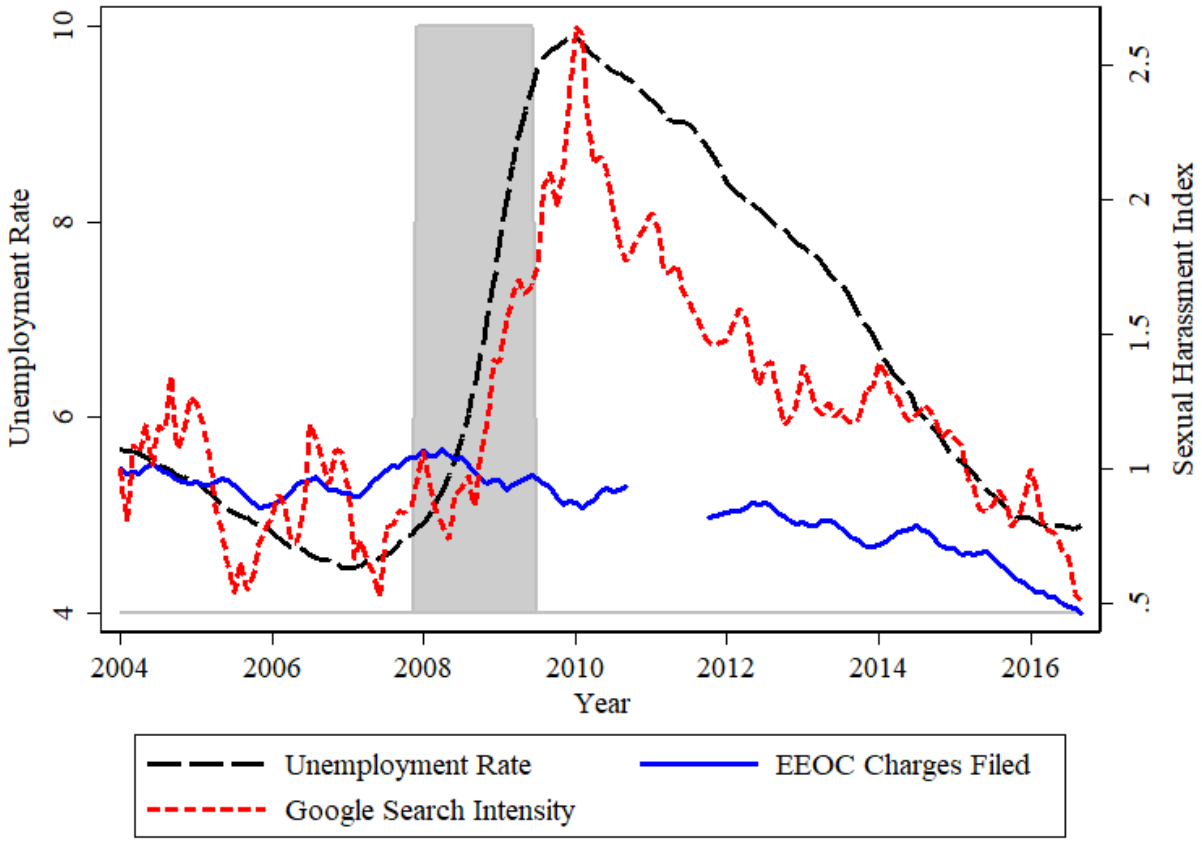
10 Figures

Figure 1: Sexual Harassment Charge Volume and % Involving Employer Retaliation



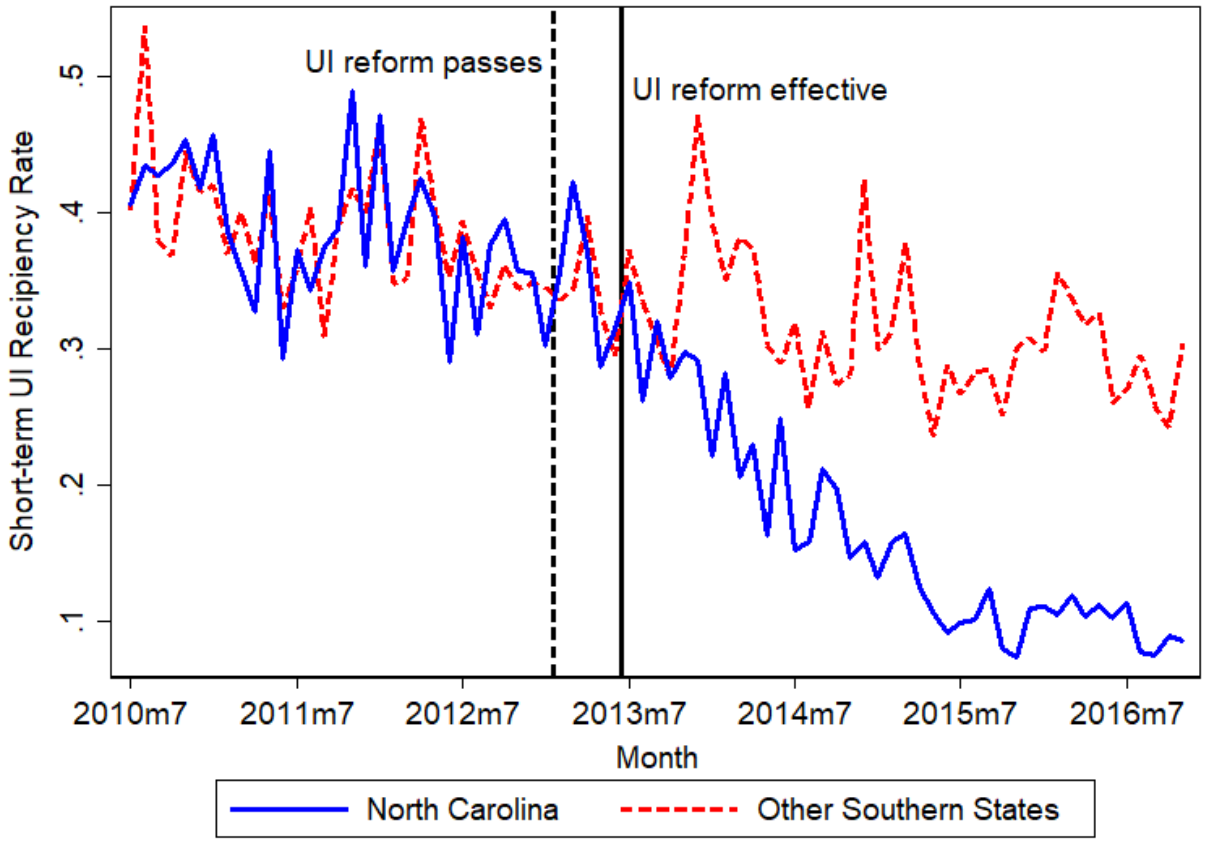
Annual number of nationally aggregated sexual harassment charges filed with the EEOC and the fraction of those charges for which the employer retaliated against the claimant. Charge data are missing in last 3 months of 1995 and 2010, and the first 9 months of 1996 and 2011, and so charges in these years are scaled by 0.75 and 0.25, respectively. Data for 2016 go through September, and so we scale by 0.75.

Figure 2: Sexual Harassment Google Search Intensity, Charges Filed, and Unemployment



Seven-month smoothed monthly Google Search Index (GSI) for "sexual harassment in the workplace," sexual harassment charges filed with the EEOC, and the smoothed unemployment rate. GSI and EEOC charges filed are measured relative to January of 2004, the first month for which the GSI is available. EEOC charge data are missing from October 2010 through September 2011. Shading indicates the Great Recession, as defined by the NBER. This graph uses data accessed on December 29, 2021.

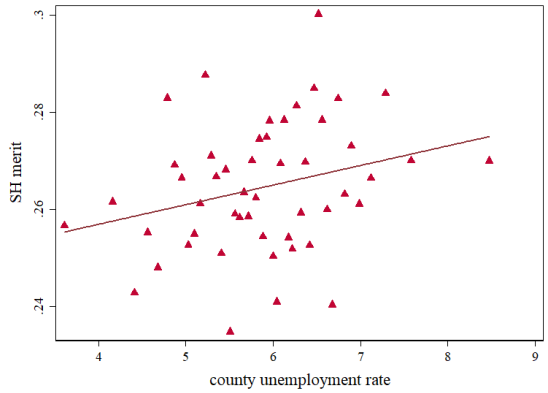
Figure 3: UI Recipiency Rate for North Carolina versus Other Southern States



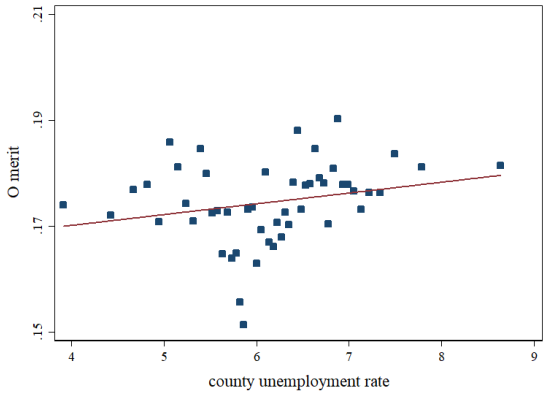
Short-term UI recipiency rate is the monthly average of total weeks compensated under regular state UI programs divided by the monthly number of short-term unemployed workers. Other Southern States include all states without UI changes from the South Atlantic and East South Central Census Divisions: Delaware, District of Columbia, Maryland, Virginia, West Virginia, Alabama, Kentucky, Mississippi, and Tennessee.

Figure 4: Sensitivity of Merit and Volume to Unemployment, by Sexual Harassment Status

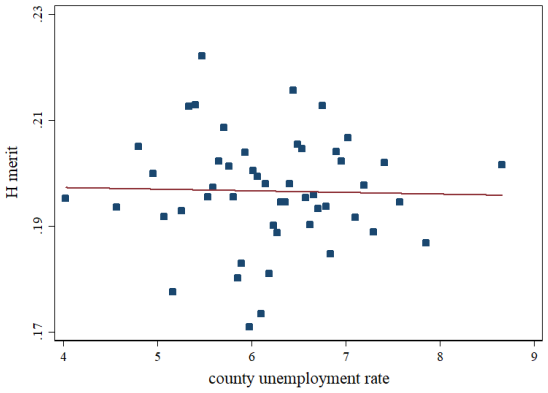
(a) Merit, Sexual Harassment



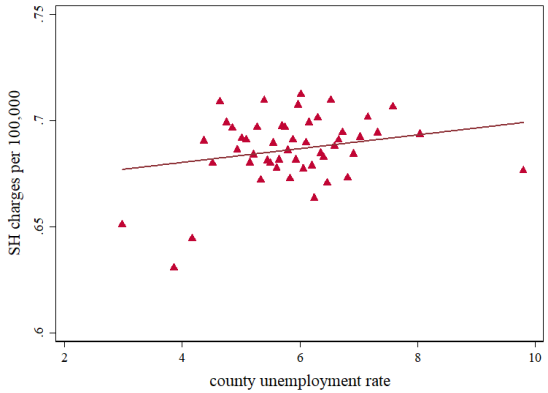
(b) Merit, Other



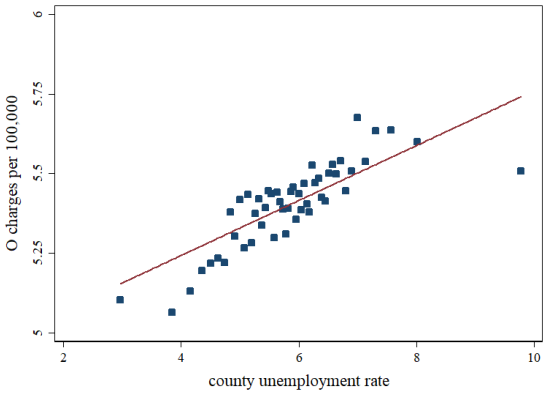
(c) Merit, Harassment



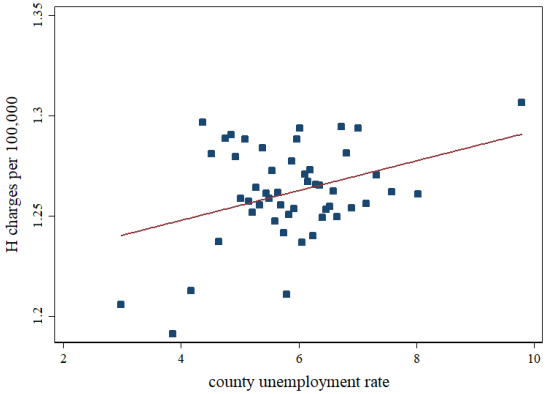
(d) Volume, Sexual Harassment



(e) Volume, Other

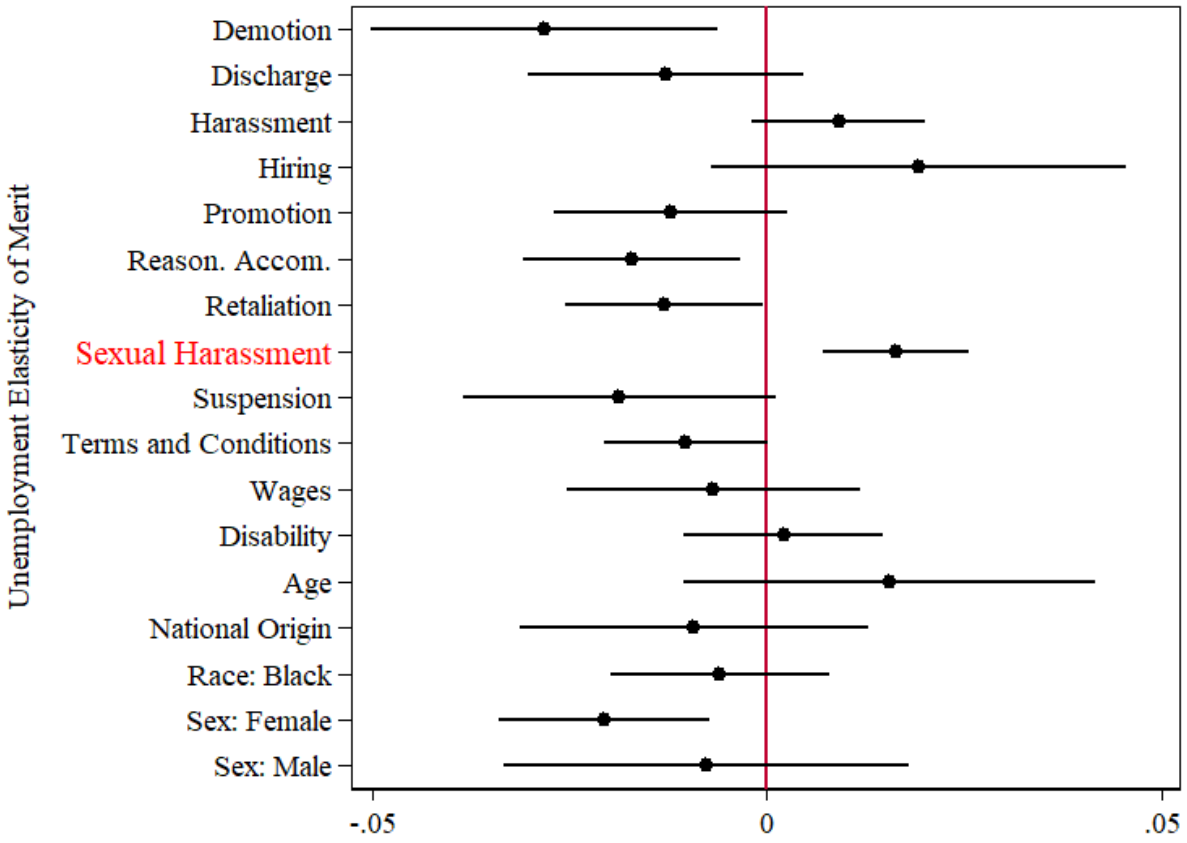


(f) Volume, Harassment



In panels (a), (b), and (c), the y-axis measures the fraction of charges determined to have merit by the EEOC in a county-month unemployment rate bin, for charges including a sexual harassment claim, excluding all harassment and sexual harassment claims, and including only (non-sexual) harassment claims, respectively. The controls and fixed effects included in equation 4 are residualized out. In panels (d), (e), and (f), the y-axis measures the volume of charges filed per 100,000 in the labor force in a county-month unemployment rate bin, for each of the three corresponding categories above. The fixed effects for county and time by agency (FEPA or EEOC) are residualized out. Observations are weighted by the county labor force and cover the period from 1995-2016.

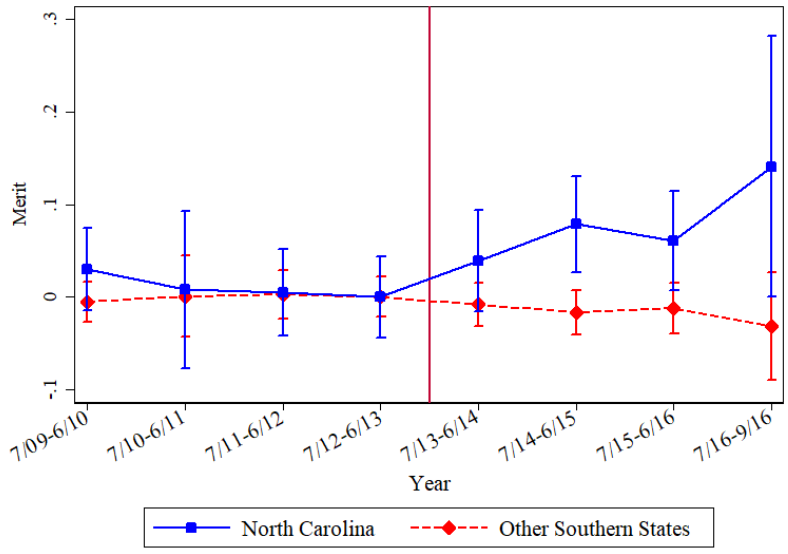
Figure 5: Unemployment Elasticity of Merit, by Discrimination Type



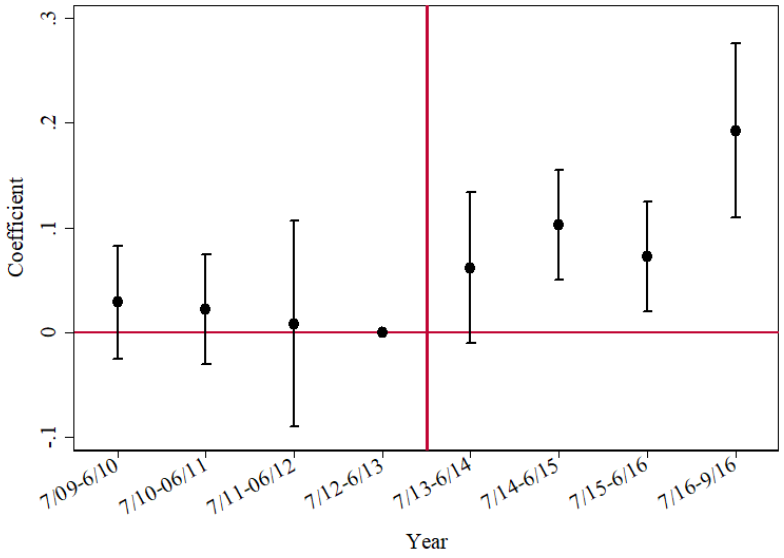
Coefficient plot for elasticities of merit with respect to county unemployment for top issues (Demotion-Wages) and bases (Disability-Sex:Male). Each estimate comes from a separate regression and includes fixed effects for the other issues and bases. Additional controls include county, time, age, race, national origin, and sex. Note that charges can have multiple issues and bases, so the categories are not mutually exclusive. Sexual harassment can also include other issues and bases. However, for the purpose of this graph, we do not allow other categories to include sexual harassment as an issue. Excluded from the figure is intimidation, since this is an outlier which distorts the scale of the graph; the unemployment elasticity for this category is -0.053 (0.022). Standard errors are clustered at the county level. Due to data availability, sample covers 1995 onward for Disability, Reasonable Accommodation, Sexual Harassment, and Harassment charges, and 1991 onward for all other charge types.

Figure 6: Selectivity of Sexual Harassment Charges for North Carolina versus Controls

(a) Using Other Southern States as Controls



(b) Using Other Southern States as Controls



The top panel reports residualized merit after netting out time by agency fixed effects, while the bottom panel reports event-study coefficients from a specification which nets out county and time by agency fixed effects. Merit is the fraction of charges determined to have merit by the EEOC, normalized to 0 in the year prior to the reform. Vertical bars denote 95% confidence intervals. See Figure 3 for the list of Other Southern States.

11 Tables

Table 1: Unemployment and Anonymous Searches versus Formal Reports

	Anonymous Searches		**Formal Reports**	
	Google Search Index		*EEOC Charge Volume*	
	(1)	(2)	(3)	(4)
unemployment rate	5.70	6.29	7.11	-3.80
	(0.63)	(0.80)	(8.56)	(4.93)
Mean(index)	30.82	30.82	953.1	953.1
% change	18.49	20.41	0.75	-0.40
Elasticity	1.22	1.34	0.05	-0.02
Quadratic time trend		X		X
Month dummies		X		X
N year-months	153	153	141	141
R^2	0.350	0.538	0.005	0.839

Google Search Index measures the normalized number of searches for "sexual harassment in the workplace" and EEOC Charge Volume measures the total number of sexual harassment charges filed with the EEOC or state FEPAs. Both measures are at the national level, and cover the period from 2004-2016, the time period for which Google Search Index and EEOC charge data are both available. EEOC charge data are missing from October 2010 through September 2011.

Table 2: Summary Statistics by Claim Type

	Sexual Harassment	All Other	Other Harassment
Resolutions by Type			
Merit	0.254	0.181	0.190
Settlement with benefits	0.107	0.085	0.090
Withdrawal with benefits	0.076	0.060	0.063
Reasonable cause	0.070	0.034	0.036
Successful conciliation	0.022	0.012	0.011
Unsuccessful conciliation	0.047	0.021	0.025
Case Characteristics			
Employer retaliation involved	0.576	0.224	0.467
Employer intimidation involved	0.066	0.018	0.094
Legal representation	0.131	0.068	0.091
Days between first offense and filing	241.6	168.5	239.3
Worker Characteristics			
Female	0.843	0.516	0.559
Age	38.0	45.7	44.1
White	0.543	0.401	0.365
Black	0.209	0.396	0.423
Charges	194,879	1,520,121	364,012

Data are from EEOC Charge Database, 1995-2016, and include charges filed with either a state FEPA or the EEOC. Sexual Harassment charges include any filing alleging sexual harassment. "No Sexual Harassment" corresponds to all EEOC discrimination charges filed that do not raise sexual harassment as an issue. Other Harassment corresponds to all harassment charges not involving sexual harassment.

Table 3: Establishment Characteristics using Linked Data

	Any Female SH Charges	Any Female Other Charges	No Charges Filed
Demographics			
% Male	0.535	0.451	0.512
% Managers Male	0.664	0.572	0.613
% Female Laborers	0.028	0.023	0.016
% Female Service Workers	0.089	0.086	0.082
% White	0.635	0.634	0.699
% Black	0.184	0.201	0.123
Size			
Employees	565.2	719.5	103.1
Plants within firm	372.9	445.5	706.2
Selected Industries			
Manufacturing	0.215	0.174	0.133
Retail	0.136	0.126	0.118
Health care & social assistance	0.117	0.223	0.118
Accommodation & food services	0.114	0.059	0.079
Admin support/waste management	0.087	0.070	0.067
Census Region			
Midwest	0.231	0.269	0.234
Northeast	0.102	0.111	0.174
South	0.464	0.441	0.379
West	0.203	0.179	0.213
Establishment-years	26,986	221,065	8,268,753

Establishment characteristics based on whether at least one employee filed a sexual harassment charge or non-sexual harassment charge against their company in an establishment-year, or whether no EEOC or FEPA charges were filed against a company in an establishment-year. Data links individual discrimination charges (the EEOC Charge data) with the characteristics of each establishment against which the charges are filed (the EEO-1 files) from 1995-2015.

Table 4: Unemployment and EEOC Charge Quality

Charge type	**Sexual Harassment**		**All Other**		**Other Harassment**	
Dep var = $\mathbb{1}(merit)$	(1)	(2)	(3)	(4)	(5)	(6)
# unemployed$_{ct}$	16.6	15.9	1.96	0.10	6.12	4.83
	(4.56)	(4.55)	(4.60)	(4.69)	(2.98)	(2.96)
female	0.052	0.031	0.022	0.015	-0.003	-0.013
	(0.003)	(0.008)	(0.001)	(0.001)	(0.002)	(0.002)
Effect of 1 pp ↑ unemp	0.00074	0.00071	0.00010	0.00000	0.00035	0.00028
mean(merit)	0.255	0.255	0.181	0.181	0.190	0.190
% change	0.29	0.28	0.06	0.00	0.19	0.15
Elasticity	0.017	0.016	0.003	0.000	0.011	0.009
County and time FEs	X	X	X	X	X	X
Other issues and bases FEs		X		X		X
N	194,879	194,879	1,520,121	1,520,121	364,012	364,012
R^2	0.042	0.043	0.018	0.024	0.024	0.027

Regression estimates based on equation 4 and the sample period of 1995-2016. 'Effect of 1 pp ↑ unemp' is the implied effect of a one percentage point increase in a county's monthly unemployment rate on the fraction of charges found to have had merit by the EEOC; '% change' is this effect divided by the mean merit rate. 'Elasticity' is calculated as the regression coefficient multiplied by the average number unemployed in a country-month cell, divided by the mean merit rate. Time FEs are year-month by agency (EEOC or FEPA) controls. Additional controls include an individual's sex, race, and age. Other charge issues raised include discharge, hiring, demotion, discipline, harassment, intimidation, promotion, reasonable accommodation, suspension, terms and conditions, and wages. Other bases raised include disability, age, race, sex, national origin, and retaliation. Robust standard errors, clustered at the county level, are reported in parentheses.

† Coefficients are multiplied by 10^{-8}

Table 5: Unemployment and Sexual Harassment Charge Quality, by Charge Type

	Discharge		**Retaliation**		**Disch. or Retal.**	
	Yes	*No*	*Yes*	*No*	*Yes*	*No*
Dep var = $\mathbb{1}(merit)$	(1)	(2)	(3)	(4)	(5)	(6)
# unemployed$_{ct}$ †	22.3	10.2	8.79	32.7	15.1	27.4
	(5.32)	(7.82)	(5.39)	(9.51)	(4.68)	(12.3)
Effect of 1 pp ↑ unemp	0.00095	0.00049	0.00041	0.00138	0.00067	0.00127
Mean(merit)	.251	.260	.254	.255	.253	.260
% change	0.38	0.19	0.16	0.54	0.26	0.49
Elasticity	0.022	0.012	0.010	0.031	0.016	0.028
County and time FEs	X	X	X	X	X	X
N charges	122,331	72,144	112,117	82,326	156,623	37,739
R^2	0.052	0.064	0.052	0.064	0.047	0.091

Regressions mirror those in Table 4 and are based on the sample period of 1995-2016. Column 2 excludes discharges but could include retaliation, column 4 excludes retaliation but could include discharges, and column 6 excludes both discharges and retaliation. Robust standard errors, clustered at the county level, are reported in parentheses.

† Coefficients are multiplied by 10^{-8}

Table 6: Unemployment and EEOC Charge Volume

Charge type	**Sexual Harassment**	**All Other**	**Other Harassment**
Dep Var = # of charges	(1)	(2)	(3)
# unemployed$_{ct}$	-12.5	50.0	16.2
	(6.29)	(23.3)	(23.5)
Effect of 1 pp ↑ unemp	-18.7	75.1	24.3
mean(dep var)	1,028.1	8,113.4	1,890.5
% change	-1.8	0.92	1.28
Elasticity	-0.107	0.054	0.075
county and time FEs	X	X	X
N (county-year-month cells)	764,588	758,558	763,865
R^2	0.715	0.885	0.813

Regressions mirror those in Table 4 and are based on the sample period of 1995-2016, but use the number of charges filed as the dependent variable. Regression coefficients estimate the change in charges filed for a 1,000,000 person increase in the number unemployed. The average size of the national labor force is 149.804 million. Robust standard errors, clustered at the county level, are reported in parentheses.

† Coefficients are multiplied by 10^{-6}

Table 7: Gendered Power Dynamics: Female Charge Quality within a Workplace

Dep var = $\mathbb{1}(merit)$	**SH** (1)	**O** (2)	**H** (3)	**SH** (4)	**O** (5)	**H** (6)
# unemployed$_{ct}$ × %men$_{estab}$ †	7.76	-0.28	0.53			
	(1.54)	(0.75)	(1.18)			
# unemployed$_{ct}$ × %male managers$_{estab}$ †				5.93	0.04	0.61
				(1.47)	(0.37)	(1.27)
sd(% men or % male mgrs.)	0.222	0.228	0.226	0.253	0.269	0.269
Effect of 1 sd ↑ % men or % male mgrs. on 1 pp ↑ unemp	0.00093	-0.00004	0.00008	0.00081	0.00001	0.00010
Mean(merit)	.248	.188	.188	.248	.188	.188
% change	0.37	-0.02	0.04	0.33	0.00	0.05
County and time FEs	X	X	X	X	X	X
N charges	26,986	175,423	45,154	26,986	221,065	45,154
R^2	0.089	0.035	0.064	0.089	0.032	0.064

Regressions mirror those in Table 4. Data links individual sexual harassment charges (the EEOC Charge data) with the characteristics of each establishment against which the charges are filed (the EEO-1 files) from 2000-2015. 'Effect of 1 sd ↑ % men or % male mgrs. on 1 pp ↑ unemp' is the implied effect of a one percentage point increase in a county's monthly unemployment rate interacted with the % of men or % of male managers in an establishment on the fraction of charges found to have had merit by the EEOC. Robust standard errors, clustered at the county level, are reported in parentheses.
† Coefficients are multiplied by 10^{-7}

Table 8: UI Reductions and Charge Quality

	NC UI Reform			*Moderate UI Reforms*		
Charge type	**Sexual Harassment**	**Other**	**Harassment**	**Sexual Harassment**	**Other**	**Harassment**
Dep var = $\mathbb{1}(merit)$	(1)	(2)	(3)	(4)	(5)	(6)
treated state(s) × post	0.072	0.010	0.019	0.024	0.010	0.008
	(0.021)	(0.010)	(0.012)	(0.009)	(0.008)	(0.007)
Mean(pre-reform merit)	.213	.154	.165	.249	.186	.196
% change	33.6	6.4	11.3	9.6	5.3	4.3
County and time FEs	X	X	X	X	X	X
N charges	9,697	95,786	27,963	32,690	294,033	86,922
R^2	0.079	0.021	0.039			

Regression estimates based on equation 5. Columns (1)-(3) use as controls Other Southern States and columns (4)-(6) use as controls Other Southern and Midwestern States (see Figures 3 and A10 for the list of states). Sample period is from July of 2009 (July of 2008 for columns 4-6) through September of 2016. Time FEs are year-month by agency (EEOC or FEPA) controls. Robust standard errors, clustered at the state by year level (columns 1-3) or state level (columns 4-6), are reported in parentheses.

Table 9: UI Reductions and Sexual Harassment Charge Quality, by High versus Low-Wage Industries

	NC UI Reform		*Moderate UI Reforms*	
Sample	**< median wage**	**> median wage**	**< median wage**	**> median wage**
Dep var = $\mathbb{1}(merit)$	(1)	(2)	(3)	(4)
treated state(s) × post	0.084	-0.012	0.030	-0.006
	(0.041)	(0.057)	(0.018)	(0.015)
Mean(pre-reform merit)	.225	.250	.239	.235
% change	37.4	-4.9	12.8	-2.5
County and time FEs	X	X	X	X
N charges	2,181	2,148	7,521	7,519
R^2	0.192	0.174		

Regressions mirror those in Table 8. Samples are split on the basis of whether a worker is in an industry whose median wage is below the median across all industries. There are 238 industries at the NAICS-4 level in the Occupational Employment and Wage Statistics (OEWS). Columns (1) and (2) use as controls Other Southern States and columns (3) and (4) use as controls Other Southern and Midwestern States. Sample period is from July of 2009 (July of 2008 for columns 3-4) through September of 2016. Robust standard errors, clustered at the state level, are reported in parentheses.

Table 10: Combined UI Reforms and Filing Delay Length

	Sexual Harassment	**Other Harassment**	**Harassment**
Dep var = *log(filing delay)*	(1)	(2)	(3)
treated state(s) × post	0.115	0.102	0.042
	(0.031)	(0.036)	(0.035)
Mean(pre-reform delay in days)	234.1	146.2	233.2
County and time FEs	X	X	X
N charges	34,409	312,016	91,652

Regressions mirror those in Table 8. Filing delay length is the number of days elapsed between the date of the first alleged incident and the date on which the charge is filed with the EEOC. Control group includes all Other Southern States and Midwestern States. Sample period is from July of 2008 through September of 2016. Robust standard errors, clustered at the state level, are reported in parentheses.