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WHY IS WORKPLACE SEXUAL HARASSMENT UNDERREPORTED? THE VALUE OF OUTSIDE OPTIONS AMID THE THREAT OF RETALIATION

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ABSTRACT

Why is workplace sexual harassment chronically underreported? We hypothesize that employers coerce victims into silence through the threat of a retaliatory firing, and test this theory by estimating whether external shocks that reduce the value of a worker's outside options exacerbate underreporting. Under mild assumptions, a rise in the severity of formal complaints is indicative of increased underreporting. Combining this insight with an objective measure of the quality of charges filed with the Equal Employment Opportunity Commission (EEOC), we perform two analyses. First, we assess whether workers report sexual harassment more selectively during recessions, when outside labor market options are limited. We estimate the fraction of sexual harassment charges deemed to have merit by the EEOC increases by 0.5-0.7% for each one percentage point increase in a state-industry's monthly unemployment rate. The effect is amplified in industries employing a larger fraction of men and in establishments with a higher share of male managers. Second, we test whether less generous UI benefits create economic incentives for victims of workplace sexual harassment to remain silent. We find the selectivity of sexual harassment charges increases by more than 30% in response to a 50% cut to North Carolina's Unemployment Insurance (UI) program following the Great Recession.

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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 11,000 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] To give a sense of the magnitude of these costs, Hersch (2018) estimates the value of statistical harassment (VSH) for serious cases at approximately \$7.6 million, which is roughly comparable to the value of a statistical life (VSL). This figure does not include 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

[1] These calculations come from the the 2015 American Working Conditions Survey conducted by 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. The EEOC estimates that conditional on being harassed in the workplace, 75% of victims do not report (Feldblum and Lipnic, 2016).

[2] See Merit (1981); Crull (1982); Fitzgerald et al. (1988, 1994); Gutek (1985); Acken et al. (1991); Frierson (1989); Gutek and Koss (1993); Faley et al. (1994); Laband and Lentz (1998).

that firms and managers coerce their employees into silence through the threat of retaliation—through a firing, wage cut, or assignment to undesirable tasks or schedules—for engaging in a "protected activity," such as 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 common over time, rising from 52% of cases in 2000 to 72% in 2015. At the same time, the number of annually filed sexual harassment charges has fallen by 37%. Viewed in isolation, this latter pattern could naively be construed as a sign that the problem of workplace sexual harassment has diminished over time. A more cynical interpretation is that firms have 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, it has not formally been tested (Berrey et al., 2017; Stainback and Tomaskovic-Devey, 2012; Knapp et al., 1997).[3] One reason is that it is difficult to measure what is not reported.

We overcome this challenge by recognizing that external forces which reduce the value of becoming unemployed should increase the employee's cost of a retaliatory firing, thus raising the threshold above which a victim is willing to file a charge. 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. The identifying assumption is that a worker's willingness to file a sexual harassment claim depends only on her outside options and the severity of sexual harassment she experiences.[4] There is not a clear prediction about how the volume of reported claims responds, as this would require assumptions about firm behavior.

We measure the quality/selectivity of sexual harassment charges using the EEOC's de-

[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. Boone et al. (2011) find that workers who report workplace accidents are subsequently more likely to be fired.

[4]In our empirical work, we only require this assumption to hold conditionally, that is, after netting out state, industry, and time fixed effects.

termination of whether a case has merit. We use this to test whether victims become increasingly reluctant to jeopardize their current employment when the value of their outside options diminishes.[5] The merit variable captures the quality of a case as long as the EEOC applies a consistent standard for determining merit, at least after netting out state, industry, and time fixed effects.

Our first analysis exploits variation over time in a state-industry's unemployment rate. Using data from 2000-2015, we find that each 1 percentage point rise in the unemployment rate increases the probability a charge is determined to have merit by 0.5-0.7%. This is consistent with the hypothesis that a weakened outside labor market leads more victims to remain silent for fear of retaliation.

We interpret this increase in the quality of filed charges more formally through the lens of a threshold model of reporting. In this model, workers choose a level of harassment they are willing to tolerate, above which they will file an EEOC charge. This threshold value increases as outside job opportunities worsen. Hence, the response by victims to a decline in labor market prospects is to increase the selectivity of charges they file.

We extend this analysis by documenting facts and providing insights into possible mechanisms. To do this, we link the EEOC charge data with information on the gender mix of managers and co-workers within establishments. We show that reporting increases in the fraction of male colleagues, consistent with related findings from Folke and Rickne (2020). Unlike Folke and Rickne (2020), we also find that reporting increases when a larger fraction of men occupy supervisory roles. We go one step further and show that underreporting, as proxied by selectivity, increases in both the fraction of male managers and 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.

Our second analysis takes advantage of a plausibly exogeneous change in the generosity of North Carolina's unemployment insurance (UI) program. Since UI partially protects workers

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

from income loss in the event of a retaliatory firing, it follows that reductions in UI generosity 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 weeks, and imposing additional eligibility restrictions.[6] 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.[7]

Our paper contributes to a nascent literature in economics inspired by the #MeToo movement. Cheng and Hsiaw (2020) 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 (2020) 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.[8] Finally, Sockin et al. (2021) 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 firm practices. They find that less restrictive NDA 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

[6]These combined changes violated the U.S. Congress' non-reduction rule, and made individuals ineligible for the additional 47 weeks of UI benefits provided through the federal Emergency Unemployment Compensation (EUC) program.

[7]We note that North Carolina also reduced corporate and personal income taxes shortly after the UI reform. This tax reform should have increased labor demand and increased labor supply (as long as substitution effects dominate income effects), and so we interpret our estimates as lower bounds of the effect of the UI reform on selectivity.

[8]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 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.

demonstrate that increases in retaliation costs 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.[9] One exception is Lusher et al. (2020), 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. (2020) shows a negative consequence of UI extensions, we document a positive effect.

The remainder of the paper proceeds as follows. We first provide institutional background on the EEOC's role in curbing sexual harassment and on North Carolina's cuts to its UI program. In Section 3, we detail the various data sources we use to study workplace sexual harassment. Section 4 provides a simple threshold model of sexual harassment reporting. In Section 5 we detail the identification strategies used to test how diminished outside options affect sexual harassment underreporting. Section 6 shows results on sexual harassment underreporting based on economic conditions and gendered power disparities, Section 7 details how the selectivity of charges responds to reductions in UI generosity, and Section 8 concludes.

[9]See, for example, 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 Institutional and Policy Background

2.1 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, 2020). 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 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.

For harassment to rise to the level of illegality according to the EEOC, it must be "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)."[10] Meritorious charges, as determined by the EEOC, will have met this "hostile work environment" standard, while non-meritorious charges may still involve some level of harassment without having exceeded the standard. In this sense, the merit designation captures more serious or repeated incidents of sexual harassment. The EEOC 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 EEOC makes a determination that the case has reasonable cause following a lengthy legal investigation. For more details on how charges are resolved, see Dahl and Knepper (2020).

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

An employee must file a charge within 180 calendar days of the offense either online, by mail, or in person at an EEOC office. Within 10 days of the receipt of the charge, the EEOC sends a notice of the charge to the named employer. While an employee may elect to have another individual or agency file a charge on her behalf, the nature of the process makes it difficult for a victim's anonymity to be preserved. Indeed, the fact that retaliation–which the EEOC defines as punishment for engaging in a protected activity, such as filing a charge–arises in over 63% of all sexual harassment cases makes clear that an individual's identity is not generally protected.

2.2 Cuts to North Carolina's UI Program

From the late 1960s through early 2011, all 50 states and DC paid regular unemployment insurance 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 payouts during the protracted recovery from the Great Recession. In response to this deepening crisis, 8 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).[11]

In no state were cuts more draconian than in North Carolina, which was the only one to 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.[12] The combined effect of these cutbacks was substantial. The maximum level of regular state-level UI benefits available to North Carolinians fell by 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.

[11] These states are Arkansas, Florida, Georgia, Kansas, Michigan, Missouri, North Carolina, and South Carolina.

[12] 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.

This policy environment is ideal for four related reasons. First, the North Carolina cuts were made on the basis of 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 explicit 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. Thus, our estimates are unlikely to be driven by an expectation of further deterioration of labor market conditions, rather than a reluctance to report.

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.[13] 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 cuts are likely to be particularly salient for women worried about a retaliatory firing.[14]

Fourth, North Carolina's UI reductions were permanent and affected the first source of benefits from which individuals claiming UI draw. 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.

One caveat is that North Carolina also reduced corporate and personal income taxes soon

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

[14]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.

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.

In spite of the many advantages of studying changes to UI generosity generated from legislative rollbacks, there is limited research leveraging this variation. A notable exception is Johnston and Mas (2018), which finds a benefit cut in Missouri reduced unemployment spells and moderately reduced the state's overall unemployment rate. In ongoing work, we study the effect of North Carolina's reform on starting salaries and other labor market outcomes. We find that starting salaries fall sharply following the reform relative to control states.

3 Data and Sample Construction

This paper leverages several novel data sources to provide a uniquely detailed view of workplace sexual harassment. We combine data on the quality (and volume) of individual sexual harassment reports filed with the EEOC 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 impute contemporaneous estimates of local labor market conditions with data produced by the Bureau of Labor Statistics (BLS). We also link the charge data to the EEO-1 files collected by the EEOC, which detail the racial, gender, and occupational distribution of workers in private establishments. Lastly, we link the charge data with publicly available information on the timing and severity of the cut to North Carolina's UI program.

3.1 EEOC Charge Data

Our study draws on the universe of all sexual harassment charges filed with the EEOC between 2000 and 2015, which is the most comprehensive and authoritative data source on workplace sexual harassment in the United States. These data are an improvement over measures of discrimination that are commonly used in the economics literature, such as wage and employment gaps, which only obliquely capture potential discrimination. The EEOC data, in contrast, directly measure on-the-job sexual harassment.

Moreover, these EEOC data include the resolution associated with each charge, which 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 virtually to the exclusion of merit considerations.

Table 1 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 2000 and 2015, we find that, compared to non-harassment charges, sexual harassment charges are nearly 50% more likely to have been meritorious (27.0% versus 18.6%) and more than twice as likely to have involved employer retaliation (63.4% versus 30.7%) and employer intimidation (5.7% versus 2.7%). 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 discrimination filings on average, they are nearly 65% more likely to have private legal representation compared to all other charges (13.0% compared to 7.9%).

The table also shows that 84% of sexual harassment complainants are female (compared to 52% of non-sexual harassment victims), and they are younger than their non-harassed counterparts. Sexual harassment victims are overrepresented in Accommodation & Food Services as well as in Retail.

3.2 State and Industry Unemployment Data

We measure local variation in the number of unemployed individuals at the state-month level using BLS Local Area Unemployment Statistics (LAUS). To impute local variation at the state-industry-month level, we combine the LAUS with national unemployment data at the industry-month level and state-industry-month employment shares from the Quarterly Census of Employment and Wages (QCEW). Our time period spans 2000 through 2015. This period provides rich variation in exposure to labor demand shocks across geography and industries, as these years include two distinct recessions with strikingly different heterogeneity across states and industries (Hershbein and Stuart, 2020).

3.3 EEO-1 Employment Files

We further link the EEOC charge data with the 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 nearly 8 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. These data allow us to explore how the gender mix of co-workers and managers influence sexual harassment charges filed by women. We are able to link roughly half of the EEOC sexual harassment charges to this data.[15]

Table 2 compares establishments on the basis of whether they were named as a respondent in a sexual harassment charge in a given year. Offending establishments have 2.8 percentage point more males and 5 percentage point more male managers. In contrast, these establishments have 2.1 percentage points more women in lower-skilled occupations, such as the laborer and service worker categories. Additionally, Southern establishments are disproportionately more likely to be accused of sexual harassment, with the Northeast having the

[15] We cannot match all charges for two primary reasons. First, EEOC federal antidiscrimination 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. Second, the EEO-1 files cover only private establishments, whereas approximately 15% of discrimination charges are leveled against public firms.

fewest charges. This pattern matches the geographic distribution of labor rights violations from 2000-2019 (Marinescu et al., 2020).

3.4 Unemployment Insurance Data

To estimate the impact of North Carolina's UI reform, we combine Department of Labor administrative data (ETA Form 5159 data) with monthly Current Population Survey data 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.

In Figure 2, we compare the short-term UI recipiency rate in North Carolina relative to other Southern states that had no changes to their UI programs during the sample period. While the two rates track one another closely prior to July 2013 (the implementation date of North Carolina's reform), they diverge sharply thereafter. Specifically, the fraction of short-term unemployed receiving UI benefits in North Carolina drops precipitously from 33% to 10%, a 59% decrease relative to control states, in just over 2 years. We use this large shock to study how the value of outside options influences the willingness to report sexual harassment.

4 Model

This section outlines a simple 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 rises. The intuition is that victims will "tough it out" rather than report and potentially jeopardize their current employment at a time when alternate labor market opportunities are limited (Biddle et al., 1998; Boone and Van Ours, 2006). The identifying assumption for this result is that a worker's willingness to file a sexual harassment claim depends only on her outside options and the severity of sexual harassment she experiences. Empirically, we also require that the EEOC applies a consistent standard of what constitutes a meritorious case, regardless of the state of the economy.

We begin by introducing a static version of the model in Boone and Van Ours (2006), which looks at the willingness of workers to report occupational injuries. We define $\alpha \geq 0$ as the level of sexual harassment a firm engages in against a particular employee. Given α, the worker decides whether to report the sexual harassment. Her harassment claim has probability $p(\alpha)$ of being successful, with $\partial p(\alpha)/\partial \alpha > 0$. Should the worker endure the harassment and not report, she receives a payoff of w, her wage, minus α.

If the worker instead reports, with probability $p(\alpha)$ the hostile work environment is eliminated and she receives compensation $\gamma(\alpha)$, which also depends positively on the level of harassment. However, such a worker also faces the possibility of a retaliatory firing with probability θ.[16] In this case 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 her new job, w', and the unemployment benefit, b: $V_u = q(u)w' + (1 - q(u))b$. The value of remaining employed is the worker's wage at their current job, w, minus the amount of sexual harassment they endure (which will be eliminated if their case is successful): $V_e = w - (1 - p(\alpha))\alpha$. Thus, a worker will report sexual harassment if:

$$p(\alpha)\gamma(\alpha) + \theta V_u + (1 - \theta)V_e \geq w - \alpha \qquad (1)$$

where the first term is the monetary gain if the claim is successful, and the next two terms capture the expected payoffs with and without a retaliatory firing. This can be rewritten with the expected gains on one side and the expected losses on the other:

[16]For simplicity, the probability of a retaliatory firing is not a function of α.

$$p(\alpha)\gamma(\alpha) + (1-\theta)p(\alpha)\alpha \geq \theta[w - \alpha - q(u)w' - (1-q(u))b] \tag{2}$$

The first term on the left hand side is the expected monetary payment and the second is the expected value of eliminating the harassment. We assume $w - \alpha > V_u$, i.e., that the worker does not prefer a retaliatory firing to keeping her job.

Since the left hand side is increasing in α while the right hand side is decreasing in α, we can define the threshold level of harassment above which an employee will report, $\bar{\alpha}$, as the level of α that satisfies equation 2 with equality:

$$p(\bar{\alpha})\gamma(\bar{\alpha}) + (1-\theta)p(\bar{\alpha})\bar{\alpha} = \theta[w - \bar{\alpha} - q(u)w' - (1-q(u))b] \tag{3}$$

Equation 3 can be used to analyze the comparative statics of how the reporting threshold $\bar{\alpha}$ responds to a lower value of becoming unemployed. A negative labor demand shock decreases the probability of finding another job, $q(u)$, which leads to a rise in the reporting threshold $\bar{\alpha}$. Similarly, a reduction in the generosity of unemployment benefits, b, lowers the value of unemployment and also leads to a rise in the reporting threshold.[17] The testable implication is that the probability a worker's claim will be successful increases when either $q(u)$ or b decreases.

Note that our theory does not model firm behavior and hence does not make predictions about the volume of reported claims. Our identifying assumption–that a worker's willingness to file a sexual harassment claim depends only on her outside options and the severity of sexual harassment she experiences–does not require us to model firm behavior. The expected benefits and costs to a firm of sexually harassing workers could depend on the business cycle. Moreover, an employer could take advantage of higher reporting thresholds by increasing the amount (i.e., number of employees harassed) and the severity of sexual harassment in the workplace. Whether this will show up in equilibrium as an increase or decrease in the volume of reported claims is ambiguous.

[17]More formally, we can define a reporting function, $R(\bar{\alpha}, u, b)$, that equals equation 3. Using the implicit function theorem to take derivatives, it is easy to verify the statements in the text.

5 Empirical Framework

5.1 Labor Demand Shocks and the Selectivity of Charges

We first 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-month level. Like Dahl and Knepper (2020), we exploit rich monthly heterogeneity in exposure to downturns across both geography and industries. We combine data on state-month unemployment from the LAUS with national industry-month unemployment and state-industry-month employment from the QCEW to impute U_{jst}, the number of unemployed individuals in a state-industry-month.[18]

As our primary hypothesis concerns how the selectivity of sexual harassment reports responds to the health of the relevant labor market, we first regress whether a charge received merit on U_{jst} while controlling for other relevant case characteristics as follows:

$$merit_{ijst} = \beta U_{jst} + \gamma_j + \alpha_s + \theta_t + \pi X_i + \epsilon_{ijst} \tag{4}$$

where i is the individual, j the industry, s the state, and t the month. The $merit_{ijst}$ variable is an indicator for whether the individual charge in a particular state-industry-month cell receives a merit designation by the EEOC. X_i measures the race, age, and sex of the charging party, though our results are invariant to the inclusion of these covariates. In all specifications, we cluster our standard errors at the state-level to allow for arbitrary correlation in merit decisions reached by each of the 53 local EEOC offices over the course of the sample period.

Following Maestas et al. (2021) and Dahl and Knepper (2020), 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 industry-state-time differ-

[18]See Dahl and Knepper (2020) for further details on the imputation procedure.

ences in the size of the labor force. Reassuringly, our results are robust to using levels instead of unemployment rates. We control further for γ_j, α_s, and θ_t, which capture fixed effects for industry, state, and time. In doing so, we absorb heterogeneity in charge selectivity that is unrelated to negative labor demand shocks at either the industry or state level, or part of a broader national trend. Our identifying assumption is that a worker's willingness to file a sexual harassment claim depends only on her outside options and the severity of sexual harassment she experiences after conditioning out the fixed effects.

5.2 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:

$$merit_{ist} = \phi \left(North\ Carolina_{is} \times post_{st}\right) + \alpha_s + \theta_t + \epsilon_{ist} \tag{5}$$

The coefficient of interest is ϕ, the effect of the North Carolina UI reform on the quality of sexual harassment charges. As before, α_s and θ_t are state and month-year 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 a yearly event study 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), but excludes those from the West South Central (LA, OK, TX) as this division exhibits differential pre-trends in merit (see Appendix Figure A1).

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.

6 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.

6.1 Labor Demand Shocks and the Selectivity of Charges

Figure 3 depicts graphically the relationship between the unemployment rate in an industry-state-month cell and the fraction of sexual harassment charges deemed to have had merit by the EEOC after residualizing out state, industry, and time fixed effects. There exists a clear, positive relationship between the two variables, indicating that victims of sexual harassment are relatively more selective about filing charges in weak labor markets. This accords with the theoretical 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.

Column (1) of Table 3 reports regression estimates for the effect of a 1 person increase in the number unemployed in a state-month-industry on the fraction of sexual harassment charges with merit, controlling for industry, state, and time fixed-effects (and the victim's race, sex, and age). To calculate the effect of a 1 percentage point increase in the local unemployment rate on merit, we multiply β by 1% of the size of a labor force in a state-industry-month cell, which is approximately 623,000. We find that each one percentage point increase in the local unemployment rate increases the fraction of charges with merit by 0.0012 percentage points. This amounts to approximately a 0.5 percent increase relative to the mean. During the Great Recession unemployment rose from 4.5% to a 10%, so that from the trough to the peak, our estimates imply that merit increased by 2.5%.

We next restrict attention to charges involving employer retaliation in column (2). The rationale is that we can be certain that these charges were filed while the individual was *still employed.* Non-retaliatory sexual harassment charges, on the other hand, include some that were filed following a discharge. Because there is no threat of job loss, discharged employees may in fact face a lower threshold for filing a discrimination or harassment complaint. The estimate using this alternative subsample is slightly larger.

Columns (3)-(4) of Table 3 re-estimate the above relationships, relying only on state-month variation in the unemployment rate. To determine the effect of a 1 percentage point increase, we multiply the coefficient of interest by 1% of the average size of a state's labor force, which is 5.7 million. Both specifications imply that a 1 percentage point increase in local unemployment increases the quality of sexual harassment charges by roughly 1%.

As a robustness exercise, in Appendix Table A1, we re-estimate the baseline relationship but instead use the imputed unemployment rate and employment-to-population ratios as the relevant measures of labor market tightness. Column (1) reveals that each one percentage point increase in the local unemployment rate increases the fraction of charges with merit by 0.0029 off a baseline of 27%, which translates to a 1.1% increase. Similar effects are found for retaliatory charges in column (2). Likewise, each one percentage point *decrease* in the employment-to-population ratio generates a 2.1 (1.4) percentage point increase in the quality of all (retaliatory) sexual harassment charges filed.

Recall that a charge may be 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, we run a multinomial logit which has three outcomes: no merit, merit due to settlement, and merit due to reasonable cause. The implied effects of a 1% increase in unemployment, evaluated at the means of the number unemployed in an industry-state-month, are reported in Appendix Table A2. We find unemployment effect sizes which are similar for both merit due to settlement (a 0.53% increase) or reasonable cause (a 0.38% increase). The two are jointly significant, but due to collinearity, only one of the two is individually statistically significant. When restricted to retaliatory charges, the effect on merit due to reasonable cause grows to 0.91% and both estimates become individually statistically significant.

If individuals are more liquidity constrained during recessions, they could have substituted towards settlement more often in order to avoid a roughly 10-month EEOC investigation to determine cause. However, as the multinomial logit estimates show, this does not appear to be the case, as both settlements and reasonable cause rulings increase.

6.2 Labor Demand Shocks and the Volume of Charges

As a reminder, our model does not make a prediction about the volume of reported claims, as this would require assumptions about firm behavior. With this caveat in mind, Table A3 shows the effects of unemployment on the volume of sexual harassment charges being filed.[19,20] These capture any potential responses by the firm to the increase in victims' reporting thresholds. Absent any employer response, an increase in selectivity would imply a reduction in the volume of sexual harassment charges filed. However, we find that a 1 percentage point increase in local unemployment rates *increases* the volume of charges filed by 0.6-1.3 percentage points, although the estimates are somewhat imprecise. This positive effect suggests that in spite of victims increasing the selectivity of their reports, employers sexually harass their employees in a way that exceeds this threshold at an even higher rate in weak labor markets. In other words, firms are willing to get caught at a higher frequency when the reporting threshold increases.

[19]Because these data are collapsed to the state-month-industry level–unlike the individual-level merit regressions–we weight by the size of the relevant labor force.

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

6.3 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 and amount of sexual harassment charges. Since 84% of all victims in our data are female, this subsection focuses on women.

Figure 4a shows that the selectivity of charges is monotonically increasing in the fraction of male workers at an establishment, while at the same time panel 4c documents that the number of charges per woman rises. 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 (2020); a measure of selectivity is not contained in their dataset, so no comparison is possible on that margin.

In Figure 4b, we explore 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. Panel 4d shows that charge volume increases monotonically in the fraction of male managers. Combined, these patterns suggest that a lack of women in power contributes both to a more sexually hostile work environment and to an increased fear of reporting illegal behavior due to a fear of retaliation. The volume result is interesting, as it diverges from the survey-based evidence in Folke and Rickne (2020). One possible explanation is that their measure captures whether an individual's closest supervisor is of the opposite sex whereas ours summarizes the extent to which a power hierarchy runs across gender lines more broadly within a workplace.

Table 4 displays heterogeneity in the selectivity of harassment charges filed by employees during recessions by (i) the percent of men in an industry and (ii) the extent to which males dominate managerial roles within a workplace. The table uses the same specification as Table 3. A one standard deviation increase in either of these margins amplifies the underreporting effect due to one percentage point higher unemployment by approximately 0.3%. Taken together, these results suggest that the threat of retaliation from male colleagues and male managers becomes an increasingly effective deterrent to reporting as outside labor market opportunities become scarcer.

7 North Carolina's UI Reform and Underreporting

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

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. These curtailments sharply reduced the short-term UI recipiency rate relative to other Southern states which did not change their UI programs (see Section 2.2 and Figure 2). 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 63% 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 5a we plot the fraction of cases with merit in each year before and after the reform, 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.[21] Prior to the reform, merit in both treatment and control states is trending down, consistent with selectivity falling as the economy recovered from the Great Recession. 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.

[21] In the year leading up to the reform, North Carolina's merit rate was 17%, while it was 24% in other Southern states.

In Figure 5c we plot regression-adjusted (netting out state and year fixed effects) event-study coefficients for the difference between treatment and control. There is a 7 percentage point rise in North Carolina relative to other Southern states in the year after the reform. This elevated rate persists in years two and three post reform, consistent with the fact that North Carolina's reduction in UI benefits was permanent.

Instrumental variable regression estimates can be found in columns (1) and (2) of Table 5. The first stage regresses the short-term UI recipiency rate in a state and year on a North Carolina post-reform dummy. Panel A of Table 5 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 33%, 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 0.35 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.

As an alternative approach, we estimate treatment effects using various types of synthetic control groups. To start, we construct synthetic weights by matching on the state female unemployment rate. Matching uses both the pre-reform state female unemployment rate as well as a variety of other pre-reform labor market characteristics as predictor variables. The extra predictors include the overall state unemployment rate, state female employment to population ratio, the state female labor force participation rate, and state-industry employment weights. Note that this approach does not match on the outcome variable of merit, or use merit to predict state female unemployment rates. These weights are found in column (1) of Appendix Table A4. Alabama contributes almost half the weight, with Oregon, Connecticut, Nevada, and New Jersey being the other top donor states. There are 35 possible donor states as we exclude states that had changes to their UI program and small states that had 0 sexual harassment charges in at least 1 year.

Figure 5b plots the fraction of cases with merit for North Carolina as compared to our synthetic control sample, similar to Figure 5a. The synthetic controls, which were not constructed by matching on the outcome variable, nevertheless closely track 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. Figure 5d plots the event-study coefficients, and reveals a more than 8 percentage point rise in merit in North Carolina in all years following the reform. Columns (3) and (4) of Table 5 report corresponding IV regression estimates. The estimates are similar to those which use other Southern states as controls.

Figure A2 plots the merit rate in North Carolina against each of the 7 donor states to provide a visualization of the trends in each individual state used to construct the synthetic control. Figure A3 performs a standard placebo test which plots the difference between the merit rate in each state and that of its own synthetic control group. While the pre-reform placebo differences in merit are somewhat noisy, North Carolina exhibits among the largest growth in merit compared to its synthetic control group in the post-reform period.

To probe the robustness of our synthetic control estimates, we add merit as an additional predictor variable, but keep the matching variable as the state female unemployment rate. This alters the set of donor states, with weights primarily on Nevada, Kentucky, California, and Arizona (see Appendix Table A4). As another alternative, we match on the outcome variable of merit using only state unemployment and merit as predictor variables. The largest donor states are now Nevada and Iowa. Event study graphs for both of these specifications are found in Appendix Figure A4. Using either approach, the general pattern replicates.

From the analyses of North Carolina's UI reform, we conclude that willingness to report sexual harassment depends crucially on outside options.

8 Conclusion

This paper provides a novel exploration of 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. The nature of the data allows us to focus on a margin that has been largely overlooked in the literature: worker reporting incentives. This element is often missed when analyzing anonymous surveys of workplace sexual harassment, as victims are shielded from any economic consequences associated with outing their employers. We view the two types of data as useful complements for a better understanding of workplace sexual harassment, and how best to combat it.

We find that reductions in outside job opportunities and in the generosity of unemployment insurance each make employees more vulnerable to sexually hostile work environments. We also document rich heterogeneity along several dimensions using linked employee-employer data that connects sexual harassment charges to the gender make-up of 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. 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 encouraging victims to report sexual harassment they might otherwise not.

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9 Figures

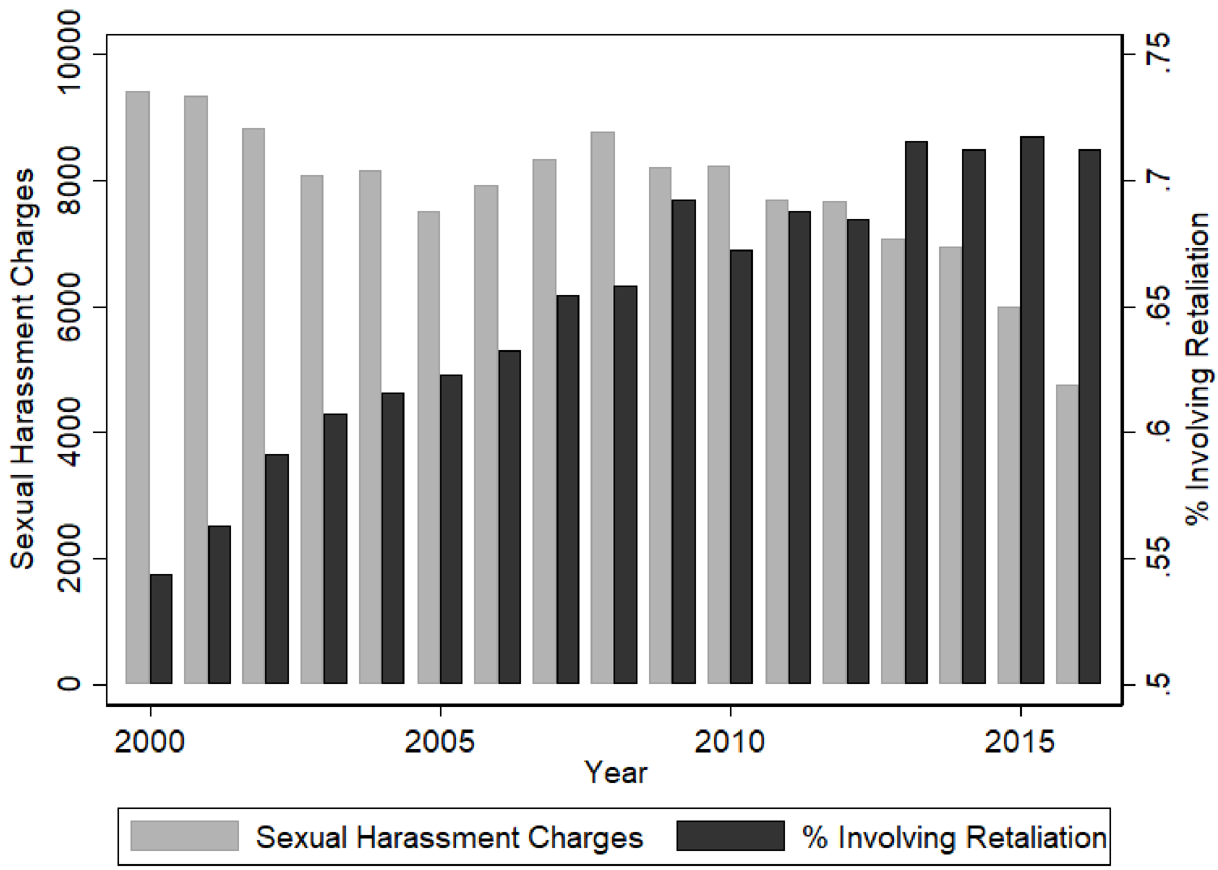


Figure 1: Sexual Harassment Charge Volume and % Involving Employer Retaliation

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 2010 and first 9 months of 2011, and so charges in these years are scaled by 0.75 and 0.25, respectively.

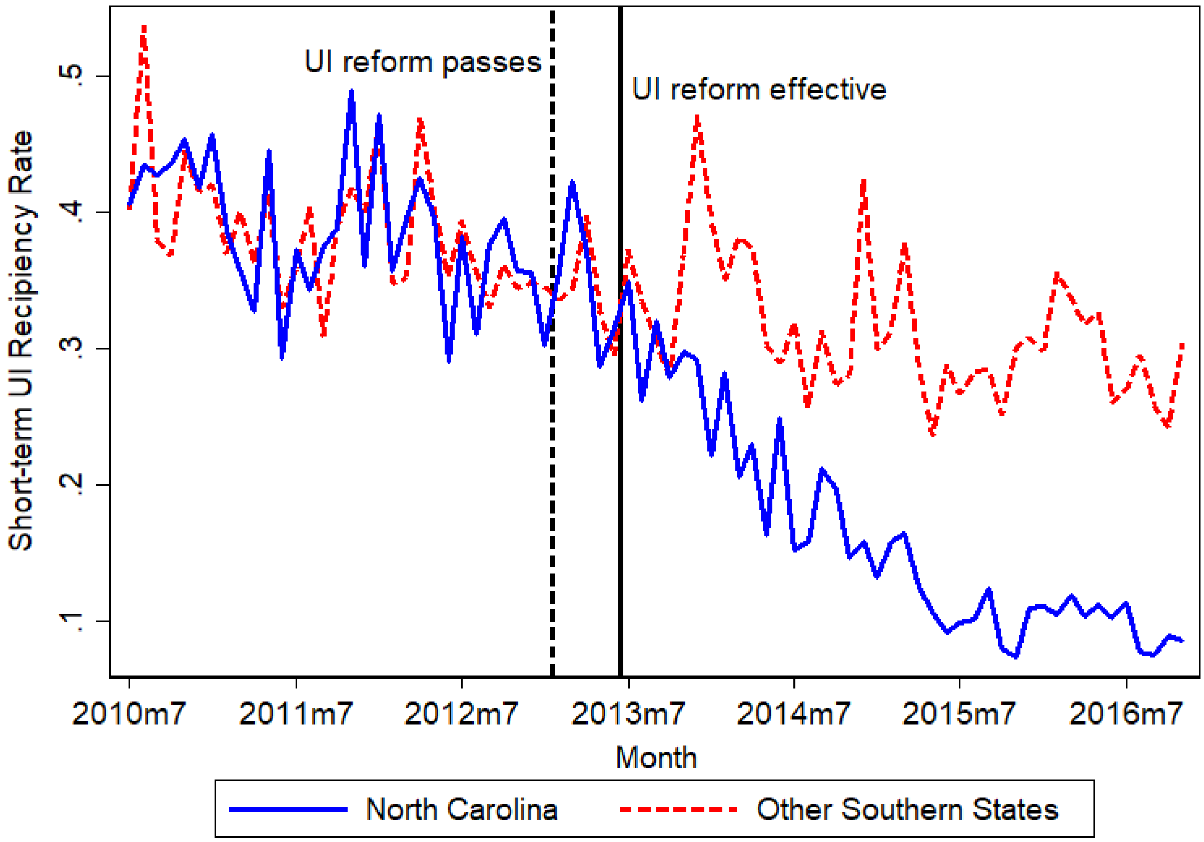


Figure 2: UI Recipiency Rate for North Carolina versus Other Southern States

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.

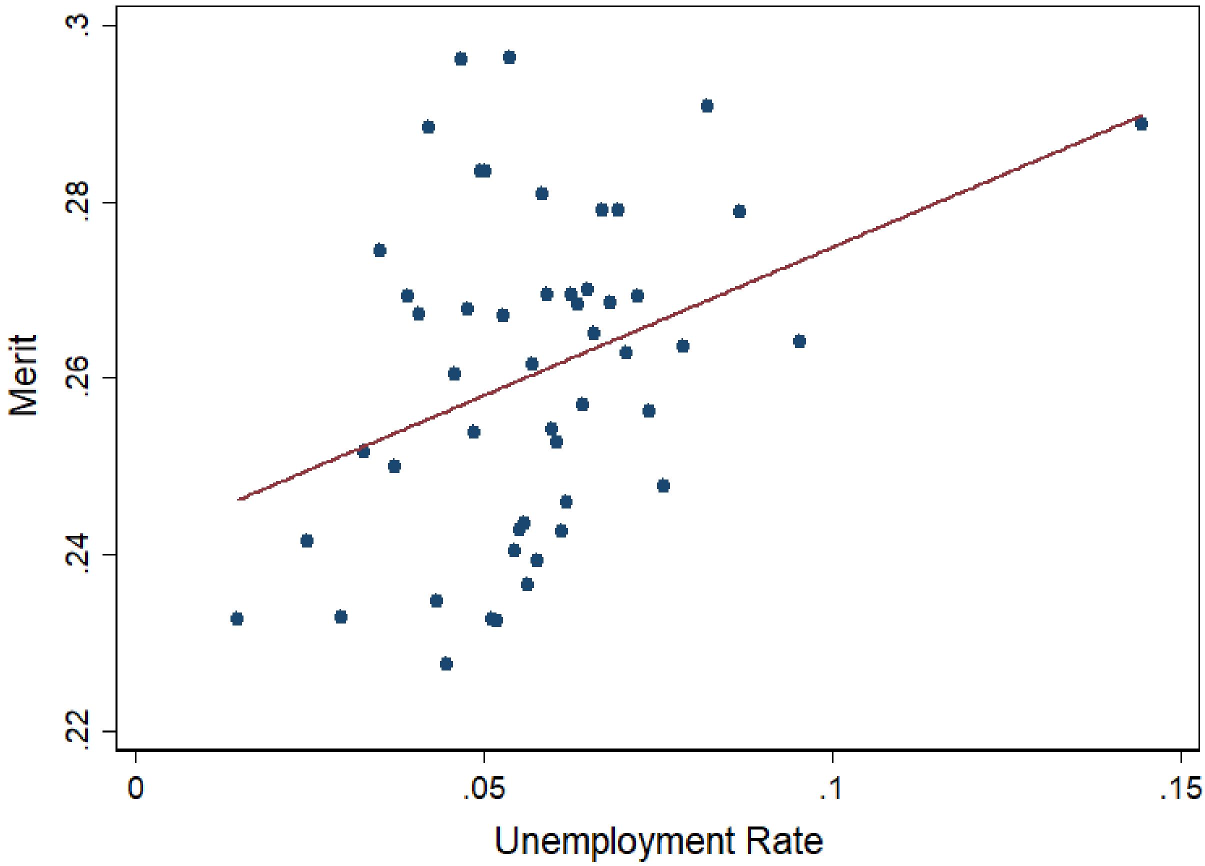
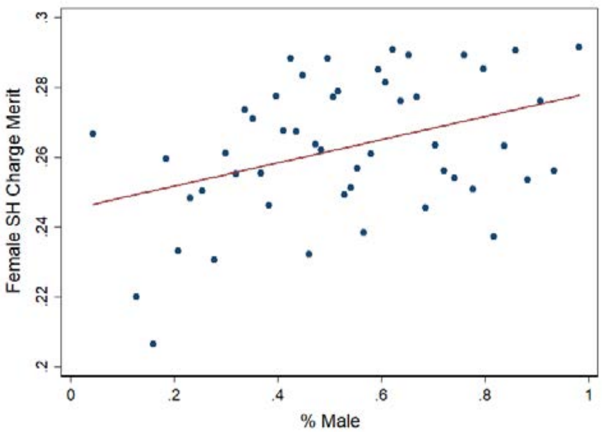
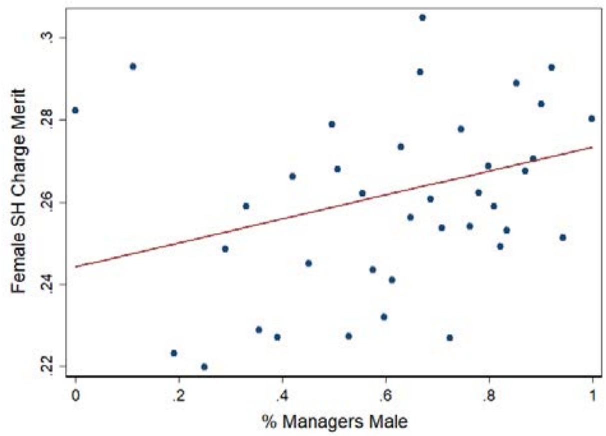


Figure 3: Unemployment Rate and the Fraction of Sexual Harassment Charges with Merit

The y-axis measures the fraction of charges determined to have merit by the EEOC in a state-industry-month unemployment rate bin. Both variables residualize out state, industry, and time fixed effects. Observations are weighted by the industry share of employment in each state's labor force, and cover the period from 2000-2015.



(a)



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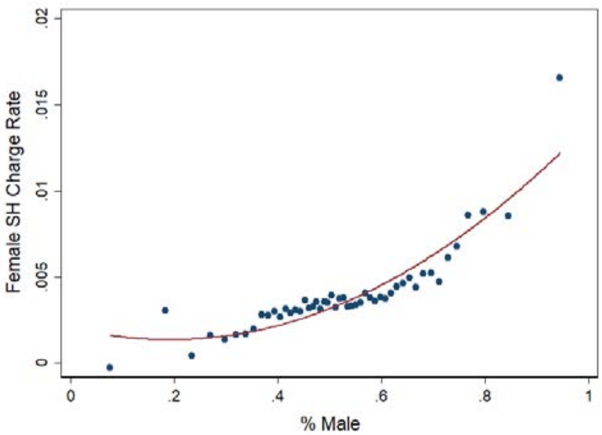

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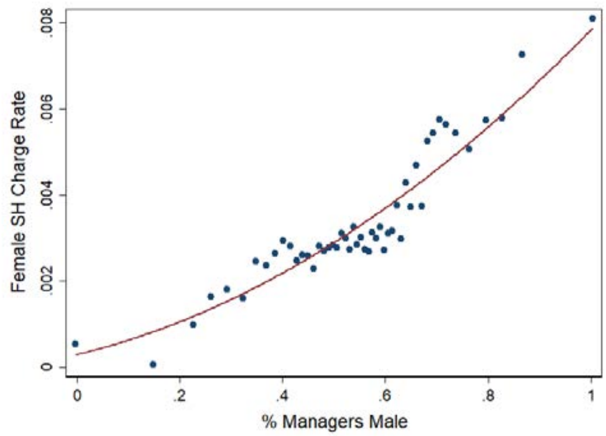


(d)

Figure 4: SH Merit and Charges by % Male and % Managers Male within Establishments

Female SH Charge Merit and Rate, respectively, measure the fraction of sexual harassment charges filed by women determined to have merit by the EEOC, and the number of charges filed by women divided by the number of women in an establishment. % Male and % Managers Male, respectively, are the fraction of males and fraction of managers who are male in an establishment. All variables residualize out month-year, state, and industry fixed effects, as well as the size of the establishment and the number of establishments within the enveloping firm. Linear and quadratic regression lines appear in the top and bottom panels, respectively. Sample period covers 2000-2015.

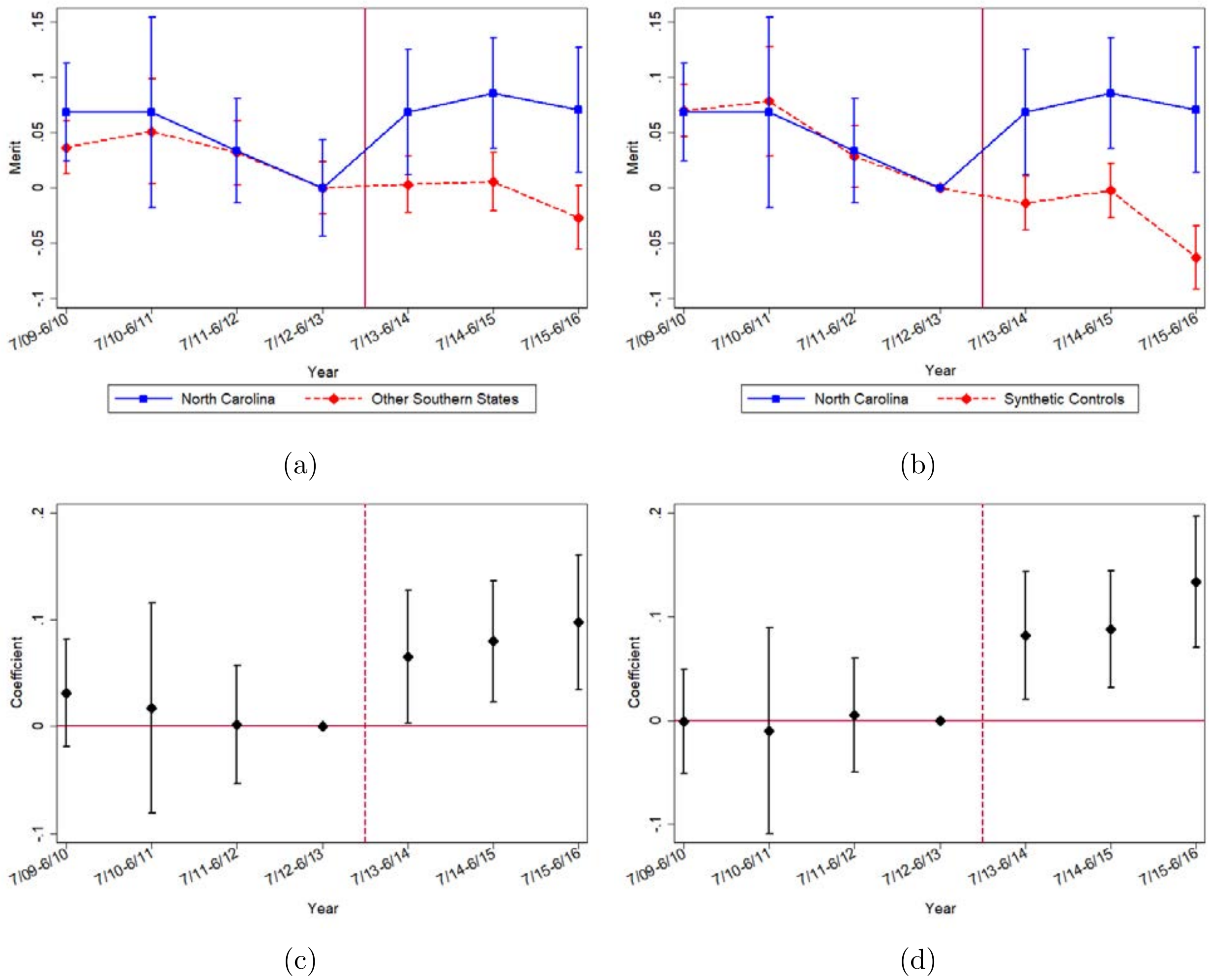


Figure 5: Selectivity of Sexual Harassment Charges for North Carolina versus Controls

The top panel reports raw averages, while the bottom panel reports event-study coefficients. 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 2 for the list of Other Southern States. Synthetic weights are constructed by matching on the state female unemployment rate using both pre-reform state female unemployment rate and the extra predictors of the overall state unemployment rate, state female employment to population ratio, the state female labor force participation rate, and state-industry employment weights. Note that this approach does not match on the outcome variable of merit, or use merit to predict state female unemployment rates. Weights can be found in column (1) of Appendix Table A4.

10 Tables

Table 1: Summary Statistics by Claim Type

	Sexual Harassment	No Sexual Harassment
Resolutions by Type		
Merit	0.270	0.186
Settlement with benefits	0.114	0.093
Withdrawal with benefits	0.059	0.049
Reasonable cause	0.096	0.038
Successful conciliation	0.029	0.014
Unsuccessful conciliation	0.067	0.030
Case Characteristics		
Employer retaliation involved	0.634	0.307
Employer intimidation involved	0.057	0.027
Legal representation	0.130	0.079
Worker Characteristics		
Female	0.839	0.519
Age	36.4	44.6
White	0.541	0.402
Black	0.258	0.439
Selected Firm Characteristics		
Accommodation & Food Services	0.138	0.059
Retail	0.134	0.112
Manufacturing	0.125	0.145
Health Care & Social Assistance	0.100	0.134
Public Administration	0.070	0.084
Charges	61,558	648,999

Data are from EEOC Charge Database, 2000-2015.

Table 2: Establishment Characteristics using Linked Data

	Any Sexual Harassment Charges	No Sexual Harassment Charges
Demographics		
Male	0.538	0.510
% Managers Male	0.654	0.604
% Female Laborers	0.028	0.015
% Female Service Workers	0.090	0.082
White	0.625	0.691
Black	0.182	0.125
Size		
Employees	497.1	103.3
Plants within Firm	428.1	758.7
Selected Industries		
Manufacturing	0.193	0.118
Retail	0.171	0.213
Health Care & Social Assistance	0.123	0.119
Accommodation & Food Services	0.111	0.085
Admin Support/Waste Management	0.081	0.052
Census Region		
Midwest	0.254	0.234
Northeast	0.131	0.171
South	0.426	0.381
West	0.188	0.214
Establishment-years	31,230	7,706,499

Establishment characteristics based on whether at least one employee filed an sexual harassment charge against their company in an establishment-year. 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.

Table 3: Unemployment and the Quality of Sexual Harassment Charges

	State x Industry Variation		**State Variation**	
	All	*Retaliatory*	*All*	*Retaliatory*
Dep var = $\mathbb{1}(merit)$	(1)	(2)	(3)	(4)
# unemployed_{jst} †	19.9***	29.6***		
	(6.87)	(8.34)		
# unemployed_{st} †			4.58***	4.71**
			(1.68)	(1.89)
female	0.054***	0.057***	0.055***	0.057***
	(0.006)	(0.006)	(0.006)	(0.007)
Effect of 1 pp ↑ unemp	**0.0012**	**0.0019**	**0.0026**	**0.0027**
Mean(merit)	.270	.267	.270	.267
% change	0.46	0.71	0.96	1.02
Elasticity	0.025	0.038	0.059	0.064
State, industry, month-year FEs	X	X	X	X
N charges	61,558	39,039	61,578	39,053
R^2	0.021	0.023	0.025	0.028

Regression estimates based on equation 4 and the sample period of 2000-2015. Bolded 'Effect of 1 pp ↑ unemp' is the implied effect of a one percentage point increase in a state-industry's (or state's) monthly unemployment rate on the fraction of charges found to have had merit by the EEOC. Additional controls include an individual's race and age. Robust standard errors, clustered at the state level, are reported in parentheses.

† Coefficients are multiplied by 10^{-8}

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Female Charge Quality, by % Male and % Male Managers using Linked Data

Dep var = $\mathbb{1}(merit)$	*All* (1)	*Retaliatory* (2)	*All* (3)	*Retaliatory* (4)
# unemployed$_{jst}$ × %men$_j$ †	9.92***	8.08**		
	(2.53)	(3.62)		
# unemployed$_{jst}$ × %male managers$_{estab}$ †			4.80***	4.04**
			(1.36)	(1.63)
sd(% men or % male mgrs.)	0.158	0.160	0.259	0.260
Effect of 1 sd ↑ % men or % male mgrs.	**0.0010**	**0.0008**	**0.0008**	**0.0007**
Mean(merit)	.279	.277	.263	0.265
% change	0.35	0.29	0.30	0.25
State, industry, month-year FEs	X	X	X	X
N charges	51,286	32,730	23,424	15,748
R^2	0.021	0.026	0.024	0.031

Regressions mirror those in Table 3 and are based on the sample period of 2000-2015. 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. Bolded 'Effect of 1 sd ↑ % men or % male mgrs.' is the implied effect of a one percentage point increase in a state-industry's monthly unemployment rate interacted with the % of men in an industry or % of male managers in an establishment on the fraction of charges found to have had merit by the EEOC. Additional controls include an individual's race and age, the number of unemployed, and the % men or the % male managers. Robust standard errors, clustered at the state level, are reported in parentheses.

† Coefficients are multiplied by 10^{-7}

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: North Carolina UI Reform and the Quality of Sexual Harassment Charges

	Panel A: First Stage			
	Other Southern States as Controls		*Synthetic Controls Matching on Female Unemployment Rate*	
Dep var = UI recipiency rate$_{st}$	(1)	(2)	(3)	(4)
North Carolina × post	-0.198***		-0.183***	
	(0.003)		(0.002)	
Dep mean NC pre-reform	.460		.460	
State and month-year FEs	X		X	
N charges	8,792		8,845	
R^2	0.812		0.914	
	Panel B: Reduced Form and IV			
Dep var = $\mathbb{1}(merit)$	Reduced Form	IV	Reduced Form	IV
North Carolina × post	0.070***		0.081***	
	(0.024)		(0.020)	
UI recipiency rate$_{st}$		-0.352***		-0.445***
		(0.131)		(0.127)
Dep mean NC pre-reform	.211	.211	.211	.211
% change	33.0		38.5	
State and month-year FEs	X	X	X	X
N charges	8,792	8,792	8,845	8,845
R^2	0.014	0.012	0.024	0.021

See Table 2 for the list of Other Southern States and column (1) of Appendix Table A4 for the set of synthetic control weights. Sample period is from July of 2009 through September of 2016. Robust standard errors, clustered at the state-year level, are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

11 Online Appendix Figures and Tables

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

By Gordon B. Dahl and Matthew Knepper

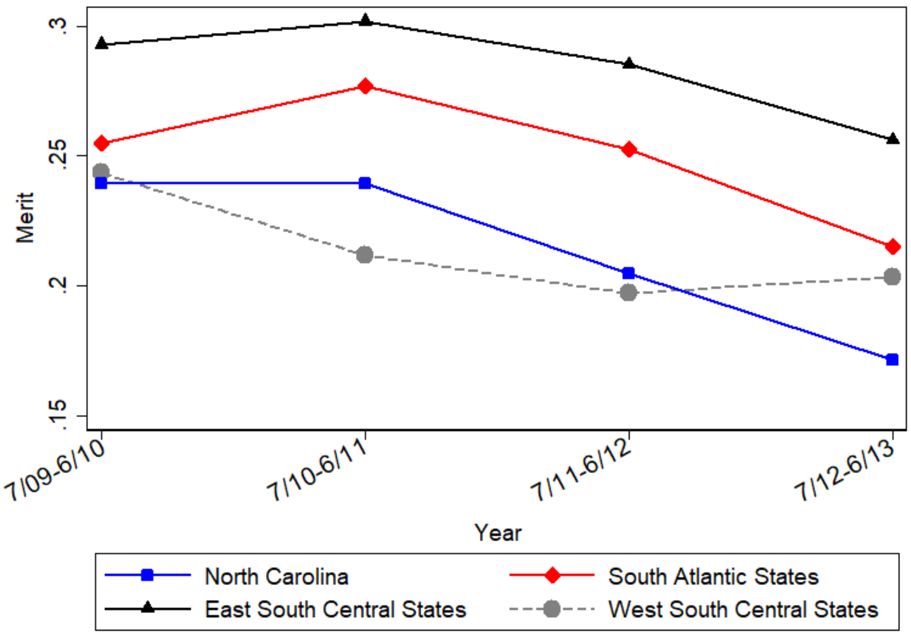


Figure A1: Merit Pre-trends in North Carolina versus each Southern Census Division

Nontreated states in the South Atlantic Census Division are Delaware, the District of Columbia, Maryland, Virginia, and West Virginia. Nontreated states in the East South Central Census Division are Alabama, Kentucky, Mississippi, and Tennessee. Nontreated states in the West South Central Census Division are Louisiana, Oklahoma, and Texas. Four Southern states which changed their UI programs are excluded (Arkansas, Florida, Georgia, South Carolina).

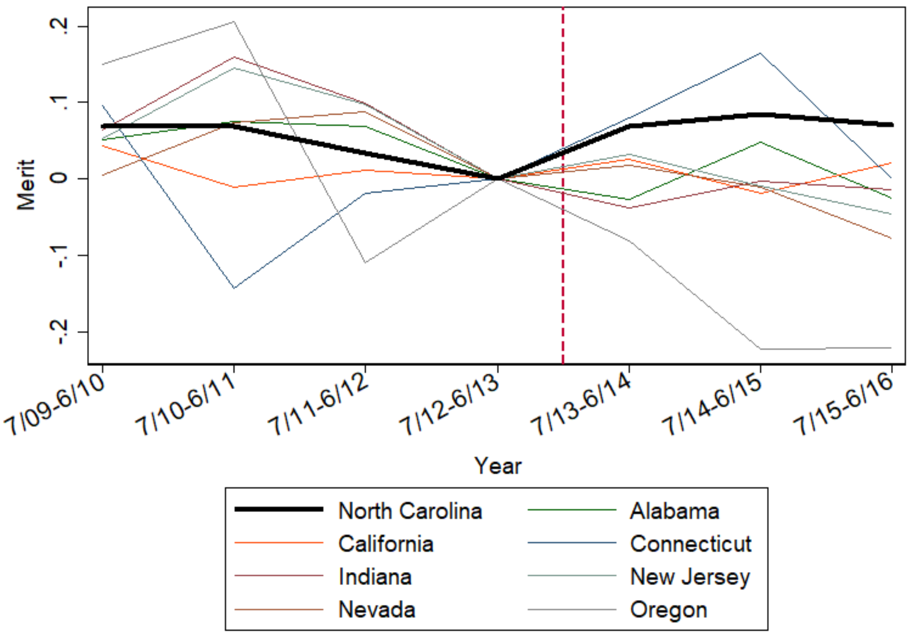


Figure A2: Merit in North Carolina versus each Synthetic Control State

Weights for each synthetic control state are found in column (1) of Appendix Table A4.

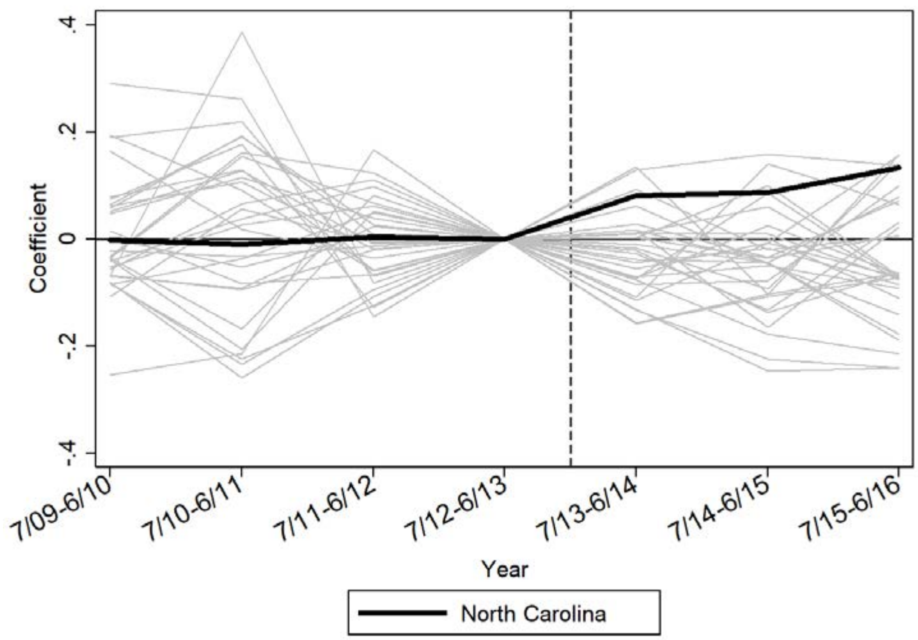
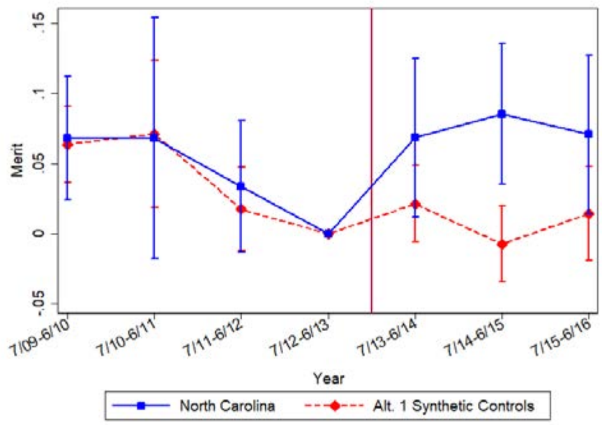
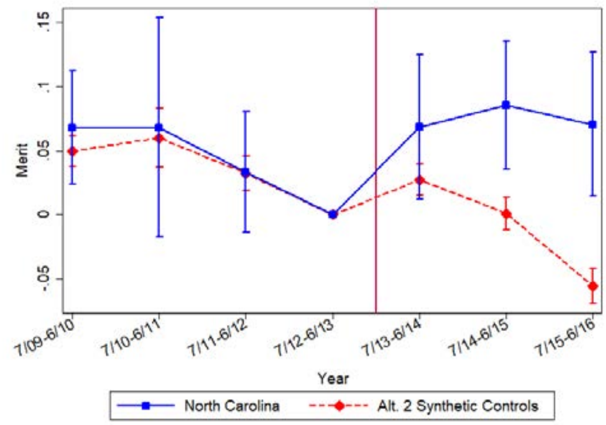


Figure A3: Merit Gaps in North Carolina and Placebo States

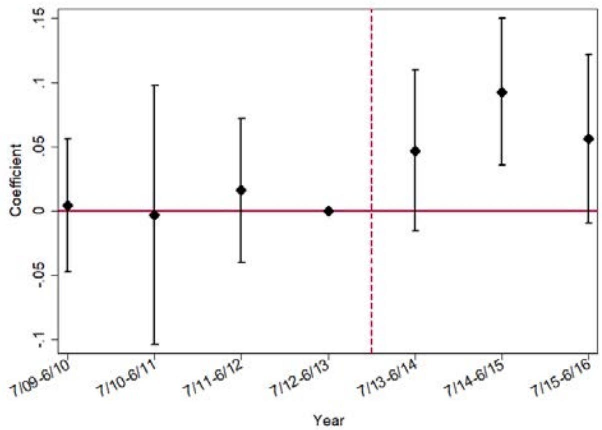
Light gray merit gaps represent the coefficients for each placebo state relative to its synthetic control. North Carolina's merit gap is shown in black.



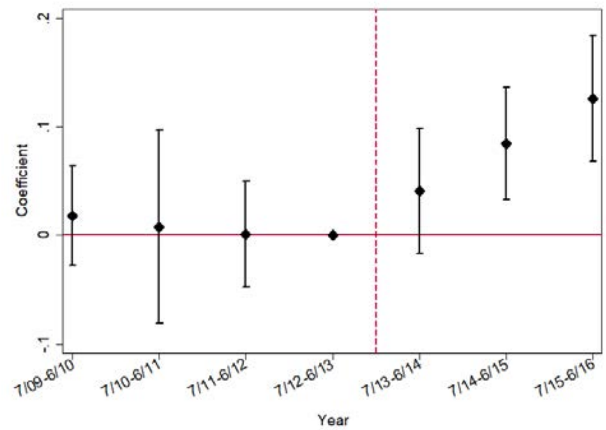
(a)



(b)



(c)



(d)

Figure A4: Event-studies Using Two Different Alternative Synthetic Controls

See notes to Figure 5. Vertical bars denote 95% confidence intervals. The first column (Alt. 1 Synthetic Controls) mirrors the approach of Figure 5 but adds merit as a predictor variable. The second column (Alt. 2 Synthetic Controls) instead matches on the outcome variable of merit using state unemployment and merit as predictor variables.

Table A1: Alternative Measures of Labor Market Tightness

Dep var = $\mathbb{1}(merit)$	*All* (1)	*Retaliatory* (2)	*All* (3)	*Retaliatory* (4)
unemployment rate$_{jst}$	0.289***	0.269***		
	(0.0906)	(0.0963)		
employment:population$_{jst}$			-0.568***	-0.368**
			(0.167)	(0.180)
female	0.0480***	0.0478***	0.0478***	0.0474***
	(0.0052)	(0.0071)	(0.0052)	(0.0072)
Effect of 1 pp ↑	**0.0029**	**0.0027**	**-0.0057**	**-0.0037**
Mean(merit)	.270	.267	.270	.267
% change	1.07	1.00	-2.10	-1.36
Elasticity	0.067	0.063	-0.134	-0.087
State, industry, month-year FEs	X	X	X	X
N charges	61,450	38,981	61,450	38,981
R^2	0.0218	0.0248	0.0220	0.0249

These regressions mirror those in Table 3, but instead of using the number unemployed, they use the unemployment rate or employment to population ratio. Robust standard errors, clustered at the state level, are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Multinomial Logit Model (Baseline Outcome = No Merit)

Panel A: Outcome = Settlement	*All* (1)	*Retaliatory* (2)
1% increase in unemployed$_{jst}$	0.0053***	0.0052**
	(0.0020)	(0.0024)
Panel B: Outcome = Reasonable Cause		
1% increase in unemployed$_{jst}$	0.0038	0.0091***
	(0.0029)	(0.0033)
state, industry, month-year FEs	X	X
χ^2 test for joint significance	12.78	17.19
p-value	0.0017	0.0002
N charges	61,558	39,039

Estimates are reported as marginal effects for a 1% increase in unemployment (6,230), evaluated at the mean number of unemployed in a state-industry-month (33,546). The χ^2 test refers to the null hypothesis that the 2 coefficients are jointly zero. Additional controls include an individual's sex, race, and age. The sample period spans 2000-2015. Robust standard errors, clustered at the state level, are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Unemployment and the Volume of Sexual Harassment Charges

	State x Industry variation		**State variation**	
	All	*Retaliatory*	*All*	*Retaliatory*
Dep var = # of charges	(1)	(2)	(3)	(4)
# unemployed$_{jst}$	2.91	2.46*		
	(2.27)	(1.47)		
# unemployed$_{st}$			2.75**	3.60***
			(1.21)	(1.05)
Effect of 1 pp ↑	4.40	3.72	4.15	5.44
mean(# national charges)	667.7	428.0	667.7	428.0
% change	0.66	0.86	0.62	1.27
Elasticity	0.042	0.055	0.039	0.080
State, Industry, Time FEs	X	X	X	X
labor force size (thous.)	151,055	151,055	151,055	151,055
N (cells)	120,906	120,906	9,985	9,985
R^2	0.409	0.353	0.884	0.872

These regressions mirror those in Table 3, but use the number of charges filed as the dependent variable. Regression coefficients estimate the change in charges filed for 1,000,000 person increase in the number unemployed. Observations are weighted by the industry share of employment in each state's labor force in columns (1) and (2) and by the state labor force size in columns (3) and (4). Bolded 'Effect of 1 pp ↑' is the implied effect of a one percentage point increase in the national unemployment rate on the national monthly number of sexual harassment charges filed. Robust standard errors, clustered at the state level, are reported in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Synthetic Control Weights

State	(1)	(2)	(3)
Alaska	0	0	0.021
Alabama	0.479	0	0.016
Arizona	0	0.135	0.019
California	0.046	0.194	0.018
Colorado	0	0	0.011
Connecticut	0.107	0	0.016
DC	0	0	0.014
Delaware	0	0	0.012
Hawaii	0	0	0.008
Iowa	0	0.05	0.102
Idaho	0	0	0.01
Indiana	0.003	0	0.018
Kentucky	0	0.20	0.017
Louisiana	0	0	0.011
Massachusetts	0	0	0.011
Maryland	0	0	0.011
Minnesota	0	0	0.008
Mississippi	0	0	0.016
North Dakota	0	0	0.002
Nebraska	0	0	0
New Jersey	0.083	0	0.02
New Mexico	0	0	0.012
Nevada	0.099	0.292	0.485
New York	0	0	0.013
Ohio	0	0	0.015
Oklahoma	0	0	0.008
Oregon	0.183	0.029	0.015
Pennsylvania	0	0	0.011
Tennessee	0	0	0.014
Texas	0	0	0.011
Utah	0	0	0.009
Virginia	0	0	0.008
Washington	0	0	0.014
Wisconsin	0	0	0.012
West Virginia	0	0.10	0.014

In column (1), weights are determined by matching on state female unemployment rate in the pre-period using state female unemployment rate, overall state unemployment rate, state female employment:population, state female labor force participation rate, and state-industry employment weights. In column (2) weights are determined by matching on the same covariates as in column (1), except we add merit as an additional predictor variable. In column (3) weights are determined by matching on merit in the pre-period using only state unemployment and merit as predictor variables.