The Effects of Sexual Harassment Law on Gender Inequality

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ABSTRACT

Interpreting anti-discrimination law to forbid sexual harassment has been one of the key practical contributions of feminist legal theory. Unlike other employment laws, sexual harassment law is generally considered "good" social policy and has not come under fire for its potential negative consequences in the way that other employment protections, such as the Americans with Disabilities Act and maternity mandates, have. Yet, economic theory, at first glance, suggests that the potential effects of forbidding sexual harassment may be similar to those of other employment mandates. It may exacerbate gender inequality overall because it could be viewed as a tax on the hiring of women. We identify the impact of court-made sexual harassment law on gender inequality by using the fact that federal judges are randomly assigned to appellate cases along with the fact that female judges and Democratic appointees decide sexual harassment cases differently than do male judges and Republican appointees. We find that sexual harassment law does not appear to exacerbate gender inequality. It increases female wages and employment relative to that of men. It also increases the proportion of female managers relative to male managers. When, however, restricted to people previously in the work force, sexual harassment law worsens female employment outcomes. These findings are more consistent with an insider-outsider theory of involuntary unemployment, where insiders harass outsiders in order to capture economic rents and forbidding harassment raises both employment and wages of outsiders, than with compensating wage differential models of sexual harassment. One of the more surprising results is that the positive effect on female management comes entirely from sexual harassment law, not gender discrimination law, highlighting a practical contribution of feminist interpretation. Moreover, while damages awarded in sexual harassment cases have a positive effect on gender inequality, law trumps economics, particularly legal doctrine, in a horse race between different measures of sexual harassment law, providing novel evidence that people may obey the law because of its legitimacy rather than its incentive effects.

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I. Introduction

Interpreting anti-discrimination law to forbid sexual harassment has been one of the key practical contributions of feminist legal theory (MacKinnon 1979; Fisher and Kennedy 2007), but little research has examined its economic impact. Legal scholars view harassment as a form of economic warfare by men battling to preserve their jobs (Schultz 1998). Under this view, forbidding harassment opens job opportunities in previously harassing work environments as insiders can no longer harass outsiders in order to capture economic rents (Lindbeck and Snower 1988). This perspective on the regulation of sexual harassment predicts that forbidding sexual harassment would attract women into the labor force since they can now compete for jobs previously dominated by men.

Economic theory, however, has to date primarily viewed sexual harassment through the lens of compensating wage differentials. The fact that sexual harassment insurance for corporations exists is suggestive evidence of a compensating wage differential (Smith 1997). Assuming that the effects of sexual harassment law were predominantly experienced by women, forbidding harassment may lower female wages, both through the resulting decreased demand for and increased supply of female labor. The law, like a mandated benefit, acts like a tax on labor demand (Summers 1989). Firms face a cost of compliance, both through the fixed costs of establishing internal infrastructures conducive to complaint, and marginal costs associated with each female worker who has some probability of filing a complaint or becoming a litigant. The law also makes work more pleasant for women who would be willing to work for lower wages in work environments that previously allowed harassment. In the legal literature, these theoretical effects are described and analogized to those of accommodation mandates (Jolls 2000 and

2001[3]). If employees value the benefit at cost, the resulting equilibrium will result in the same level of employment but with the full cost reflected in lower wages. If there are wage rigidities, then the cost of the benefit cannot be reflected in wages and thus, unemployment may result (Summers 1989). The effect on male wages is reversed; men would now be compensated for the lost utility from harassment. These hypothesized effects - lower female wages and higher male wages - could be contributing to the unexplained persistence of the gender wage gap (documented in Blau and Kahn 2004; Card and DiNardo 2002).

At a practical level, sexual harassment law, which is primarily court-made, has likely impacted the behavior of firms in various ways. Specifically, litigation has imposed direct and indirect costs on firms in at least four distinct ways. First, for those cases that were actually litigated, the law resulted in direct litigation costs and potentially large damage awards. Second, the potential for litigation likely led many firms to settle with potential plaintiffs, particularly after such plaintiffs filed a complaint with the Equal Employment Opportunity Commission (EEOC) and received a right to sue letter from the agency. Third, many firms invested in formal grievance procedures, in part because of the advice of personnel experts who believed that instituting formal grievance procedures analogous to those for civil rights violations would help defend against damage awards (Dobbin and Kelly 2007). Fourth, some firms mandated training along the lines of the diversity training implemented in response to the Civil Rights Act of 1964 to educate their workers about sexual harassment. This last approach was controversial since

[3] "Restrictions on differential job conditions are just like accommodation requirements, for just as disadvantaged employees will be more willing to supply labor at any given wage once a particular benefit must be provided to them, they will be more willing to supply labor at any given wage once the workplace is free of differential job conditions. Effects parallel to those of accommodation requirements also occur for labor demand. Restrictions on differential job conditions impose costs on employers, just as do accommodation requirements; with such restrictions in place, employers are subject to a potential lawsuit over every adverse incident on the job suffered by a disadvantaged employee. These costs shift down the marginal revenue product of labor for disadvantaged employees, just as the costs associated with accommodation requirements shift down this marginal revenue product of labor." (Jolls 2001 p. 690)

some firms and attorneys feared that such training might make potential plaintiffs more aware of harassment and, therefore, more likely to sue (Dobbin and Kelly 2007).

Regardless of which of these consequences of increasing sexual harassment law imposed the greatest costs, they all imposed both direct and indirect costs on firms, which could have caused changes in employment and earnings. The theoretical effects of these costs on the relative wages and employment of women, however, are ambiguous. One might think of the law as a tax on the hiring of women, making it more costly to hire women. Accordingly, economic theory would predict lower wages and employment for women relative to men. While the Equal Pay Act of 1963 (see U.S.C. § 206(d)) makes lower wages for women for the same work illegal, women’s wages could be constrained through a failure to promote given the same qualifications. On the other hand, hiring more women and perhaps even promoting women to managerial status might very well change firm cultures, improve grievance procedures, reduce complaints, and ultimately reduce the costs of sexual harassment litigation. The ambiguity of theoretical predictability is what motivates our empirical analysis.

This paper builds on a substantively similar literature on the impact of discrimination law on inequality (Hellerstein, Neumark, and Troske 1997; Neumark and Stock 2001; Beller 1979; Eberts and Stone 1985; Autor, Donohue, and Schwab 2006; Acemoglu and Angrist 2001; Jolls and Prescott 2004) and methodologically related literature exploiting the random assignment of judges to cases to identify the impact of law on individual outcomes (Kling 2006; Chang and Schoar 2006). Although sexual harassment was legally recognized as being a form of discrimination, its economic interpretation is quite different from that of anti-discrimination law. While sexual harassment law can be thought of as a tax reducing the earnings of women, anti-discrimination law can be viewed as a subsidy, which increases the employment and earnings of the group being discriminated against. Chay (1998) finds that when smaller establishments were covered by the Civil Rights Act in 1972, the employment and earnings of black workers increased in these establishments more than in establishments that were already covered. Sexual harassment law could have similar, opposite, or no such empirically observable effects. If no wage or labor movements can be found, the effects of sexual harassment law would be in stark contrast to those of other employment discrimination protections (ADA: Jolls and Prescott, Acemoglu and Angrist; Employment Outsourcing: Autor, Donahue; Maternity mandates: Gruber). The presence of survey evidence demonstrating that job satisfaction increased after sexual harassment laws came into place (Newman, Jackson, and Baker 2003) would make the interpretation of anti-discrimination law to forbid sexual harassment - a key practical contribution of feminist legal theory - strictly Pareto-improving (Basu 2000).

This paper uses variation from the random assignment of judges to appellate panels to identify the impact of sexual harassment law on employment outcomes. Because of the minimal regional variation in sexual harassment law prior to *Meritor Savings Bank v. Vinson* (1986), we primarily identify the effects of regional law changes derived from the interpretation of anti-discrimination law by appellate courts to make it more or less difficult for individual plaintiffs to bring, prove, win, and collect damages for sexual harassment cases. Our strategy is to use the random assignment of female judges and Democratic appointees to the three-judge panels deciding these cases to identify the effects of these appellate case decisions on the employment and earnings outcomes of women relative to those of men.

In order to understand our empirical strategy, it is helpful to consider an analogy to a weighted pair of dice. Circuit judge characteristics, such as the fraction of female judges and the fraction of Democratic appointees in a circuit-year, are like the weight of a pair of dice. The actual number of sexual harassment panels with female judges or Democratic appointees is like the outcome of a dice roll. Controlling for the weight of the dice, the actual dice roll is random and is the source of identification for inferring causal effects of sexual harassment law on economy-wide outcomes.

Using this strategy, we find that sexual harassment law increases female wages relative to male wages. Moreover, female labor supply as evidenced by the number of hours worked by women increases relative to that of men. Employment of women relative to men also increases. Sexual harassment law has a negative employment impact, however, on gender inequality when the population is restricted to those already in the labor force. A particularly interesting finding is that the increased severity of sexual harassment law causes the proportion of female managers to increase relative to the proportion of male managers. These results are consistent with the theory that sexual harassment is a form of economic warfare which kept women out of particular jobs. The contrasting results on employment inequality when those not in the labor force are not included suggests that some women may be encouraged to leave the home and seek work in response to favorable work environments while other women bear the cost of lost economic rents or monitoring programs. Interestingly, sexual harassment law has a stronger effect on the hiring of female managers than does gender discrimination law, which has a stronger effect on earnings, suggesting that sexual harassment and gender discrimination are theoretically and empirically very different.

Finally, damages awarded in sexual harassment cases have a positive effect on gender inequality, but neither the damages awarded nor the number of cases awarding damages has an effect when also controlling for the number of pro-punishment sexual harassment cases. An open debate in the development of legal institutions is whether individuals obey the law because the law incentivizes or because the law has legitimacy (Tyler and Huo 2002; Hurd 2003). The classic distinction is between whether punishment in the form of economic sanctions from breaking the law is motivating actors or whether actors decide they should follow the law because they believe it is moral and just. The finding that the law, not damages, stimulates social change provides novel evidence that behavior responds to the law not because of financial incentives as measured by damages alone, but for other reasons, such as the moral legitimacy that the law provides.

The remainder of the paper is organized as follows. Section II describes a simple model illustrating the potential benefits of sexual harassment law for gender inequality. Section III describes the data sources. Section IV discusses the empirical design. Section V presents the results. Section VI concludes.

II. Theory

In this section, we briefly explicate an economic model that formally captures the view by feminist legal scholars of harassment as a form of economic warfare. Under this view, forbidding sexual harassment opens job opportunities for women. This model (Lindbeck and Snower 1988) assumes that outsiders are unable to find jobs even though they are prepared to work for less than the prevailing wages of incumbent workers (insiders). The outsiders do not underbid insiders; if they did and were to successfully become new entrants, insiders would withdraw cooperation and make the work of these entrants unpleasant. In other words, insiders would "harass" the entrants, thereby reducing the productivity of underbidders.

Firms, therefore, find it costly to substitute outsiders for insiders. These harassment and labor turnover costs create economic rents, which the insiders capture via wage setting, and as a result, involuntary unemployment arises. Outsiders are unable to find work even though they would be just as profitable to the firm as the insiders, provided they faced identical conditions of employment. The insiders' harassment activities, however, ensure that conditions are not the same for insiders and outsiders.

Applying the model in its simplest form assumes that females are outsiders and men are insiders. Under these assumptions, forbidding harassment can increase the employment, wages, and managerial opportunities of outsiders. Insider males would no longer be allowed to engage in harassment activities, thereby raising the productivity of females. Firms would be willing to hire females, so their employment, wages, and managerial role would increase. Of course, not all females are outsiders; hence, some females - those who previously obtained the insider rents - may see decreases in their employment outcomes.

This predicted differential effect of sexual harassment law on some females, and not others, is akin to that of the model discussed in Basu (2000). Under Basu's model, forbidding sexual harassment may be Pareto-improving because some women may actually be better off when the government restricts their freedom to enter into contracts by which they are compensated for harassment. The existence of such contracts can hurt women who choose not to enter into those contracts. Forbidding harassment allows wages for women in previously low-harassment jobs to converge to the wages for women in high-harassment jobs as they can now compete for previously high-harassment jobs. Wages for women in previously high-harassment jobs decrease since those women no longer need to be compensated for harassment after sexual harassment is forbidden. The interpretation of these two models is slightly different—the first asks whether insider females are capturing economic rent from the exclusion of outsider females, and the second asks whether females who tolerate harassment are being compensated for it.

Hence, the first model more directly addresses the issue of female management than does the second one. Nevertheless, regardless of the model, the theoretical ambiguity remains; gender inequality may increase or decrease.

III. Data

Our empirical analysis draws on four sources of data on sexual harassment cases - three established datasets as well as the results of our own data collection. The first dataset is the Chicago Judges Project (CJP) Data (Sunstein, Schkade, and Ellman 2006). The CJP data consists of all federal appellate sexual harassment cases between 1995 and 2002, totaling 461 cases.[4] This data set is limited to published opinions. If the plaintiff was afforded any relief, then the vote was coded as a pro-plaintiff vote. The CJP data also separately tabulates all gender discrimination cases during the same time period.[5] The second dataset is from Boyd, Epstein, and Martin (2007), which we use to perform a randomization check. This data set codes case characteristics, such as the presence of common facts and legal issues, for a subset of Title VII discrimination claims in the CJP data. The third dataset is from Sharkey (2006) and covers a comprehensive set of 232 cases in which plaintiffs won some positive amount of compensatory damages from state and federal, trial and appellate court decisions from 1982-2004.[6] We restrict this data to the 90 federal appellate cases between 1982 and 2002. Our fourth dataset is

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[4] The CJP data was derived from searching Lexis for "sex! harassment." The sample includes cases from 01/01/1995 to 12/31/2002. We learned via communication with a co-author of the paper that the CJP data further restricts to cases substantively about sexual harassment and cases where substantive decisions regarding sexual harassment were made, rather than cases decided on procedural grounds.

[5] Gender discrimination cases were derived from searching Lexis for "sex! discrimination." Sexual harassment cases are removed from this sample.

[6] The data comes from the Westlaw search: "DA(AFTER 2/12/1998) & JURY & AWARD! & (SEXUAL/5 HARASS!) & (EMPLOYEE "TITLE VII")" on May 20, 2004. Sharkey further restricts to (1) cases between plaintiff employees and defendant employers/supervisors/co-employees (i.e., excluding cases brought by the EEOC on behalf of employees); (2) cases raising at least one claim of sexual harassment under either Title VII or state civil rights laws; (3) cases involving trial by jury; and (4) cases in which the jury awarded some positive amount of damages on the basis of sexual harassment.

composed of our own collection of cases from 1982 to 2002 of all sexual harassment cases brought in an employment context; we thereby extended the CJP dataset backwards.[7] However, we were unable to replicate the CJP method of collection for 1995-2002. Consequently, in our 1982-2002 analyses, we use our own data collection method, which resulted in 230 cases.[8] When, however, we compare sexual harassment and gender discrimination law, we use the CJP data in order to make sure the cases being compared were selected under the same method.

We use the Merged Outgoing Rotation Groups (MORG) of the Current Population Survey (CPS) for information on individual employment outcomes, including weekly earnings, amount of time worked, and employment status. The MORG provides point-in-time measures of the variables of interest (Autor, Katz, and Kearney 2005), including age, sex, race, marital status, educational attainment, and the geographic location of the individual (matching the state of residence to the circuit having legal jurisdiction). We restrict to individuals between the ages of 18 and 65.

We do not need data on actual harassment since we are examining the effect of sexual harassment law on employment outcomes and not on sexual harassment itself. We also do not need data on state laws because they use boilerplate anti-discrimination clauses that are interpreted by courts to include sexual harassment (only two state statutes explicitly mention sexual harassment).

Since we need to control for several factors at the circuit-year level in our specifications, we collect information on federal appellate courts from several sources. In order to obtain the fractions of female judges and Democratic appointees, we gather information from the Federal

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[7] We have run analyses that combined CJP data for years after 1995 with an expanded Sharkey dataset for years prior to 1995. The results are similar and available upon request.
[8] The data comes from the Westlaw search: "DA(BEFORE 1/1/2003 & AFTER 1981) & JURY & AWARD! & (SEXUAL /S HARASS!) & (EMPLOYEE "TITLE VII")." The data was further manually restricted to cases brought by employees for sexual harassment in the workplace.

Judicial Center.[9] We also obtain a measure of annual circuit workload - the number of federal appeals terminated by fiscal year - from Federal Court Management Statistics.[10]

IV. Empirical Design

Our identification strategy begins by exploiting variation across circuits and over time in the degree to which the law punishes harassment. Our basic specification, therefore, examines the effect of punishment for harassment at the circuit-year level on individual employment outcomes. Since we are interested in whether sexual harassment law affected men and women differently, our main specification of interest includes an interaction between the degree to which a circuit punishes sexual harassment in a given year (Pun_harass_{ct}) and the sex of individual *i*.

We measure Pun_harass_{ct} in two different ways for the bulk of our analysis. First, we codify Pun_harass_{ct} as the number of three-judge panels (cases) that result in a pro-plaintiff (pro-punishment) outcome ($Numpropun_{ct}$). Second, we measure Pun_harass_{ct} as the number of pro-punishment panels normalized by the number of non-sexual harassment appeals (DS_{ct}), which is an indicator of docket size. We use this normalization to take into account differences across circuits and over time in factors driving the overall quantity of litigation. An alternative measure would be the number of sexual harassment cases, but such a measure may significantly weaken the power of the instruments since the number of sexual harassment cases is likely to respond to previous plaintiff win rates attributable to the random assignment of females or Democratic appointees in prior panels.

Despite the random assignment of judges to any one case, however, the availability of the type of judges to be assigned may differ across time and space for reasons that are correlated

[9] See http://www.fjc.gov/history/home.nsf.
[10] See http://www.uscourts.gov/fcmstat/index.html.

with case availability and with employment outcomes. To address this problem and to isolate the variation attributable to random assignment, we control for characteristics of the judicial pool, such as the fraction of women and Democratic appointees. We must control for the fraction of female judges and the fraction of judges appointed by a Democratic president in a given circuit-year since unobservable characteristics of a circuit-year, such as its progressiveness with respect to women's issues, could affect both the quantity and outcomes of sexual harassment litigation and employment outcomes for women. Controlling for characteristics of the judicial pool mitigates this bias. Note that since workers are mobile, earnings and employment have a tendency to converge across circuits. Consequently, our identification strategy provides a lower bound on the true effect of sexual harassment law in the absence of such convergence. The basic OLS specification is:

$$\begin{aligned} &LogR\_Earnings_{ict} = \beta_0 + \beta_1 X_{ict} + \beta_2 C_c + \beta_3 T_t + \beta_4 W_{ct} + \beta_5 W_{ct} * Sex_{ict} \\ (1)\quad &+ \beta_6 Pun\_harass_{ct} + \beta_7 Pun\_harass_{ct} * Sex_{ict} + \varepsilon^0_{ict} \end{aligned}$$

where $LogR\_Earnings_{itc}$ is the log of the weekly real earnings of individual i in circuit c and year t.[11] X_{ict} is a vector of observable characteristics, including sex, age, race, marital status, and educational attainment (with the exception of age, these variables enter as dummies). C_c is a vector of circuit fixed effects. T_t is a vector of year fixed effects. W_{ct} is a vector of circuit by year controls, including the fraction of female judges and the fraction of Democratic appointees. If Pun_harass_{ct} is measured by $Numpropun_{ct}$, W_{ct} also includes the number of non-sexual harassment appeals in a given circuit-year.

[11] We use weekly earnings as the outcome measure since annual earnings captures two margins – wages and weeks worked.

The inclusion of an interaction term allows for the estimation of differential effects of the law on women. We acknowledge the fact that courts follow precedent from other circuit-years so ε_{ict} is not *i.i.d.* Hence*,* all specifications are clustered at the circuit-year level. Specification 1 is altered to examine the effects of sexual harassment law for different employment outcomes. In addition to earnings, we perform the same regression for hours worked, employment status, and management status.[12] We also vary our measure of Pun_harass_{ct} to reflect the number of cases awarding damages, the sum of damage awards at the circuit-year level, and significant doctrinal changes in the law.

A. *Temporality*

We modify the basic specification above in a number of ways in order to explore various hypotheses and perform a number of robustness checks. The first of these modifications is to take into account temporal considerations. It is not obvious ex ante how quickly appellate decisions are absorbed into the actions of employers and employees. If the appellate decision is simply affirming a lower court decision, the effect of the current decision may be a proxy for the effect of the lower court decision which, on average, would have been decided a year to a year and a half earlier. At the same time, appellate decisions should have a greater effect than district court opinions because of both their geographic jurisdiction and precedential importance (they

[12] We use hours last week instead of usual weekly hours because usual weekly hours are not consistently available. As a result of the CPS redesign in 1994, workers who report that their weekly hours vary are not asked to report usual weekly hours, yielding a non-report rate of 7.0 to 8.5 percent of workers in 1994 to 2003 (Autor, Katz, and Kearney 2005). In the March dataset, the number of hours worked last week is coded as zero for individuals who are not in the labor force and individuals who are unemployed. The number of hours worked last week for the same demographic group is coded as missing in the MORG dataset. To adjust for this inconsistency, we recoded the number of hours worked for individuals who are either not in the labor force or unemployed as zero in the MORG dataset. We also recode earnings as zero for individuals who are not in the labor force or unemployed. Earnings are adjusted to be in 2000 real terms. We do not recode management status, which is constructed from the occupation variable, because occupation is available for about 90% of the unemployed and 33% of those not in the labor force, about 10% of which are managerial.

are much more difficult to overturn than district court opinions). Thus, we also lag Pun_harass_{ct}, thereby allowing for the effects of past decisions on employment outcomes:

$$\begin{aligned}
&LogR_Earnings_{ict} = \lambda_0 + \lambda_1 X_{ict} + \lambda_2 C_c + \lambda_3 T_t + \lambda_4 W_{ct} + \lambda_5 W_{ct} * Sex_{ict} \\
&+ \lambda_6 Pun_harass_{ct} + \lambda_7 Pun_harass_{c(t-1)} + \lambda_8 Pun_harass_{c(t-2)} \\
&+ \lambda_9 Pun_harass_{ct} * Sex_{ict} + \lambda_{10} Pun_harass_{c(t-1)} * Sex_{ict} + \lambda_{11} Pun_harass_{c(t-2)} * Sex_{ict} \\
&+ \varepsilon^1{}_{ict}
\end{aligned} \tag{2}$$

B. *Identification Strategy*

Using our two distinct measures for Pun_harass_{ct} and its lags, as well as controlling for judge pool characteristics at the circuit-year level, will likely mitigate the problem that any effect we observe is a spurious correlation. We still must be concerned, however, about whether in equation (1), the coefficients β_6 and β_7 are biased because of omitted characteristics at the circuit-year level that vary systematically with the degree to which a particular circuit-year punishes harassment. For example, judges may be influenced by socioeconomic factors, and the relationship between the environment in a particular circuit-year and judicial decisions could bias estimates of the effects of judicial decisions on economic outcomes. To address this potential bias, we exploit the randomization of judge assignments to appellate panels together with the extent to which judge propensities to vote pro-plaintiff in sexual harassment cases can be predicted by the judge's gender and the political party of the president who appointed the judge. Note that random assignment alone is not sufficient to address the bias we describe. Random assignment ensures that judges are not selecting the cases on which they wish to have an impact; it prevents the problem that would arise where the judge's choice is correlated with omitted factors that bias our estimates. Instead, our identification of the effect of judicial decisions on the environment derives from the variation in judicial decisions that results from the random assignment of appellate judges with certain characteristics. These characteristics, whether judges are females or Democratic appointees, are not characteristics that would be influenced by the socioeconomic environment. Accordingly, reverse causality - where the socioeconomic environment drives judicial decisions - is unlikely to plague our estimates of the causal effects of judicial decisions on the socioeconomic environment.

A number of papers have documented the effect female judges have on sexual harassment cases, the most careful of which cluster standard errors at the panel level to account for interaction among judges in a panel (Farhang and Wawro 2004), use propensity score methods (Epstein 2007; see Epstein [2007] for a more general critique of the literature on gendered judging), or include unpublished opinions (Peresie 2005). Peresie (2005) finds that for sexual harassment cases, having a female judge increased the probability of a pro-plaintiff decision by 86% (from 22% to 41%). This difference was significant at the 5% level.

Similar effects are found for judges appointed by Democratic presidents. Peresie (2005) finds a positive and significant coefficient on Democratic Party affiliation even after controlling for the gender of the judge. Sunstein, Schkade, and Ellman (2003) also find a positive and significant coefficient on being associated with the Democratic Party for judges making pro-plaintiff decisions in sexual harassment cases. They find that Republican appointees vote for plaintiffs at a rate of 37% compared with Democratic appointees, who vote for plaintiffs at a rate of 52%.

The instruments we use are motivated by findings at the judge level (Peresie, Epstein, etc). We replicate some of these findings in our dataset, from 1982 to 2002. A regression of the judge's decision on gender and party of the judge, clustering standard errors at the case level, shows that Democratic Party affiliation predicts the judge’s votes positively and significantly, but gender does not. The same regression shows that both being female and affiliated with the Democratic Party significantly predict the judge's votes in the Peresie data.[13]

Despite these findings at the judge level, the question still arises as to how significantly both gender and party affiliation will predict outcomes once we aggregate judges to the case level and when we aggregate cases to the circuit-year level. At the case level, in a regression of the outcome on a dummy indicating whether a woman is on the panel and circuit-fixed effects, female presence strongly predicts plaintiff wins in the Peresie dataset. The same does not hold for Democratic Party affiliation. In the CJP data, at the case level, a regression of case decision on whether a female is present and circuit-fixed effects indicates that female presence does not strongly predict the case decision whereas the presence of a Democratic appointee does. Thus, even before aggregating to the circuit-year level, we have reason to believe that party affiliation will be a stronger instrument when using the CJP data.

We aggregate these findings to the circuit level by creating instruments based on the number of three-judge panels containing at least one female judge or at least one Democratic appointee. Accordingly, the instruments (J_{ct}) for Pun_harass_{ct}, its lags, and their interactions with a dummy equal to one if an individual is female in equation (2) are:

$$\text{(3)} \qquad \begin{array}{ll} \{Pan\_Fem_{ct}, Pan\_Dem_{ct}\} \in J_{ct} & \text{if } Pun\_harass_{ct} = Numpropun_{ct} \\ \{Pan\_Fem\_DS_{ct}, Pan\_Dem\_DS_{ct}\} \in J_{ct} & \text{if } Pun\_harass_{ct} = Numpropun_{ct}/DS_{ct} \end{array}$$

Pan_Fem_{ct} is simply the number of sexual harassment panels in a given circuit and year that contained at least one female judge. This instrument should predict the number of pro-plaintiff sexual harassment decisions in that same circuit and year. Moreover, since appellate judges are randomly assigned to panels and we control for the fraction of female judges and docket size, it

[13] This regression was performed using Peresie's data, available from the Yale Law Journal's website.

should not be correlated with employment outcomes except through its effect on the measure of Pun_harass_{ct}. Pan_Dem_{ct} is a similar instrument constructed for judges appointed by Democratic presidents. $Pan\_Fem\_DS_{ct}$ is an instrument constructed by dividing Pan_Fem_{ct} by docket size. $Pan\_Dem\_DS_{ct}$ is an instrument constructed by dividing Pan_Dem_{ct} by docket size. Docket size is measured as the number of non-sexual harassment appeals terminated in a circuit-year.

Formally, the two first-stage regressions in the two-stage least squares procedure are:[14]

$$Pun\_harass_{ct} = \varphi_0 + \varphi_1 X_{ict} + \varphi_2 C_c + \varphi_3 T_t + \varphi_4 W_{ct} + \varphi_5 W_{ct} * Sex_{ict} + \varphi_6 J_{ct} + \varphi_7 J_{ct} * Sex_{ict} + \varepsilon^2_{ict} \tag{4a}$$

$$Pun\_harass_{ct} * Sex_{ict} = \gamma_0 + \gamma_1 X_{ict} + \gamma_2 C_c + \gamma_3 T_t + \gamma_4 W_{ct} + \gamma_5 W_{ct} * Sex_{ict} + \gamma_6 J_{ct} + \gamma_7 J_{ct} * Sex_{ict} + \varepsilon^3_{ict} \tag{4b}$$

In practice, the instrumental variables regressions include lags of Pun_harass_{ct}, J_{ct}, W_{ct}, and the interactions of these lags with sex.

C. *Randomization Checks*

Typically, a useful robustness check of a randomization strategy would be to see if future policy decisions affect past outcomes by running a reduced form specification regressing outcome variables on forwards of the instruments and controls. In our case, a mechanical correlation between current and future instruments may arise because the current number of panels with female or Democratic appointee judges may be positively correlated with the future

[14] Our two-stage least squares estimates use random assignment of judges' propensity to vote pro-plaintiff as indicated by having a woman or Democratic appointee on the panel, which is *i.i.d.* conditional on characteristics of the judicial pool, such as the fraction of women and Democratic appointees. As with the OLS regression, the two-stage least squares estimates are clustered at the circuit year level.

number of sexual harassment cases, which in turn, is positively correlated with the future number of panels with female or Democratic appointee judges. The number of sexual harassment cases is likely to respond to previous plaintiff win rates attributable to the random assignment of females or Democratic appointees in prior panels. Thus, a reduced form regression of the outcome variables on forwards of the instruments is a less informative test of randomization.

As an alternative randomization and identification check, we use data from Boyd, Epstein, and Martin (2007), which codes some case characteristics for a subset of the gender discrimination cases in the Chicago Judges Projects data. We regress case characteristics on whether there is a female (Democratic appointee) on the panel controlling for the fraction of women (Democratic appointees) in the judicial pool and circuit and year-fixed effects and find that most characteristics are not correlated with the gender or party of the judge. Appendix Table B shows that of 19 case characteristics, two are correlated with having a female on the panel and none are correlated with having a Democratic appointee on the panel. For the additional data we coded from 1982 to 1995, we also noted whether the plaintiff was the victim of sexual harassment and which party appealed. The last two rows of Appendix Table B show that these case characteristics are not correlated with whether there is a female on the panel or whether there is a Democratic appointee on the panel controlling for circuit and year-fixed effects and characteristics of the judicial pool.[15]

V. Results

Before summarizing our findings, we demonstrate, as a threshold matter, the validity of our empirical strategy. Table 1 shows the results from basic first-stage specifications. The table

[15] For more information about random assignment of cases at the appellate level, see Brown, Jr. and Lee (2000), in particular, http://law.du.edu/images/uploads/neutral-assignment/Neutral_assignment_links.pdf.

shows the coefficients and standard errors on the instruments when the endogenous variable, *numpropun* or *numpropun* normalized by docket size, is regressed on each instrument individually and the two together. In all cases, we can see that each instrument individually is a positive and significant predictor of the quantity of pro-plaintiff sexual harassment cases. The coefficients in columns 1 and 2 indicate that each additional panel with a female judge leads to 0.712 additional pro-punishment decisions and each additional panel with a Democratic appointee judge leads to 0.793 additional pro-punishment decisions. In other words, out of 100 sexual harassment panels with at least 1 female judge present, about 71 of them would rule pro-punishment, and out of 100 sexual harassment panels with at least one Democratic appointee present, about 79 of them would rule pro-punishment. The effects are slightly larger in the specifications with the docket size normalization with coefficients of 0.853 and 0.807 for female judges and Democratic appointees respectively in columns 4 and 5. Although the panels with female judges instrument becomes negative when it is included along with the number of panels with Democratic appointees instrument, it is not significant. Because of the high percentage of females who are Democratic appointees - as high as 60% to 70% - this result probably indicates that male Democratic appointees are more likely to be pro-plaintiff than are female Republican appointees. Despite the negative effect of the panels with female judges instrument found when both instruments are used, the fact that in most of the specifications the instruments are positive and significant predictors of the endogenous variable implies that the instruments are sufficiently strong to obtain unbiased two-stage least squares estimates of the effects of the quantity of pro-plaintiff cases on the employment outcomes of women relative to men. The F-statistic tests of joint significance are 70.68 and 91.05 for the non-normalized and normalized specifications respectively.

A. *Earnings*

Table 2 shows the results from OLS and IV specifications examining the effects of the quantity of pro-plaintiff cases on log earnings. The different columns of the tables indicate whether the specification includes neither, either, or both instruments. IV with female judges contains either the instrument equal to the number of panels with female judges or the ratio of the number of panels with female judges to docket size in a given circuit and year. IV with Democratic appointees contains an instrument equal to either the number of panels with Democratic appointees or the ratio of the number of panels with Democratic appointees to docket size. IV with both contains a pair of instruments, either the numbers of panels or the ratios of these numbers to docket size. The coefficients represent the effect of a unit increase in the endogenous variable on individual earnings. These regressions control for individual characteristics, circuit and year-fixed effects, the fraction of female judges, the fraction of Democratic appointees, the interaction of the fraction terms with the female dummy, and - when the endogenous variable is not a ratio - docket size. They are also clustered at the circuit-year level. The tables show OLS and IV estimates of the coefficients on the endogenous variable (the number of pro-punishment cases and the ratio of the number of pro-punishment cases to docket size) and its lags interacted with the female dummy. In the upper half of the table, in Panels 1 and 2, the lag specifications are run in separate regressions while in the lower half of the table, in Panels 3 and 4, the lags are run together in a single regression with the F-statistic for joint significance displayed below. The number of observations and the R-square for the IV specification with both instruments are displayed to the right of column 4. Columns 1-4 in the left half of the table include individuals not in the labor force; columns 5-8 in the right half of the table exclude individuals not in the labor force.

We see in Table 2 that the quantity of pro-plaintiff cases has a positive effect on current female earnings and the effect does not exist for the sample excluding those not in the labor force. The coefficient of 0.101 in the final IV specification in Column 4 indicates that an additional pro-punishment decision increases the current earnings of women relative to men by 10.1%. This result is significant at the 1% level. The coefficient of 330.3 in the final IV specification of Panel 2 indicates that an additional pro-punishment decision relative to an average docket size of 3,612 (Appendix Table A) increases the current earnings of women relative to men by 9.1%. This result is significant at the 1% level. The average number of pro-punishment decisions per circuit year is 0.599 (Appendix Table A). In the lag specifications, the positive effect generally shows up in each year and the coefficients are individually statistically significant, though largest in magnitude (Panels 1 and 2) and significance (Panels 1-4) for the current year. However, when the sample is restricted to those previously in the labor force, the positive effect on earnings disappears (Columns 5-8).

These results also highlight the remarkable consistency of estimates across specifications with 0, 1, or 2 instruments, thus serving as an over-identification check of our empirical strategy. The variation in gender inequality, whether due to the deviation from expected assignment of female judges, deviation from expected assignment of Democratic appointees, or from both, all give similar results. Moreover, the OLS estimates are similar to the IV results, suggesting that sexual harassment case law, controlling for judicial pool characteristics, such as the fractions of female and Democratic appointee judges and docket size, may be exogenous to gender inequality. The IV estimates are roughly comparable to the OLS estimates in magnitude, indicating that the instruments are not weak.

B. *Quantity of Labor Supply – Hours Worked*

Table 3 is organized analogously to Table 2.[16] It contains hours worked as the outcome of interest. We see positive significant effects of sexual harassment law on the number of hours worked last week. When using the March CPS, a smaller dataset that also measures number of weeks worked, the analysis using weeks worked last year gives similarly positive effects (not shown). In Column 4, the coefficient of 0.491 hours per week out of an average of 22.62 hours per week (Appendix Table A) translates to roughly 3.2 8-hour workdays for women relative to men. The coefficient of 1576.9 in the specification where *numpropun* is normalized by docket size in Column 4 of Panel 2 means that each additional pro-punishment case out of an average of a docket size of 3,612 (Appendix Table A) translates to 0.437 additional hours per week, or roughly 2.8 additional 8-hour workdays for women relative to men. Both estimates are statistically significant at the 1% level.

The lag specifications also display statistical significance at the 1% level when each lag is run by itself but the joint specification sometimes displays statistical significance at the 10% level or 5% level for the current year and is weaker for the lags. The lag coefficients are, however, jointly significant even when individually they are not (see Panels 3 and 4.) When the sample is restricted to those previously in the labor force, sexual harassment law has a negative effect, raising gender inequality by 0.131 hours per week (Column 8 of Panel 1) or 451.4/3612 hours per week (Panel 2), amounting to 0.8-0.9 8-hour workdays for women relative to men per year.

[16] Although we do not show results for more than three lags, we do examine up to seven lags. The results are broadly similar; the coefficients are jointly significant, and the more recent years have stronger effects.

C. *Employment Status*

While Part B above examined the effect of sexual harassment law on the intensive margin of employment, this section discusses the results from linear probability models examining the effects of sexual harassment law on the extensive margin - employment status. In Table 4, the results indicate a positive and significant effect on the margin between non-employment and any employment. The coefficient of 0.0165 in Column 4 of Panel 1 indicates that each pro-punishment decision leads to an increase of 1.7 percentage points in the probability a female has any employment relative to the probability a male has any employment.[17] The coefficient of 54.92 in Column 4 of Panel 2 indicates that each pro-punishment decision out of an average docket size of 3,612 leads to an increase of 1.5 percentage points in the probability a woman is fully or partially employed relative to the probability a man is fully or partially employed. The results are significant at the 1% level.

The specifications with lags also display statistical significance. They indicate a stronger effect in the most recent year and weaker effects for earlier years. For instance, Column 4 of Panel 3 indicates that an additional pro-punishment case in the current year results in an increase of 0.8 percentage points in the probability a female has any employment relative to the probability a male has any employment. An additional pro-punishment case in the previous year results in an increase of 0.5 percentage points. An additional pro-punishment case two years prior results in an increase of 0.7 percentage points. The effects are statistically significant at the 1% level or 5% level. Again, when restricting to those previously in the labor force, sexual harassment law has a negative effect on gender inequality, increasing the probability a female is unemployed by 0.1% relative to men in Column 8 of Panel 1. This effect is statistically

[17] A probit regression indicates that each pro-punishment decision leads to an increase of 1.0 percentage points in the probability a female is employed relative to the probability that a male is employed.

significant at the 1% level.

D. *Management Status*

In order to further illuminate our findings regarding the labor supply and employment status of female workers, we explore whether sexual harassment law differentially impacted the probability of a female becoming a manager. Firms could have an incentive to promote females as a way of decreasing problems with sexual harassment in the workplace. Historically, sexual harassment has been conducted by male managers more frequently than by female managers. Having a greater female-to-male ratio of managers may therefore reduce the occurrence of sexual harassment in the office. Hiring female managers may also be a cheaper way to mitigate the risk of litigation than to implement highly costly training programs. Females may also be more willing to be managers in a workplace that disallows sexual harassment. Those higher quality females more likely to be promoted as managers may be more attracted to a workplace without sexual harassment.

We explore these hypotheses by utilizing linear probability models of the form used for employment status where the outcome variable is an indicator for whether an individual is a manager. An individual is defined to be a manager if he/she is classified as employed under the categories of "Administrators and Officials, Public Administration," "Other Executive, Administrative, & Manager," or "Management Related Occupations." When we restrict to the first category of managers or when we use the Execucomp dataset on the top five executive officers in companies included in the S&P 500, S&P 400 MidCap, and S&P SmallCap 600 indexes, available from 1992 forward, we do not find an effect (not shown), suggesting that sexual harassment law had a strong effect on the hiring of mid-level managers but not on the hiring of the highest levels of management. Table 5 shows these results from the CPS. The coefficient in Column 4 of Panel 1 indicates that an additional pro-punishment case causes females to be 0.5% more likely to be a manager relative to the probability of a male being a manager.[18] Coefficients in the IV regressions are statistically significant at the 1% level and are remarkably consistent across specifications, corroborating the robustness of the identification strategy. The final column of the docket size normalization specification indicates that a unit increase in *numpropun* causes the probability of a female being a manager to increase 0.4% relative to the probability of a male being a manager.

Using the information on the average number of pro-punishment cases per circuit-year, we graphically show what the probabilities for females and males being managers would have been had there been no sexual harassment law during the span of our data's time period, 1982-2002. The effect of sexual harassment law appears to be quite significant in terms of reducing the gender inequality gap, especially in the later years when there were more pro-punishment sexual harassment cases. Figure 1 is based on the results of the specification with both instruments (Column 4) in Table 5. The outside lines indicate the counterfactual male and female management trends while the inner two lines indicate the actual male and female management trends. By 2002, the management probabilities have converged but in the absence of sexual harassment law, management inequality would be approximately what it was five years earlier (Figure 1A). Notice that sexual harassment law appears to have had a much larger effect on management inequality as a fraction of overall management inequality than it did on employment or earnings inequality as a fraction of overall employment or earnings inequality

[18] A probit regression indicates an additional pro-punishment case causes females to be 0.4% more likely to be a manager relative to the probability of a male being a manager.

(Figure 1B-1D).[19]

Figure 1 shows the effect that sexual harassment law had on management each year, but assumes that for each year, sexual harassment law in previous years did not have any effect on management in that year. Although this assumption may not be reasonable because of the possibility of persistence in the data, a regression of the *changes* in probabilities of management for males and females on the number of pro-punishment cases reveals the coefficient on *numpropun* to be statistically insignificant at the 5% level. In other words, while the number of pro-punishment cases within a circuit-year does have a significant effect on the probability of a given male or female employee being a manager, it does not have a significant effect on the *change* in probability that a given male or female employee is a manager. Consequently, we are able to rule out the possibility of persistence of the impact of the law. The specifications with lags jointly included (Panels 3 and 4) show that the strongest effects are found for law in the contemporaneous year. When the sample is restricted to those previously in the work force, the effect of sexual harassment law is smaller but almost unchanged.

Figures 2-5 show a scatter plot of measures of employment inequality on the y-axis and the number of pro-punishment cases on the x-axis. We construct the inequality measures by computing what the employment outcome - management, any employment, hours, and earnings - would be if the gender were switched to female. In other words, we match men and women on the basis of their individual characteristics. The average difference in the outcomes between the men and women who are matched is deemed employment inequality. Econometrically, we run a regression of the outcome variable on observed characteristics (listed after equation 1) fully interacted with a dummy for being female. The

[19] One possible explanation for why the literature has found a slowdown in the convergence of gender inequality as measured by employment and earnings is that convergence of gender inequality has continued apace at the management level.

coefficients on the interaction terms tell us how each covariate, such as education, race, or year, contributes to gender inequality were the individual to have that characteristic. Then the coefficients on the interaction terms are multiplied by the non-interacted actual covariates for every male and female to compute an individualized measure of employment inequality. The methodology and structural equations are explained in Chen (2004). This inequality measure is then averaged to the circuit year level and plotted against the number of pro-punishment cases as shown in Figures 2-5. The line of best fit and confidence interval are also displayed. In the case of management, earnings, and employment status, we can visually see a positive and statistically significant effect of sexual harassment law even with the data collapsed to the circuit-year level. For the hours worked outcome, it is positive but not statistically significant. In sum, more pro-punishment cases lead to better female outcomes relative to male outcomes.

E. *Gender Discrimination vs. Sexual Harassment Law*

In the previous section, we explored the impact of sexual harassment law on management. In this section, we explore the possibility that the results may have been partially the result of overarching gender discrimination law rather than specifically sexual harassment law. Although historically sexual harassment has been categorized as gender discrimination, the act of sexual harassment is inherently different from that of gender discrimination. We therefore investigate the effects of gender discrimination law on outcomes and whether the effect of sexual harassment law is still significant when controlling for gender discrimination law. If the effect of pro-punishment cases for gender discrimination is large and statistically significant, and the effect of sexual harassment law on management is not significant, then the results we have attributed to pro-punishment sexual harassment cases may actually result from gender discrimination law.

In order to test this hypothesis, we use a similar methodology to the sexual harassment analysis, with the exception that we use new predictor and instrumental variables that correspond to gender discrimination appellate court cases rather than sexual harassment appellate court cases. Specifically, we use the predictor variable, *numpropunish_gd* in place of *numpropunish* to represent the number of pro-punishment cases for gender discrimination appellate court cases, and we use the instrumental variables, *panelswithwomen_gd* and *panelswithdem_gd* to represent the number of panels with at least one female and one Democrat for gender discrimination appellate court cases. The data for gender discrimination court cases is compiled from the Chicago Judges Project for the years 1995-2002.

Before running two-stage least squares regressions to investigate the effect of gender discrimination law on management and other outcomes, we first test to see whether the first-stage regressions for the instrumental variables, *panelswithfem_gd* and *panelswithdem_gd,* are strong. Table 6 indicates that the first-stage regressions are strong for gender discrimination. We find that each instrument is individually a positive and significant predictor of the quantity of pro-plaintiff gender discrimination cases, and when both instruments are used, the F-statistic testing for the significance of both coefficients is 24.18. The coefficients in columns 1 and 2 indicate that each additional panel with a female judge leads to 0.450 additional pro-punishment decisions and each additional panel with a Democratic appointee judge leads to 0.413 additional pro-punishment decisions. In other words, out of 100 gender discrimination panels with at least 1 female judge present, about 45 of them would rule pro-punishment, and out of 100 gender discrimination panels with at least one Democratic appointee present, about 41 of them would rule pro-punishment. All of the coefficients on *numpropun_gd* or *numpropun_gd* normalized by docket size prove to be statistically significant at the 1% level. We also run a robustness check of the instrumental variables by regressing the endogenous variables on the incorrect, but analogous instruments corresponding to the sexual harassment law cases. As expected, both instruments fail to be statistically significant at the 5% level; the F-statistic of joint significance is 2.36. This finding is expected since the identity of judges assigned to gender discrimination cases should be uncorrelated with the identity of judges assigned to sexual harassment cases, because both sets of cases receive their own random assignments. Based on our first-stage findings, we conclude that the instrumental variables are sufficiently strong to obtain two-stage least squares estimates of the effects of the quantity of pro-plaintiff gender discrimination cases on the probability of management for female employees relative to that of male employees and other measures of gender inequality.

Since our data on gender discrimination cases only includes cases from 1995-2002, we restrict our analysis in Table 7 to only those years. Columns 1, 4, 7, and 10 in this table show the results from OLS and IV specifications examining the effects of both the quantity of pro-plaintiff sexual harassment cases and the quantity of pro-plaintiff gender discrimination cases, controlling for the effects of the other type of law. Columns 2, 5, 8, and 11 show the effects of pro-plaintiff sexual harassment court cases alone to allow for the comparison of the effects of sexual harassment law alone versus the effects of sexual harassment law while controlling for gender discrimination law. When the effect of gender discrimination is run alone, the impact is much weaker than the effect of sexual harassment on managerial status (Columns 3 and 6). When both gender discrimination law and sexual harassment law are included, the impact of gender discrimination law is not significant while the impact of sexual harassment law is. The estimates of the effect of sexual harassment law are remarkably unaffected when comparing specifications with and without gender discrimination law. The magnitudes and statistical significance are virtually the same in the case of management. The magnitudes are slightly smaller than those in Table 5, which may be because of the different time frame being analyzed; this is suggestive evidence that sexual harassment law had a larger effect in earlier years than in later years. This is strikingly exhibited in the case of hours worked for which there is an effect when using the 1982-2002 time frame, but no effect when analyzed over the 1995-2002 time frame.

In summary, the management results are robust to controlling for changes in anti-discrimination law. Gender discrimination law appears to have a stronger effect on earnings than sexual harassment law (Columns 7 and 10), but sexual harassment law has a stronger effect on management (Columns 1 and 4). This is the first study, to our knowledge, finding that the effects of sexual harassment are empirically very different from those of gender discrimination, a distinction that has not been noted in the economic literature. Second, these results open up an interesting avenue of research as to why gender discrimination law is less effective with respect to managerial inequality as compared to sexual harassment law. Are firms less threatened by the prospect of being sued for gender discrimination than they are for sexual harassment? Is hiring female management more effective at reducing sexual harassment litigation than at reducing gender discrimination litigation? Third, our findings evidence the power of interpretation, that feminist legal theorists interpreting existing law had practical ramifications that did not exist prior to the introduction of the interpretation.

F. *Costs to Insiders*

Thus far, we have shown positive effects of sexual harassment law on female wages, employment, and management. Theory suggests, however, that there may have been some negative effects of sexual harassment law as well because of the cost of implementing sexual harassment programs. Moreover, the cost of sexual harassment law may have been borne disproportionately by insider females, according to either the insider-outsider theory of harassment and involuntary unemployment or the compensating wage differentials theory of harassment. We now turn to some evidence of these costs to insiders.

Sub-sections B and C above examine the impact of sexual harassment law on employment for the entire sample of workers, including those not in the labor force (see results in Columns 1-4 of Tables 2-5. When the analysis is restricted to those in the labor force (i.e. “insiders”), the picture changes (see Columns 5-8 of Tables 2-5). To review the results, we see a negative effect of sexual harassment law on hours worked in Table 3, a negative effect on the probability of any employment in Table 4, a much reduced positive effect on earnings in Table 2, and a smaller but basically unchanged positive effect on management in Table 5.

As further evidence of the incidence of sexual harassment law, using the March CPS dataset (a smaller dataset than the MORG CPS that contains additional variables such as firm size), we examine the differential impact of sexual harassment law for large and small firms.[20] Large firms are defined as those with over one thousand employees. For large firms, sexual harassment law decreases the hours worked of females relative to males, but for small firms, sexual harassment law has a positive and statistically significant effect. The large and small firm effects are statistically significantly different from each other. In general, the effects of sexual harassment law on gender inequality are more favorable towards women in small firms than in large firms. These results suggest females working at large firms may be bearing the cost of the programs described by Dobbin (2007) and others, while females working at small firms may

[20] As with the MORG CPS, we restrict to people ages 18 to 65. People who are not in the universe are coded as missing. We address top-coding of hours and earnings by multiplying the largest value by 1.5 as is standard in the literature.

enjoy the benefit.

G. *Damages*

We now turn to the effect of sexual harassment damages on gender inequality. Damages awarded are the jury total damages if final or if adjusted final total damage values do not exist in the opinion. Damages are considered 0 if reversed on appeal.[21] Damages are summed at the circuit-year level.[22] If no case with damages occurred in a circuit year, it is coded as 0.[23] In Appendix Table C, the first stage relationship between the number of sexual harassment damages cases with female judges and Democratic appointees shows the same pattern as in Table 1. When run together, the number of damages cases with Democratic appointees has a strong positive effect on the average damages awarded, the number of cases with any damages, and the number of cases where the judges voted to keep or increase damages. The number of cases with female judges generally does not have an effect, but when the specification with female judges is run separately (not shown), the effect is positive and significant as in Table 1. The F-statistic of joint significance is quite large, ranging from 15.62 to 101.01.[24] The coefficient in column 1 indicates that each additional panel with a Democratic appointee judge leads to $13,880 in additional damages awarded.

The effects of sexual harassment damages are examined in Table D. Across all measures, damages awarded have a positive effect on female outcomes relative to male outcomes. OLS specifications are displayed in the odd columns and IV specifications using both instruments are

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[21] Damages are considered reversed only if the decision is completely reversed.
[22] The sum of damages captures the probability that a sexual harasser is caught as well as the damages he pays, ceteris paribus.
[23] Coding as 0 makes this analysis consistent with the analysis using the number of pro-plaintiff cases. If there are no sexual harassment cases in a circuit year, the number of pro-plaintiff cases is also coded as 0. Damages are adjusted to be in 2000 real values. Log damages, where shown, are log of (1+sum of damages in the circuit year).
[24] When the relationship between judge characteristics and judge votes on damages are examined, Democratic appointees have a positive effect while female judges have a negligible effect.

displayed in the even columns. Every $10,000 of damages awarded (the average damages awarded in a circuit year is $47,820 (Appendix Table A)) increases female earnings relative to male earnings by 0.8% (Panel 1 Column 2), female hours relative to male hours by 0.035 hours per week or a 0.23 8-hour workday (Panel 1 Column 6), the probability a female has any employment relative to the probability a male has any employment by 0.12% (Panel 2 Column 2), and the probability a female is a manager relative to the probability a male is a manager by 0.04% (Panel 2 Column 6). The lag coefficients are jointly significant and there is evidence that lagged damages have a stronger effect than damages awarded in the current year (Columns 3-4 and 7-8). This effect is likely due to the delay between the announcement of the damage award by the district court and final affirmation by the court of appeals. Note that this result contrasts with the lag patterns found in Tables 2-5, where contemporaneous law generally has the strongest effect.

To examine whether the extensive margin of damages has an effect, we turn to Panels 3 and 4. Each case where the appellate judges approved any damages increases female earnings relative to male earnings by 13.6% (Panel 3 Column 2), female hours relative to male hours by 0.608 hours per week or a 4.0 8-hour workday (Panel 3 Column 6), the probability a female has any employment relative to the probability a male has any employment by 2.1% (Panel 4 Column 2), and the probability a female is a manager relative to the probability a male is a manager by 0.6% (Panel 4 Column 6). The lag coefficients are also large and significant and, in the case of hours, the lagged measure of the number of cases with any damages has a significant effect while the current year measure does not.

H. *Law or Economics?*

We now turn to the question of whether sexual harassment damages or the law itself has a greater effect. In Panels 1-3 of Appendix Table E, we display a horse race between sexual harassment damages and the number of pro-punishment cases using our collection of sexual harassment cases from 1982-2002, while in Panels 4-5, we display the horse race between damages and sexual harassment cases using the CJP collection from 1995-2002. Quite remarkably, across all outcome measures, damages awarded have no statistically significant effect while the number of pro-punishment cases has a strong effect (Columns 2, 4, 6, and 8 in Panels 1-3) in the IV specifications. The effect of the number of pro-punishment cases changes little in magnitude when we control for damages (Compare Column 2 with Table 2, Column 4 with Table 3, Column 6 with Table 4, and Column 8 with Table 5).

Moreover, the estimates in Panels 1-3 for the number of pro-punishment cases are very similar and extremely close in magnitude. This is a corroboration of our identification strategy. The damage cases are a subset of the overall sexual harassment cases, so it is not surprising that the effect for one set of laws should be reduced. Panel 1 would suggest that the intensive margin of damages is not what matters, but rather the extensive margin does. Panel 2, however, indicates that the extensive margin of damages also fades in effect in comparison to the number of pro-punishment cases. Panel 3 indicates that when we measure damages in logs, it has an independent positive effect in the OLS specifications (Columns 1, 3, 5, and 7) but not in the IV specifications, despite having a very strong first stage (Columns 2 and 6 in Appendix Table C). Panels 4-5 using the CJP data indicate that the number of pro-punishment cases has a positive effect on management in the shorter time frame, but damages have no effect or even a negative effect (Column 8). Log damages with the CJP data show similar results (not displayed).

Intriguingly, when restricted to the large firms with over 1000 employees, gender inequality in employment does not respond to the number of pro-punishment cases, and for earnings, it responds to sexual harassment damages. In the IV damages specifications, female employment status and earnings worsen at large firms relative to male employment status and earnings. The respective coefficients are positive for small firms. Overall and for small firms, we do replicate the finding that the number of pro-punishment cases generally has a strong positive effect on gender inequality while sexual harassment damages do not.

The number of pro-punishment cases is one measure of law but legal doctrine is another measure. We capture three significant moments of doctrinal change through the major Supreme Court decisions on sexual harassment law (MacKinnon 2007). In *Meritor Savings Bank v. Vinson*, 1986, the Supreme Court recognized that when a supervisor harasses a subordinate on the basis of sex, the supervisor discriminates on the basis of sex. Thus, *Meritor* established the hostile work environment doctrine of sexual harassment law. In *Harris v. Forklift Inc. Systems*, 1993, the Supreme Court established that a plaintiff's psychological well-being did not need to be investigated and that only the environment would need to be reasonably perceived as hostile and abusive. This decision made it much easier to get damages. In *Faragher v. City of Boca Raton*, 1998, the Supreme Court subjected an employer to vicarious liability for a supervisor's sexual harassment of an employee, thereby expanding the potential for employer liability.[25] To code these doctrinal shifts, we use information in the Supreme Court opinions and their direct history on Westlaw noting whether there was a circuit split. We code the law as 1 for each circuit and year in the year of the Supreme Court decision and following years. We code the

[25] *Burlington Industries, Inc. v. Ellerth*, 1998, was decided in the same year and stood for the same doctrine as *Faragher v. City of Boca Raton*; hence we do not code it separately.

circuits mentioned in the circuit split as 0 or 1 for years prior to the decision.[26] For *Meritor*, the DC circuit was coded as 0 and Circuits 5, 9, and 11 were coded as 1 before 1986. For *Harris,* Circuits 6 and 11 were coded as 0 and Circuit 9 was coded as 1 before 1993. For *Faragher*, before 1998, Circuits 3, 7, 9, and 11 were coded as 0 and Circuits 5 and 6 were coded as 1.[27]

All regressions are OLS and their results are shown in Appendix Table F. *Meritor* decreased female earnings relative to male earnings by 68.4% (Panel 1 Column 1), decreased female hours relative to male hours by 6.082 hours per week or 40 8-hour workdays (Panel 1 Column 9), decreased the probability a female has any employment relative to the probability a male has any employment by 9.4% (Panel 2 Column 1), and affected negligibly the probability a female is a manager relative to the probability a male is a manager (Panel 2 Column 9). The lag coefficients are large and significant and generally indicate a delayed effect of *Meritor* rather than a contemporaneous effect of *Meritor*. Since there is no random assignment of Supreme Court justices and the DC circuit may be very different from other circuits for a number of reasons before and after 1986, consequently we do not view these results as robust.

The results of the other two Supreme Court cases are more consistent with our findings using appellate cases. *Harris* increased female earnings relative to male earnings by 25.4% (Panel 1 Column 3), female hours relative to male hours by 1.164 hours per week or 7.6 8-hour workdays (Panel 1 Column 11), the probability a female is employed relative to the probability a male is employed by 3.7% (Panel 4 Column 3), and the probability a female is a manager relative to the probability a male is a manager by 1.2% (Panel 4 Column 11). The lag coefficients here display the same pattern we have seen with the number of pro-punishment cases

[26] Circuits not mentioned in these opinions are coded as missing. In order to include all of the law changes in one regression, we create an additional variable that is a dummy variable indicating whether the law variable is missing. In accordance with standard econometrics practice, we fill in the missing values with a constant.

[27] The DC Circuit and the Fourth Circuit were coded as missing because the Supreme Court noted two opposing opinions in those circuits prior to 1998.

and the contemporaneous year effects are larger than the lagged effects.

Faragher increased female earnings relative to male earnings by 9.7% (Panel 1 Column 5), female hours relative to male hours by 0.497 hours per week or 3.2 8-hour workdays (Panel 1 Column 13), the probability a female is employed relative to the probability a male is employed by 1.9% (Panel 2 Column 5), and the probability a female is a manager relative to the probability a male is a manager by 0.8% (Panel 2 Column 13). The lag coefficients show weaker effects than the contemporaneous year effect for employment status (Panel 2 Columns 6).

These effects are similar in magnitude when all three decisions are run together and when we control for the number of cases with any damages and the number of pro-punishment cases. The number of pro-punishment cases has positive and statistically significant effects for earnings, employment status, and management, though the effects are somewhat smaller than the OLS estimates for the number of pro-punishment cases displayed in previous tables. Note that in Column 16 of Table F, *Meritor* has a negative effect on female management and this effect is statistically significant at the 5% level. When using the CJP collection of sexual harassment cases for 1995-2002, *Faragher* has a positive and statistically significant effect on employment status while the number of pro-punishment cases has positive and statistically significant effects on employment status and management status (not shown). The number of cases with any damages awarded does not have a statistically significant effect on any outcome.

In sum, sexual harassment law, and in particular, legal doctrine, has a strong effect on gender inequality while damages awarded in sexual harassment cases have a weak effect when we also control for the law itself. This finding sheds light on the longstanding debate on why individuals obey the law – is it because the law has legitimacy or because the law incentivizes. At least in the case of sexual harassment law, our analysis corroborates the former view rather than the latter.

VI. Conclusion

This paper estimates the impact of sexual harassment law on wage, employment, and managerial inequality between men and women. We use the random assignment of appellate court judges to test the relevance of legal and economic theories of the effects of sexual harassment. Female judges and Democratic appointees are more likely to decide in favor of punishing sexual harassment. Using this fact to instrument for the quantity of pro-plaintiff cases at the circuit-year level, we find that sexual harassment law does not decrease female wages relative to male wages. Each pro-punishment decision increases the hours worked of women by 3.3 days per year relative to that of their male colleagues. We also find that each pro-punishment decision increases the probability that a female is partially or fully employed by roughly 1.7 percentage points relative to that of her male colleagues. Moreover, we find positive and significant effects of sexual harassment law on the likelihood that a female will be a manager, effects that appear to explain a substantial portion of the gender gap in management in recent years.

These findings are more consistent with an insider-outsider theory of harassment and involuntary unemployment and legal theories that view sexual harassment as a form of economic warfare by men battling to preserve their jobs than with economic theories that view sexual harassment through the lens of compensating differentials and mandated benefits, which predict that wages and/or employment would fall rather than rise. That sexual harassment law has a positive impact on gender inequality when we include those not in the labor force but a negative employment impact on gender inequality when the population is restricted to those already in the labor force suggests that some women may be encouraged to leave the home and seek work in response to favorable work environments while other women bear the cost of lost economic rents or monitoring programs.

Feminist legal theorists interpreting existing gender discrimination law had practical ramifications that did not exist prior to the introduction of the interpretation. Our analysis of gender discrimination vs. sexual harassment law shows that random assignment of interpreters with different but predictable decision-making tendencies permits the evaluation of rules and interpretations. Using the random assignment of appellate judges, scholars can consider the causal impact of appellate law on economy-wide outcomes and examine which legal rules and interpretations are socially beneficial. We have considered the possibility that the effects of sexual harassment law are spurious since we could have captured the effects of discrimination law more generally as opposed to the effects of sexual harassment law. Since, however, our identification comes from the random assignment of females and Democratic appointee judges to sexual harassment panels, this concern is mitigated. Our analysis demonstrates that, in fact, gender discrimination law has independent effects on earnings and employment status, and surprisingly little effect compared to sexual harassment law on female management. We show that the effects of sexual harassment law and gender discrimination law are robust to a horserace between both sets of laws, each instrumented for by the random assignment of judges to the respective case types. Our results distinguishing sexual harassment law from gender discrimination law suggest that sexual harassment is theoretically and empirically very different from gender discrimination, a distinction not noted in the economic literature.

The question of whether law delivers its impact through its legitimacy or its incentives is of interest beyond the discrimination context. We find that damages awarded in sexual harassment cases have a positive effect on gender inequality, but that this effect disappears when we control for other, more legal, measures of sexual harassment law, such as the number of pro-punishment cases and the doctrinal changes brought about by significant Supreme Court decisions. This finding sheds light on the long-standing debate on why individuals obey the law – is it because individuals believe it is moral to obey the law and so they should, or is it because individuals fear the monetary sanctions received from breaking the law (Becker 1968)? At least in the case of sexual harassment law, our analysis does not corroborate the latter view.

We conclude that we do not find many of the negative effects theorized by existing economic models of sexual harassment. In light of our mostly positive effects and survey evidence indicating higher job satisfaction following sexual harassment law, our findings raise questions as to whether any other forms of anti-discrimination law forbidding identity-based harassment are welfare-improving with respect to their economic consequences.

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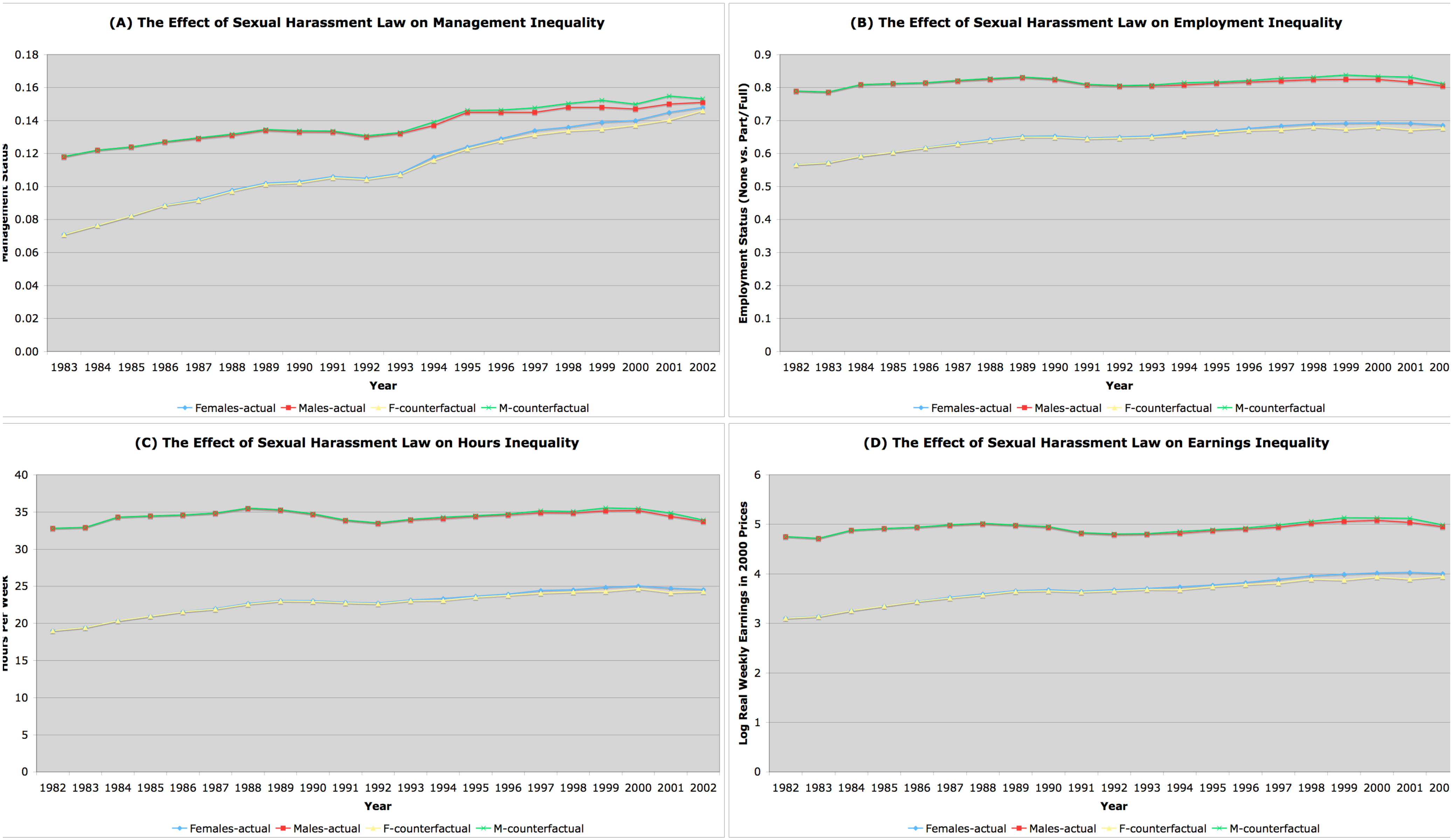

Figure 1: This figure shows the male and female probability of being in management, the probability of being employed, hours per week, and weekly earnings from 1982 to 2002 using the MORG CPS data. The outer two lines display the counterfactual trend for these employment outcomes. The inner two lines display the actual trend for males and females. The counterfactual is computed using the specification with both instruments (Column 4) of Tables 2-5.

Figure 2: The Effect of Sexual Harassment Law on Earnings Inequality

Figure 3: The Effect of Sexual Harassment Law on Hours Inequality

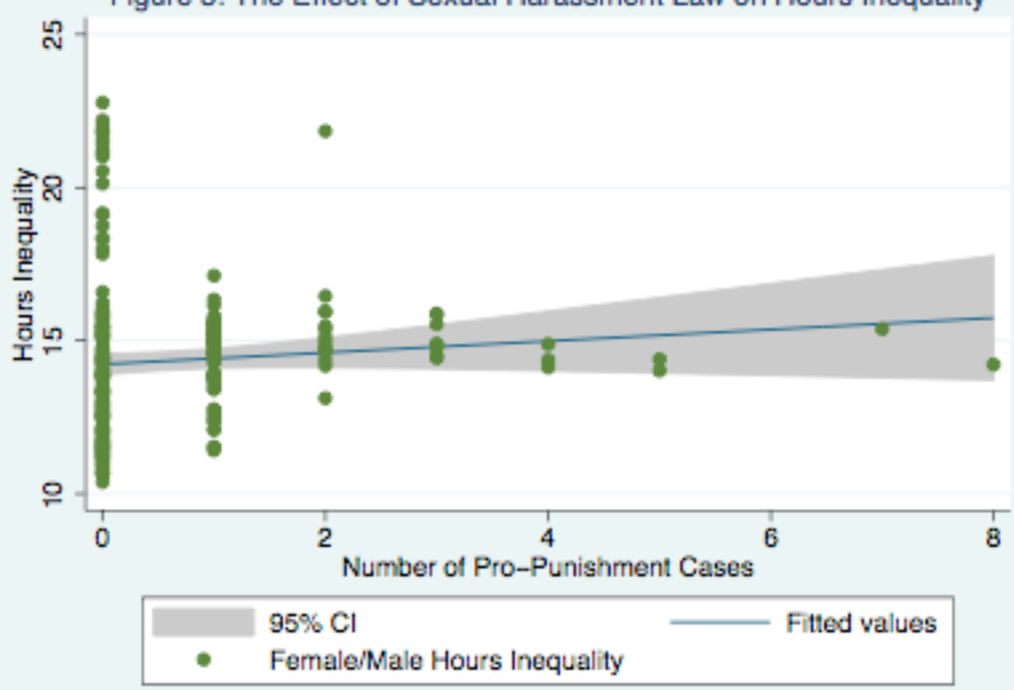

Figure 4: The Effects of Sexual Harassment Law on Employment Inequality

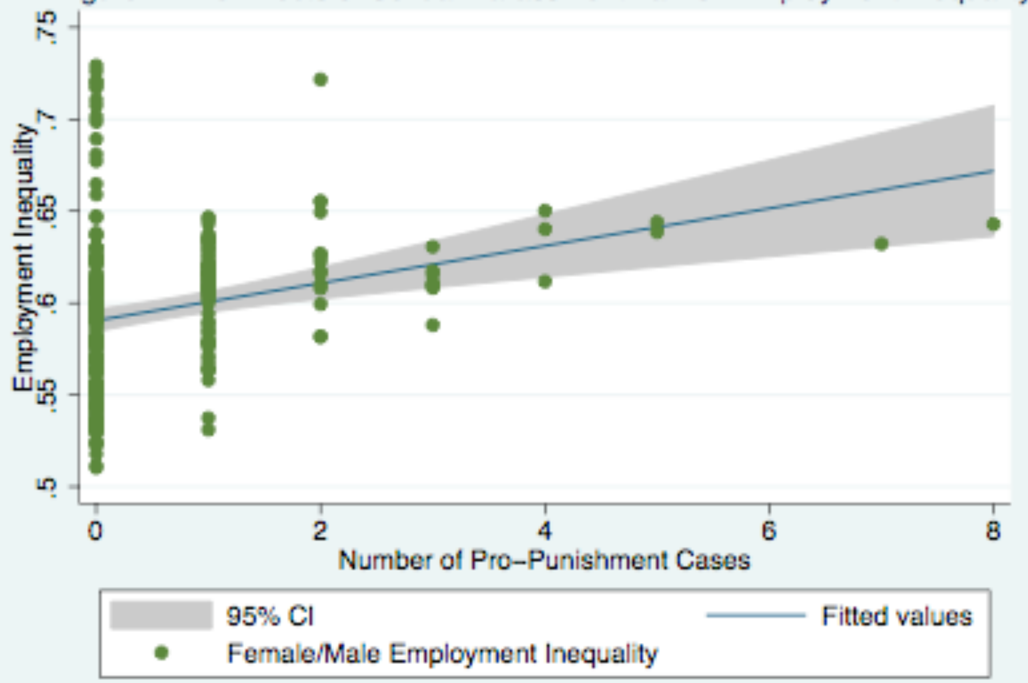

Figure 5: The Effect of Sexual Harassment Law on Management Inequality

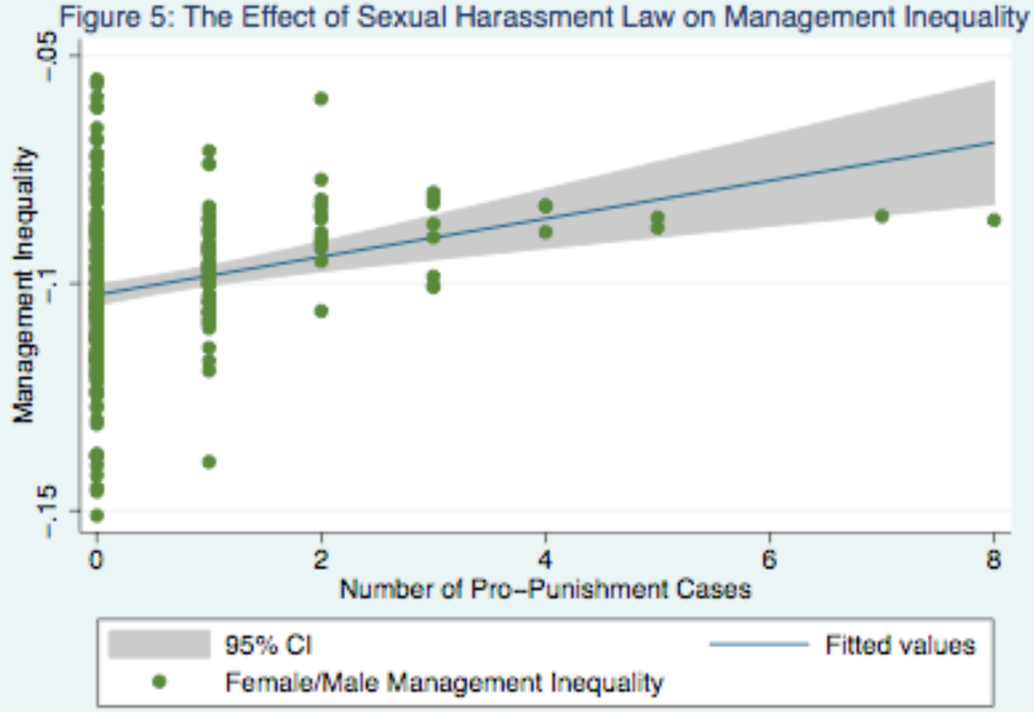

Table 1 -- First Stage: Relationship Between Number of Pro-punishment Sexual Harassment Decisions and Number of Sexual Harassment Panels with Female and Democratic Appointee Judges, 1982-2002

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of Pro-punishment Sexual Harassment Decisions			Number of Pro-punishment Sexual Harassment Decisions / Docket Size		
Sexual Harassment Panels with Female Judges	0.712**		0.0113			
	(0.164)		(0.143)			
Sexual Harassment Panels with Democratic Appointee Judges		0.793**	0.791**			
		(0.0679)	(0.0824)			
Sexual Harassment Panels with Female Judges / Docket Size				0.853**		0.104
				(0.158)		(0.150)
Sexual Harassment Panels with Democratic Appointee Judges / Docket Size					0.807**	0.776**
					(0.0621)	(0.0812)
F-statistic testing joint significance of instruments			70.68			91.05
(p-value)			(0.000)			(0.000)
N	5429470	5429470	5429470	5429470	5429470	5429470
R-sq	0.507	0.754	0.754	0.530	0.772	0.774

Notes: Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for circuit fixed-effects, year fixed-effects, the fraction of female judges in a given circuit-year, the fraction of Democratic appointees in a given circuit-year, and, in Columns 1-3, Docket Size. Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions.
+ Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 2: The Effect of Sexual Harassment Cases on Log Real Weekly Earnings, 1982-2002

	Including Not in Labor Force					Excluding Not in Labor Force				
	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)	
Panel 1	OLS	IV with female judges	IV with democratic appointee judges	IV with both	N R-sq	OLS	IV with female judges	IV with democratic appointee judges	IV with both	N R-sq
Number of Pro-punishment Cases * Female	0.0753**	0.0922**	0.106**	0.101**	4943024	0.000368	-0.00547	0.00178	0.000715	3707383
	(0.0128)	(0.0250)	(0.0153)	(0.0163)	0.132	(0.00358)	(0.00771)	(0.00450)	(0.00470)	0.110
Number of Pro-punishment Cases$_{t-1}$ * Female	0.0701**	0.0748**	0.0966**	0.0921**	4683268					
	(0.0127)	(0.0212)	(0.0144)	(0.0151)	0.130					
Number of Pro-punishment Cases$_{t-2}$ * Female	0.0742**	0.0845**	0.0978**	0.0957**	4426490					
	(0.0104)	(0.0216)	(0.0126)	(0.0128)	0.129					
Panel 2										
(Number of Pro-punishment Cases / Docket Size) * Female	240.1**	295.7**	338.6**	330.3**	4943024	-7.667	-14.51	8.814	6.204	3707383
	(41.90)	(83.90)	(48.20)	(54.05)	0.132	(13.38)	(25.26)	(15.38)	(16.62)	0.110
(Number of Pro-punishment Cases / Docket Size)$_{t-1}$ * Female	220.4**	246.1**	315.4**	302.3**	4683268					
	(41.98)	(73.20)	(45.62)	(50.83)	0.130					
(Number of Pro-punishment Cases / Docket Size)$_{t-2}$ * Female	225.4**	282.8**	309.3**	310.7**	4426490					
	(38.79)	(86.51)	(45.02)	(47.49)	0.129					
Panel 3										
Number of Pro-punishment Cases * Female	0.0352**	0.0597*	0.0488**	0.0466**	4426490	-0.00107	0.00789	0.000810	-0.00103	3336493
	(0.0103)	(0.0276)	(0.0145)	(0.0145)	0.129	(0.00491)	(0.0120)	(0.00665)	(0.00652)	0.108
Number of Pro-punishment Cases$_{t-1}$ * Female	0.0273*	0.0549+	0.0314	0.0312+		0.000432	0.00460	0.000414	-0.000534	
	(0.0108)	(0.0279)	(0.0195)	(0.0188)		(0.00520)	(0.0139)	(0.00882)	(0.00835)	
Number of Pro-punishment Cases$_{t-2}$ * Female	0.0426**	-0.00267	0.0467*	0.0476*		0.00395	-0.0358	0.00132	0.00486	
	(0.0128)	(0.0428)	(0.0210)	(0.0207)		(0.00576)	(0.0218)	(0.00877)	(0.00873)	
F-statistic testing joint significance of lags	23.09	9.54	20.81	20.87		0.28	1.52	0.08	0.19	
(p-value)	(0.000)	(0.000)	(0.000)	(0.000)		(0.841)	(0.211)	(0.972)	(0.906)	
Panel 4										
(Number of Pro-punishment Cases / Docket Size) * Female	110.4**	162.5	142.7**	134.6**	4426490	-9.998	27.47	-1.238	-10.31	3336493
	(35.66)	(108.0)	(48.36)	(50.23)	0.129	(17.51)	(42.74)	(21.23)	(21.10)	0.108
(Number of Pro-punishment Cases / Docket Size)$_{t-1}$ * Female	85.32*	170.9+	101.3	108.9+		-10.05	8.764	-8.690	-12.14	
	(39.05)	(99.79)	(63.63)	(64.37)		(19.05)	(38.85)	(25.44)	(25.29)	
(Number of Pro-punishment Cases / Docket Size)$_{t-2}$ * Female	134.2**	44.44	153.0*	158.1*		8.118	-92.86+	10.34	23.67	
	(48.27)	(136.4)	(71.00)	(70.59)		(22.00)	(53.91)	(28.36)	(29.35)	
F-statistic testing joint significance of lags	19.80	10.89	19.31	20.10		0.36	1.78	0.06	0.24	
(p-value)	(0.000)	(0.000)	(0.000)	(0.000)		(0.785)	(0.152)	(0.983)	(0.865)	

Notes: All regressions use MORG CPS. Weekly earnings are coded as zero for individuals not in the labor force. Log real weekly earnings are in 2000 prices. Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for age, sex, race, educational attainment, marital status, circuit and year-fixed effects, as well as the fraction of female judges, the fraction of Democratic appointees, and, when the endogenous variable is not a ratio, docket size. The circuit-by-year controls are lagged and interacted with the female dummy in the same way that the endogenous variable and instruments are. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. In Panels 1 and 2, regressions on the endogenous variables and its lags are run separately, while in Panels 3 and 4, the endogenous variable and lags are run together. The number of observations and R-square are displayed only for the IV regression with both instruments. Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions.

+ Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 3: The Effect of Sexual Harassment Cases on Hours Worked Last Week, 1982-2002

	Including Not in Labor Force					Excluding Not in Labor Force				
	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)	
Panel 1	OLS	IV with female judges	IV with democratic appointee judges	IV with both	N R-sq	OLS	IV with female judges	IV with democratic appointee judges	IV with both	N R-sq
Number of Pro-punishment Cases * Female	0.330**	0.472**	0.542**	0.491**	5223699	-0.137**	-0.148*	-0.110*	-0.131**	3988058
	(0.0628)	(0.131)	(0.0848)	(0.0861)	0.134	(0.0338)	(0.0735)	(0.0473)	(0.0417)	0.077
Number of Pro-punishment Cases$_{t-1}$ * Female	0.329**	0.400**	0.518**	0.464**	4950255					
	(0.0611)	(0.110)	(0.0754)	(0.0773)	0.132					
Number of Pro-punishment Cases$_{t-2}$ * Female	0.343**	0.444**	0.512**	0.479**	4678751					
	(0.0620)	(0.133)	(0.0843)	(0.0801)	0.130					
Panel 2										
(Number of Pro-punishment Cases / Docket Size) * Female	968.3**	1424.1**	1636.8**	1576.9**	5223699	-582.5**	-565.5**	-431.1*	-451.4**	3988058
	(224.9)	(468.7)	(295.5)	(304.9)	0.134	(121.5)	(199.0)	(179.8)	(144.1)	0.077
(Number of Pro-punishment Cases / Docket Size)$_{t-1}$ * Female	934.4**	1205.4**	1581.1**	1466.0**	4950255					
	(224.9)	(399.3)	(264.4)	(277.6)	0.132					
(Number of Pro-punishment Cases / Docket Size)$_{t-2}$ * Female	954.2**	1324.1*	1507.8**	1522.7**	4678751					
	(234.8)	(546.0)	(299.8)	(301.7)	0.130					
Panel 3										
Number of Pro-punishment Cases * Female	0.120*	0.289+	0.212*	0.208*	4678751	-0.109*	-0.0602	-0.0907	-0.0944	3588754
	(0.0560)	(0.157)	(0.0950)	(0.0836)	0.130	(0.0444)	(0.100)	(0.0751)	(0.0627)	0.074
Number of Pro-punishment Cases$_{t-1}$ * Female	0.145*	0.304+	0.194	0.169		-0.0189	-0.0339	-0.0122	-0.0331	
	(0.0607)	(0.173)	(0.129)	(0.113)		(0.0467)	(0.106)	(0.0843)	(0.0691)	
Number of Pro-punishment Cases$_{t-2}$ * Female	0.223**	-0.0257	0.255+	0.248+		-0.0110	-0.183	-0.00329	0.00542	
	(0.0778)	(0.273)	(0.144)	(0.132)		(0.0483)	(0.161)	(0.0784)	(0.0736)	
F-statistic testing joint significance of lags	15.79	7.42	13.34	15.12		6.29	3.44	2.08	3.70	
(p-value)	(0.000)	(0.001)	(0.000)	(0.000)		(0.000)	(0.018)	(0.104)	(0.012)	
Panel 4										
(Number of Pro-punishment Cases / Docket Size) * Female	315.2	720.4	530.2	523.1+	4678751	-457.6**	-229.9	-397.9	-410.1+	3588754
	(208.4)	(642.8)	(342.5)	(298.0)	0.130	(167.7)	(323.3)	(295.8)	(225.7)	0.074
(Number of Pro-punishment Cases / Docket Size)$_{t-1}$ * Female	389.4	909.4	524.5	529.6		-204.5	-239.0	-208.4	-261.0	
	(238.4)	(620.9)	(442.0)	(397.2)		(183.9)	(332.6)	(324.5)	(238.9)	
(Number of Pro-punishment Cases / Docket Size)$_{t-2}$ * Female	675.3*	113.9	806.5	868.5+		-85.68	-531.6	0.274	103.9	
	(309.2)	(862.9)	(497.1)	(467.0)		(196.3)	(501.3)	(315.5)	(280.6)	
F-statistic testing joint significance of lags	9.70	6.87	9.32	12.16		9.29	5.83	4.14	5.07	
(p-value)	(0.000)	(0.002)	(0.000)	(0.000)		(0.000)	(0.001)	(0.007)	(0.002)	

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for age, sex, race, educational attainment, marital status, circuit and year-fixed effects, as well as the fraction of female judges, the fraction of Democratic appointees, and, when the endogenous variable is not a ratio, docket size. The circuit-by-year controls are lagged and interacted with the female dummy in the same way that the endogenous variable and instruments are. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. In Panels 1 and 2, regressions on the endogenous variables and its lags are run separately, while in Panels 3 and 4, the endogenous variable and lags are run together. The number of observations and R-square are displayed only for the IV regression with both instruments. Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions.

+ Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 4: The Effect of Sexual Harassment Cases on Employment Status (None vs. Part/Full-time), 1982-2002

	Including Not in Labor Force					Excluding Not in Labor Force				
	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)	
Panel 1	OLS	IV with female judges	IV with democratic appointee judges	IV with both	N R-sq	OLS	IV with female judges	IV with democratic appointee judges	IV with both	N R-sq
Number of Pro-punishment Cases * Female	0.0123**	0.0156**	0.0174**	0.0165**	5398935	0.000998*	-0.00164*	-0.00130**	-0.00134**	4163294
	(0.00184)	(0.00368)	(0.00218)	(0.00232)	0.098	(0.000287)	(0.000671)	(0.000384)	(0.000396)	0.034
Number of Pro-punishment Cases$_{t-1}$ * Female	0.0116**	0.0134**	0.0162**	0.0154**	5114313					
	(0.00180)	(0.00316)	(0.00205)	(0.00215)	0.097					
Number of Pro-punishment Cases$_{t-2}$ * Female	0.0121**	0.0162**	0.0163**	0.0157**	4832139					
	(0.00151)	(0.00345)	(0.00192)	(0.00189)	0.096					
Panel 2										
(Number of Pro-punishment Cases / Docket Size) * Female	41.30**	51.39**	55.98**	54.92**	5398935	-2.184*	-2.744	-2.624*	-2.679*	4163294
	(5.916)	(12.20)	(6.790)	(7.610)	0.098	(0.901)	(1.788)	(1.141)	(1.132)	0.034
(Number of Pro-punishment Cases / Docket Size)$_{t-1}$ * Female	38.70**	45.35**	53.08**	51.39**	5114313					
	(5.855)	(11.03)	(6.491)	(7.171)	0.097					
(Number of Pro-punishment Cases / Docket Size)$_{t-2}$ * Female	38.55**	51.93**	51.50**	51.67**	4832139					
	(5.460)	(13.62)	(6.593)	(6.871)	0.096					
Panel 3										
Number of Pro-punishment Cases * Female	0.00587**	0.00936*	0.00785**	0.00779**	4832139	-0.000562	-0.000342	-0.000777	-0.000712	3742142
	(0.00141)	(0.00424)	(0.00197)	(0.00197)	0.096	(0.000352)	(0.000900)	(0.000538)	(0.000511)	0.030
Number of Pro-punishment Cases$_{t-1}$ * Female	0.00470**	0.00886*	0.00564*	0.00545*		-0.0000676	-0.000672	-0.000102	-0.000278	
	(0.00143)	(0.00413)	(0.00265)	(0.00257)		(0.000364)	(0.00102)	(0.000670)	(0.000653)	
Number of Pro-punishment Cases$_{t-2}$ * Female	0.00669**	0.00266	0.00760*	0.00735*		-0.0000198	-0.00109	0.0000116	0.000141	
	(0.00170)	(0.00683)	(0.00308)	(0.00294)		(0.000418)	(0.00163)	(0.000649)	(0.000641)	
F-statistic testing joint significance of lags	30.25	15.95	26.08	27.31		1.83	2.70	2.01	1.86	
(p-value)	(0.000)	(0.000)	(0.000)	(0.000)		(0.142)	(0.046)	(0.113)	(0.137)	
Panel 4										
(Number of Pro-punishment Cases / Docket Size) * Female	20.33**	27.28	24.68**	24.29**	4832139	-0.761	1.776	-1.092	-1.356	3742142
	(4.929)	(16.76)	(6.696)	(7.033)	0.096	(1.229)	(3.357)	(1.433)	(1.447)	0.030
(Number of Pro-punishment Cases / Docket Size)$_{t-1}$ * Female	16.53**	31.52*	19.80*	21.25*		-0.0753	1.089	0.0866	-0.270	
	(5.227)	(15.46)	(9.161)	(9.279)		(1.310)	(2.955)	(2.092)	(2.039)	
(Number of Pro-punishment Cases / Docket Size)$_{t-2}$ * Female	20.79**	10.60	22.88*	22.78*		-0.995	-7.026	-0.801	-0.192	
	(6.510)	(23.04)	(10.33)	(10.12)		(1.490)	(4.889)	(1.981)	(2.015)	
F-statistic testing joint significance of lags	29.50	18.22	26.74	29.48		1.08	1.71	0.78	0.77	
(p-value)	(0.000)	(0.000)	(0.000)	(0.000)		(0.356)	(0.166)	(0.508)	(0.512)	

Notes: All regressions use MORG CPS. MORG does not distinguish between part-time and full-time employment. Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for age, sex, race, educational attainment, marital status, circuit and year-fixed effects, as well as the fraction of female judges, the fraction of Democratic appointees, and, when the endogenous variable is not a ratio, docket size. The circuit-by-year controls are lagged and interacted with the female dummy in the same way that the endogenous variable and instruments are. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. In Panels 1 and 2, regressions on the endogenous variables and its lags are run separately, while in Panels 3 and 4, the endogenous variable and lags are run together. The number of observations and R-square are displayed only for the IV regression with both instruments. Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions.

+ Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 5: The Effect of Sexual Harassment Cases on Management, 1982-2002

	Including Not in Labor Force					Excluding Not in Labor Force				
	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)	
	OLS	IV with female judges	IV with democratic appointee judges	IV with both	N R-sq	OLS	IV with female judges	IV with democratic appointee judges	IV with both	N R-sq
Panel 1										
Number of Pro-punishment Cases * Female	0.00388**	0.00527**	0.00509**	0.00483**	4334364	0.00335**	0.00401**	0.00458**	0.00413**	3939156
	(0.000487)	(0.00101)	(0.000601)	(0.000606)	0.059	(0.000442)	(0.000895)	(0.000569)	(0.000553)	0.058
Number of Pro-punishment Cases$_{t-1}$ * Female	0.00342**	0.00419**	0.00457**	0.00419**	4334364					
	(0.000506)	(0.00103)	(0.000577)	(0.000601)	0.059					
Number of Pro-punishment Cases$_{t-2}$ * Female	0.00326**	0.00310**	0.00449**	0.00429**	4092008					
	(0.000516)	(0.00114)	(0.000646)	(0.000602)	0.059					
Panel 2										
(Number of Pro-punishment Cases / Docket Size) * Female	11.65**	13.71**	15.19**	15.25**	4334364	9.776**	9.844**	12.97**	12.71**	3939156
	(1.606)	(3.426)	(2.118)	(2.069)	0.059	(1.487)	(3.243)	(2.051)	(1.837)	0.058
(Number of Pro-punishment Cases / Docket Size)$_{t-1}$ * Female	9.744**	9.624**	13.68**	12.78**	4334364					
	(1.764)	(3.230)	(2.055)	(2.105)	0.059					
(Number of Pro-punishment Cases / Docket Size)$_{t-2}$ * Female	8.839**	7.143+	12.40**	12.69**	4092008					
	(1.866)	(4.181)	(2.414)	(2.201)	0.059					
Panel 3										
Number of Pro-punishment Cases * Female	0.00262**	0.00390**	0.00294**	0.00273**	4092008	0.00241**	0.00330**	0.00269**	0.00235**	3730397
	(0.000536)	(0.00134)	(0.000746)	(0.000626)	0.059	(0.000498)	(0.00116)	(0.000781)	(0.000602)	0.058
Number of Pro-punishment Cases$_{t-1}$ * Female	0.00117*	0.00393*	0.00151+	0.00135+		0.000886+	0.00283+	0.00116	0.000833	
	(0.000508)	(0.00177)	(0.000851)	(0.000717)		(0.000466)	(0.00165)	(0.000899)	(0.000729)	
Number of Pro-punishment Cases$_{t-2}$ * Female	0.00122+	-0.00216	0.00162	0.00166+		0.000916	-0.00264	0.00157	0.00173*	
	(0.000632)	(0.00265)	(0.00107)	(0.000909)		(0.000609)	(0.00216)	(0.00104)	(0.000864)	
F-statistic testing joint significance of lags	22.08	14.29	21.27	22.63		21.26	11.74	18.27	18.99	
(p-value)	(0.000)	(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)	(0.000)	
Panel 4										
(Number of Pro-punishment Cases / Docket Size) * Female	7.695**	10.06*	8.664**	7.965**	4092008	6.710**	8.193+	7.317*	6.457**	3730397
	(1.709)	(4.785)	(2.776)	(2.091)	0.059	(1.701)	(4.548)	(2.949)	(2.077)	0.058
(Number of Pro-punishment Cases / Docket Size)$_{t-1}$ * Female	3.153+	8.973+	4.108	3.965+		1.913	5.738	2.519	2.023	
	(1.682)	(5.215)	(3.145)	(2.391)		(1.630)	(5.234)	(3.327)	(2.459)	
(Number of Pro-punishment Cases / Docket Size)$_{t-2}$ * Female	3.197	-5.285	3.978	4.921		2.271	-6.494	3.302	4.722	
	(2.265)	(6.655)	(3.815)	(3.145)		(2.169)	(6.305)	(3.787)	(2.996)	
F-statistic testing joint significance of lags	18.63	8.01	13.38	18.06		16.05	5.87	11.23	14.68	
(p-value)	(0.000)	(0.000)	(0.000)	(0.000)		(0.000)	(0.001)	(0.000)	(0.000)	

Notes: All regressions use MORG CPS. Management is defined as: (1) Administrators and Officials, Public Administration, (2) Other Executive, Administrators, and Managers, or (3) Management Related Occupations. Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for age, sex, race, educational attainment, marital status, circuit and year-fixed effects, as well as the fraction of female judges, the fraction of Democratic appointees, and, when the endogenous variable is not a ratio, docket size. The circuit-by-year controls are lagged and interacted with the female dummy in the same way that the endogenous variable and instruments are. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. In Panels 1 and 2, regressions on the endogenous variables and its lags are run separately, while in Panels 3 and 4, the endogenous variable and lags are run together. N and R-square are displayed only for the IV regression with both instruments. Docket Size is the number of appellate terminations minus the number of sexual harassment decisions.

+ Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 6 -- First Stage: Relationship Between Number of Pro-punishment Gender Discrimination Decisions and Number of Gender Discrimination/Sexual Harassment Panels with Female and Democratic Appointee Judges, 1995-2002

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Number of Pro-punishment Gender Discrimination Decisions						Number of Pro-punishment Gender Discrimination Decisions / Docket Size					
Gender Discrimination Panels with Female Judges	0.450**		0.0720									
	(0.107)		(0.124)									
Gender Discrimination Panels with Democrat Appointee Judges		0.413**	0.387**									
		(0.0612)	(0.0777)									
Gender Discrimination Panels with Female Judges / Docket Size							0.461**		0.0851			
							(0.119)		(0.149)			
Gender Discrimination Panels with Democrat Appointee Judges / Docket Size								0.427**	0.380**			
								(0.0922)	(0.127)			
F-statistic testing joint significance of instruments			24.18						10.87			
(p-value)			(0.000)						(0.000)			
Sexual Harassment Panels with Female Judges				0.198+		0.249*						
				(0.111)		(0.116)						
Sexual Harassment Panels with Democratic Appointee Judges					0.0375	-0.0682						
					(0.0905)	(0.0983)						
Sexual Harassment Panels with Female Judges / Docket Size										0.0938		0.206
										(0.121)		(0.140)
Sexual Harassment Panels with Democratic Appointee Judges / Docket Size											-0.0594	-0.141
											(0.104)	(0.119)
F-statistic testing joint significance of instruments						2.36						1.18
(p-value)						(0.100)						(0.311)
N	1895577	1895577	1895577	1895577	1895577	1895577	1895577	1895577	1895577	1895577	1895577	1895577
R-sq	0.411	0.520	0.537	0.541	0.598	0.601	0.274	0.266	0.299	0.408	0.408	0.423

Notes: Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for circuit fixed-effects, year fixed-effects, the fraction of female judges in a given circuit-year, the fraction of Democratic appointees in a given circuit-year, and, in Columns 1-6, Docket Size. Docket Size is the number of appellate terminations in the circuit year minus the number of gender discrimination decisions. Gender discrimination cases do not include sexual harassment cases.

+ Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 7: A Horse Race Between Sexual Harassment and Gender Discrimation Law, 1995-2002

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	OLS			IV			OLS			IV		
	Management						*Employment Status*					
Number of Pro-punishment Sexual Harassment Cases * Female	0.00153**	0.00143**		0.00202**	0.00213**		0.00172+	0.00199*		0.00184	0.00236**	
	(0.000389)	(0.000373)		(0.000515)	(0.000481)		(0.000889)	(0.000816)		(0.00114)	(0.000866)	
Number of Pro-punishment Gender Discrimination Cases * Female	-0.000459		0.000123	0.000435		0.00196+	0.00120		0.00185+	0.00253		0.00448**
	(0.000496)		(0.000560)	(0.000856)		(0.00102)	(0.00121)		(0.00111)	(0.00205)		(0.00166)
N	1541695	1541695	1541695	1541695	1541695	1541695	1886023	1886023	1886023	1886023	1886023	1886023
R-sq	0.056	0.056	0.056	0.056	0.056	0.056	0.084	0.084	0.084	0.084	0.084	0.084
(Number of Pro-punishment Sexual Harassment Cases / Docket Size) * Female	4.583**	4.392**		6.310**	6.518**		9.729**	11.75**		11.75**	14.81**	
	(1.241)	(1.231)		(1.622)	(1.565)		(2.930)	(2.959)		(3.819)	(3.215)	
(Number of Pro-punishment Gender Discrimination Cases / Docket Size) * Female	-0.953		0.470	0.932		5.606	9.971**		13.04**	15.07*		25.89**
	(1.504)		(1.888)	(2.803)		(3.749)	(3.222)		(3.355)	(6.197)		(6.278)
N	1541695	1541695	1541695	1541695	1541695	1541695	1886023	1886023	1886023	1886023	1886023	1886023
R-sq	0.056	0.056	0.056	0.056	0.056	0.056	0.084	0.084	0.084	0.084	0.084	0.084
	Hours						*Earnings*					
Number of Pro-punishment Sexual Harassment Cases * Female	-0.0146	-0.00934		0.00192	0.00380		0.00519	0.00651		0.00413	0.00604	
	(0.0366)	(0.0359)		(0.0478)	(0.0412)		(0.00591)	(0.00557)		(0.00757)	(0.00620)	
Number of Pro-punishment Gender Discrimination Cases * Female	0.0234		0.0179	0.0143		0.0263	0.00582		0.00778	0.0101		0.0172
	(0.0512)		(0.0499)	(0.0884)		(0.0740)	(0.00807)		(0.00762)	(0.0127)		(0.0106)
N	1835070	1835070	1835070	1835070	1835070	1835070	1734500	1734500	1734500	1734500	1734500	1734500
R-sq	0.112	0.112	0.112	0.112	0.112	0.112	0.115	0.115	0.115	0.115	0.115	0.115
(Number of Pro-punishment Sexual Harassment Cases / Docket Size) * Female	-47.35	-3.372		99.33	141.2		35.00+	46.04*		45.13+	60.42**	
	(114.5)	(117.6)		(149.9)	(141.2)		(18.76)	(18.98)		(23.27)	(20.34)	
(Number of Pro-punishment Gender Discrimination Cases / Docket Size) * Female	216.9		202.0	201.8		330.5	54.13*		65.22**	75.30*		122.7**
	(137.1)		(140.2)	(244.7)		(234.1)	(21.11)		(20.83)	(37.82)		(36.09)
N	1835070	1835070	1835070	1835070	1835070	1835070	1734500	1734500	1734500	1734500	1734500	1734500
R-sq	0.112	0.112	0.112	0.112	0.112	0.112	0.115	0.115	0.115	0.115	0.115	0.115

Notes: All regressions use MORG CPS and include those not in the labor force. Outcome variables are described as before. Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for age, sex, race, educational attainment, marital status, circuit and year-fixed effects, as well as the fraction of female judges, the fraction of Democratic appointees, and, when the endogenous variable is not a ratio, docket size. The circuit-by-year controls are lagged and interacted with the female dummy in the same way that the endogenous variable and instruments are. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. The number of observations and R-square are displayed only for the IV regression with both instruments. Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions.

+ Significant at 10%; * Significant at 5%; ** Significant at 1%

Appendix Table A: Summary Statistics

	Case Type		
Circuit Characteristics	Sexual Harassment, 1982-2002	Damages, 1982-2002	Gender Discrimination, 1995-2002
Total Judges in Circuit-Year	18.47	18.50	19.95
	(0.461)	(0.463)	(0.782)
Number of Female Judges in Circuit-Year	1.937	2.020	3.042
	(0.106)	(0.110)	(0.194)
Number of Democratic Appointee Judges in	7.722	7.849	8.646
Circuit-Year	(0.273)	(0.266)	(0.502)
Total Panels of Case Type	0.913	0.357	5.510
	(0.0907)	(0.0470)	(0.392)
Panels of Case Type with Female Judges	0.298	0.131	1.927
	(0.0387)	(0.0260)	(0.197)
Panels of Case Type with Democratic Appointee	0.722	0.298	4.135
Judges	(0.0712)	(0.0422)	(0.304)
Docket Size	3611.5	3612.0	4454.7
	(123.3)	(123.3)	(239.3)
Number of Pro-punishment Cases	0.599	0.290	2.062
	(0.0701)	(0.0428)	(0.165)
Number of Pro-punishment Cases / Docket Size	0.000184	0.0000879	0.000577
	(0.0000225)	(0.0000143)	(0.0000587)
Damages Awarded in 10,000s		4.782	
		(1.076)	
N (circuit-years)	252	252	96
Individual Outcomes			
Log Real Weekly Earnings - Female	3.654		
	(0.00177)		
Log Real Weekly Earnings - Male	4.910		
	(0.00177)		
Hours Worked - Female	22.78		
	(0.0122)		
Hours Worked - Male	34.33		
	(0.0131)		
Employment Status (Part-time/Full) - Female	0.646		
	(0.000285)		
Employment Status (Part-time/Full) - Male	0.813		
	(0.000242)		
Management - Female	0.110		
	(0.000217)		
Management - Male	0.135		
	(0.000228)		
N	5,998,268		

Notes: This data comes from MORG CPS, which we treat as a repeated cross-section with 5,998,268 observations after restricting to individuals between the ages of 18 and 65. A pro-punishment damages case is a decision that allows any damages. Damages awarded are the jury total damages if final or adjusted final total damage values do not exist. Damages are considered 0 if reversed on appeal. Damages are adjusted to real prices in 2000. Coefficients displayed are from an OLS regression on a constant.

Appendix Table B
Randomization Check

Case Characteristics	*Panel with Female*	*Panel with Democrat*
Direction of Lower Court Decision	-0.030	-0.044
	(0.047)	(0.051)
P claims employer acted in retaliation	0.040	-0.064
	(0.052)	(0.056)
All plaintiffs are female	-0.014	-0.059
	(0.041)	(0.044)
Title IX claim	-0.001	0.019
	(0.014)	(0.015)
Section 1983 claim	-0.034	0.015
	(0.027)	(0.030)
Constructive discharge from employment	0.035	-0.021
	(0.031)	(0.034)
Procedural issues dominate	-0.014	0.011
	(0.033)	(0.035)
P suing under state law	-0.028	0.039
	(0.044)	(0.048)
P claims illegally denied promotion	-0.021	-0.024
	(0.042)	(0.046)
P claims illegally not being hired	0.006	-0.017
	(0.030)	(0.032)
P claims illegally fired	0.027	0.038
	(0.053)	(0.058)
P claims unequal pay	-0.013	-0.064
	(0.037)	(0.040)
P sued under 14th Amendment	-0.072**	0.011
	(0.023)	(0.025)
P sued under 1st Amendment	-0.035+	-0.000
	(0.019)	(0.021)
Damages major point of contention	0.054	0.012
	(0.037)	(0.040)
Contains Section 1981 claim	0.031	0.008
	(0.032)	(0.034)
Contains age discrimination claim	-0.061	-0.021
	(0.039)	(0.042)
Contains pregnancy discrimination claim	0.019	-0.011
	(0.027)	(0.029)
Contains emotional distress claim	0.029	-0.013
	(0.028)	(0.031)
P not victim of harassment	0.103	0.112
	(0.111)	(0.106)
P is appellant	-0.092	0.017
	(0.160)	(0.154)

Notes: Heteroskedasticity-robust standard errors in parentheses. Each row in column 1 is a regression of a distinct case characteristic on a dummy equal to one when at least one female is on the panel. Each row in column 2 is a regression of the same outcome variable on a dummy equal to one when the panel includes at least one Democratic appointee. All regressions include circuit and year-fixed effects, as well as the fraction of female judges in the circuit (for column 1) and the fraction of Democratic appointee judges in the circuit (for column 2). "P" refers to plaintiff.

+ Significant at 10%; * Significant at 5%; ** Significant at 1%

Appendix Table C -- First Stage: Relationship Between Sexual Harassment Damages and the Number of Sexual Harassment Damage Cases with Female and Democratic Appointee Judges, 1982-2002

	(1) Damages	(2) Log Damages	(3) Number of Cases with Any Damages	(4) Number of Cases with Keep or Increase Damages	(5) Damages	(6) Log Damages	(7) Number of Cases with Any Damages / Docket Size	(8) Number of Cases with Keep or Increase Damages / Docket Size
Sexual Harassment Damage Cases with Female Judges	2.254	1.686	0.0658	-0.0657				
	(5.108)	(1.091)	(0.135)	(0.120)				
Sexual Harassment Damage Cases with Democratic Appointee Judges	13.88**	3.600**	0.774**	0.538**				
	(2.226)	(0.542)	(0.0791)	(0.0737)				
Sexual Harassment Damage Cases with Female Judges / Docket Size					-2858.4	3604.6	0.113	0.0125
					(14033.9)	(3197.7)	(0.120)	(0.121)
Sexual Harassment Damage Cases with Democratic Appointee Judges / Docket Size					45985.1**	12164.8**	0.825**	0.569**
					(8947.5)	(2033.8)	(0.0863)	(0.0812)
F-statistic testing joint significance of instruments	20.87	42.03	71.54	41.00	15.62	39.86	101.01	54.76
(p-value)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	5429470	5429470	5429470	5429470	5429470	5429470	5429470	5429470
R-sq	0.419	0.611	0.804	0.673	0.365	0.561	0.830	0.715

Notes: Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for circuit fixed-effects, year fixed-effects, the fraction of female judges in a given circuit-year, the fraction of Democratic appointees in a given circuit-year, and, in Columns 1-4, Docket Size. Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment damage decisions. Columns 3-4 are the number of appellate decisions with any damages and the number of appellate decisions that keep or increase damages; Columns 7-8 are the same except the number of decisions is normalized by docket size. First stage regressions using only female or only Democratic appointees display strong positive relationships as in earlier tables. Damages awarded are the jury total damages if final or adjusted final total damage values do not exist. Damages are considered 0 if reversed on appeal. Damages are adjusted for inflation. Log Damages are the natural logarithm of (1 + sum of damages in circuit-year adjusted to real prices in 2000).

Appendix Table D: The Effects of Sexual Harassment Damages, 1982-2002

	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Panel 1	Earnings				Hours			
Damages * Female	0.00227*	0.00790**	0.00144+	0.00233	0.0108*	0.0351**	0.00624+	0.00569
	(0.00111)	(0.00181)	(0.000796)	(0.00149)	(0.00514)	(0.00926)	(0.00377)	(0.00784)
Damages$_{t-1}$ * Female			0.00182**	0.00334*			0.00983**	0.0157*
			(0.000564)	(0.00140)			(0.00247)	(0.00737)
Damages$_{t-2}$ * Female			0.00263**	0.00483**			0.0155**	0.0283**
			(0.000703)	(0.00144)			(0.00341)	(0.00816)
F-statistic testing joint significance of lags			9.86	20.65			13.42	14.78
(p-value)			(0.000)	(0.000)			(0.000)	(0.000)
N	4943024	4943024	4426490	4426490	5223699	5223699	4678751	4678751
R-sq	0.132	0.131	0.129	0.129	0.134	0.133	0.130	0.130
Panel 2	Any Employment				Management			
Damages * Female	0.000300	0.00123**	0.000170	0.000333	0.000120**	0.000408**	0.0000875**	0.000189*
	(0.000194)	(0.000288)	(0.000151)	(0.000254)	(0.0000463)	(0.0000943)	(0.0000323)	(0.0000923)
Damages$_{t-1}$ * Female			0.000247*	0.000551*			0.0000635*	0.000205**
			(0.000107)	(0.000224)			(0.0000314)	(0.0000723)
Damages$_{t-2}$ * Female			0.000381**	0.000771**			0.000129**	0.000190*
			(0.000124)	(0.000231)			(0.0000378)	(0.0000833)
F-statistic testing joint significance of lags			6.17	24.21			10.04	19.49
(p-value)			(0.001)	(0.000)			(0.000)	(0.000)
N	5398935	5398935	4832139	4832139	4334364	4334364	4092008	4092008
R-sq	0.098	0.098	0.096	0.096	0.059	0.059	0.059	0.059
Panel 3	Earnings				Hours			
Number of Cases w/ Any Damages	0.117**	0.136**	0.0531**	0.0549**	0.504**	0.608**	0.150	0.172
* Female	(0.0173)	(0.0233)	(0.0156)	(0.0202)	(0.101)	(0.137)	(0.1000)	(0.126)
Number of Cases w/ Any Damages$_{t-1}$			0.0530**	0.0480*			0.280**	0.244*
* Female			(0.0155)	(0.0196)			(0.0955)	(0.118)
Number of Cases w/ Any Damages$_{t-2}$			0.0567**	0.0714**			0.342**	0.421*
* Female			(0.0177)	(0.0274)			(0.111)	(0.163)
F-statistic testing joint significance of lags			22.13	18.67			15.73	12.57
(p-value)			(0.000)	(0.000)			(0.000)	(0.000)
N	4943024	4943024	4426490	4426490	5223699	5223699	4678751	4678751
R-sq	0.132	0.132	0.129	0.129	0.134	0.134	0.130	0.130
Panel 4	Any Employment				Management			
Number of Cases w/ Any Damages	0.0179**	0.0213**	0.00783**	0.00809*	0.00601**	0.00687**	0.00311**	0.00348**
* Female	(0.00262)	(0.00360)	(0.00231)	(0.00320)	(0.000876)	(0.00116)	(0.00103)	(0.00119)
Number of Cases w/ Any Damages$_{t-1}$			0.00834**	0.00818**			0.00252**	0.00262*
* Female			(0.00222)	(0.00309)			(0.000846)	(0.00105)
Number of Cases w/ Any Damages$_{t-2}$			0.00883**	0.0113**			0.00282**	0.00314*
* Female			(0.00259)	(0.00408)			(0.000934)	(0.00141)
F-statistic testing joint significance of lags			25.86	24.87			21.91	19.72
(p-value)			(0.000)	(0.000)			(0.000)	(0.000)
N	5398935	5398935	4832139	4832139	4334364	4334364	4092008	4092008
R-sq	0.098	0.098	0.096	0.096	0.059	0.059	0.059	0.059

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for age, sex, race, educational attainment, marital status, circuit and year-fixed effects, as well as the fraction of female judges, the fraction of Democratic appointees, and docket size. The circuit-by-year controls are lagged and interacted with the female dummy in the same way that the endogenous variable and instruments are. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment damage decisions. Damages awarded are the jury total damages if final or adjusted final total damage values do not exist. Damages are considered 0 if reversed on appeal. Individual outcomes are defined in Tables 2-5.

+ Significant at 10%; * Significant at 5%; ** Significant at 1%

Appendix Table E: A Horse Race Between Sexual Harassment Damages and Law, 1982-2002

	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Panel 1	Earnings		Hours		Any Employment		Management	
Damages * Female	0.000544	0.00144	0.00336	0.00108	0.00000238	0.000119	0.0000308	0.000117
	(0.000695)	(0.00172)	(0.00363)	(0.00957)	(0.000114)	(0.000270)	(0.0000223)	(0.0000868)
Number of Pro-punishment Cases	0.0714**	0.0937**	0.305**	0.494**	0.0123**	0.0160**	0.00365**	0.00396**
* Female	(0.0145)	(0.0219)	(0.0700)	(0.110)	(0.00215)	(0.00327)	(0.000511)	(0.000790)
N	4943024	4943024	5223699	5223699	5398935	5398935	4334364	4334364
R-sq	0.132	0.132	0.134	0.134	0.098	0.098	0.059	0.059
Panel 2								
Number of Cases w/ Any Damages	0.0558*	0.0293	0.223	0.0190	0.00648+	0.00265	0.00279*	0.00294
* Female	(0.0234)	(0.0342)	(0.150)	(0.212)	(0.00345)	(0.00546)	(0.00130)	(0.00182)
Number of Pro-punishment Cases	0.0505**	0.0891**	0.230*	0.491**	0.00937**	0.0155**	0.00262**	0.00331**
* Female	(0.0172)	(0.0244)	(0.0946)	(0.132)	(0.00247)	(0.00369)	(0.000727)	(0.000947)
N	4943024	4943024	5223699	5223699	5398935	5398935	4334364	4334364
R-sq	0.132	0.132	0.134	0.134	0.098	0.098	0.059	0.059
Panel 3								
Log Damages * Female	0.00929**	0.00445	0.0450*	0.000493	0.00113*	0.000341	0.000411**	0.000525+
	(0.00340)	(0.00551)	(0.0191)	(0.0335)	(0.000536)	(0.000902)	(0.000156)	(0.000290)
Number of Pro-punishment Cases	0.0542**	0.0920**	0.227**	0.499**	0.00970**	0.0160**	0.00294**	0.00342**
* Female	(0.0136)	(0.0235)	(0.0700)	(0.121)	(0.00200)	(0.00355)	(0.000519)	(0.000862)
N	4943024	4943024	5223699	5223699	5398935	5398935	4334364	4334364
R-sq	0.132	0.132	0.134	0.134	0.098	0.098	0.059	0.059
Panel 4 (1995-2002)								
Damages * Female	0.0000526	0.00224	-0.000968	0.000244	-0.0000190	0.000200	-0.0000449	-0.000140
	(0.000733)	(0.00139)	(0.00458)	(0.0101)	(0.000107)	(0.000227)	(0.0000447)	(0.0000914)
Number of Pro-punishment Cases	0.00626	-0.00641	-0.00404	-0.00247	0.00210*	0.00121	0.00167**	0.00293**
* Female	(0.00655)	(0.01000)	(0.0428)	(0.0694)	(0.000933)	(0.00151)	(0.000467)	(0.000663)
N	1734500	1734500	1835070	1835070	1886023	1886023	1541695	1541695
R-sq	0.115	0.115	0.112	0.112	0.084	0.084	0.056	0.056
Panel 5 (1995-2002)								
Number of Cases w/ Any Damages	0.00344	0.0178	-0.156	-0.117	-0.000651	-0.0000159	0.000338	-0.00173
* Female	(0.0191)	(0.0261)	(0.127)	(0.189)	(0.00309)	(0.00416)	(0.00126)	(0.00169)
Number of Pro-punishment Cases	0.00556	0.000435	0.0356	0.0384	0.00218+	0.00236	0.00133*	0.00270**
* Female	(0.00822)	(0.0101)	(0.0522)	(0.0748)	(0.00125)	(0.00154)	(0.000533)	(0.000674)
N	1734500	1734500	1835070	1835070	1886023	1886023	1541695	1541695
R-sq	0.115	0.115	0.112	0.112	0.084	0.084	0.056	0.056

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for age, sex, race, educational attainment, marital status, circuit and year-fixed effects, as well as the fraction of female judges, the fraction of Democratic appointees, and docket size. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment damage decisions. Damages awarded are the jury total damages if final or adjusted final total damage values do not exist. Damages are considered 0 if reversed on appeal. Panels 1-3 compare damages cases with our collection of sexual harassment cases, 1982-2002. Panels 4-5 compare damages cases with CJP collection of sexual harassment cases, 1995-2002. Individual outcomes are defined in Tables 2-5.

+ Significant at 10%; * Significant at 5%; ** Significant at 1%

Appendix Table F: The Effects of Sexual Harassment Doctrine, 1982-2002

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Panel 1	Earnings								Hours							
Meritor * Female	-0.684**	-0.0301**					-0.718**	-0.719**	-6.082**	-0.244					-6.444**	-6.760**
	(0.0585)	(0.00854)					(0.0666)	(0.0669)	(0.292)	(0.149)					(0.395)	(0.402)
Meritor$_{t-1}$ * Female		-0.0459**								-0.850**						
		(0.000898)								(0.00411)						
Meritor$_{t-2}$ * Female		-0.516**								-4.382**						
		(0.0188)								(0.114)						
Harris * Female			0.254**	0.216**			0.158**	0.118*			1.164**	0.978			0.648*	0.335
			(0.0489)	(0.0801)			(0.0519)	(0.0525)			(0.292)	(0.604)			(0.311)	(0.323)
Harris$_{t-1}$ * Female				0.0288								-0.0653				
				(0.0907)								(0.799)				
Harris$_{t-2}$ * Female				0.0458								0.556				
				(0.0653)								(0.610)				
Faragher * Female					0.0971**	0.0664	0.0638*	0.0405					0.497*	0.197	0.281	0.181
					(0.0341)	(0.0456)	(0.0299)	(0.0265)					(0.213)	(0.355)	(0.177)	(0.160)
Faragher$_{t-1}$ * Female						-0.0374								-0.141		
						(0.0507)								(0.472)		
Faragher$_{t-2}$ * Female						0.0392								0.317		
						(0.0423)								(0.401)		
Number of Pro-punishment Cases								0.0266*								0.104
* Female								(0.0131)								(0.0774)
Number of Cases w/ Any Damages								0.0282								0.0892
* Female								(0.0179)								(0.125)
N	4943024	4426490	4943024	4426490	4943024	4426490	4943024	4943024	5223699	4678751	5223699	4678751	5223699	4678751	5223699	5223699
R-sq	0.132	0.129	0.132	0.129	0.132	0.129	0.132	0.132	0.134	0.130	0.134	0.130	0.134	0.130	0.134	0.134
Panel 2	Any Employment								Management							
Meritor * Female	-0.0942**	-0.00769**					-0.105**	-0.0972**	-0.00101	-0.000820					-0.0130*	-0.0176**
	(0.00855)	(0.00155)					(0.00971)	(0.00963)	(0.00607)	(0.00873)					(0.00591)	(0.00579)
Meritor$_{t-1}$ * Female		-0.0121**								0.00767**						
		(0.000113)								0.0000370						
Meritor$_{t-2}$ * Female		-0.0605**								-0.00697**						
		(0.00264)								(0.000980)						
Harris * Female			0.0366**	0.0210**			0.0268**	0.0248**			0.0124**	0.00844**			0.0131**	0.00861**
			(0.00597)	(0.00531)			(0.00650)	(0.00645)			(0.00235)	(0.00296)			(0.00201)	(0.00196)
Harris$_{t-1}$ * Female				0.0146**								0.000910				
				(0.00263)								(0.00217)				
Harris$_{t-2}$ * Female				0.00670*								0.00470**				
				(0.00338)								(0.000990)				
Faragher * Female					0.0189**	0.0168**	0.0149**	0.0114**					0.00781**	0.00454	0.00640**	0.00510**
					(0.00454)	(0.00556)	(0.00403)	(0.00368)					(0.00199)	(0.00524)	(0.00153)	(0.00132)
Faragher$_{t-1}$ * Female						-0.00259								0.00270		
						(0.00626)								(0.00625)		
Faragher$_{t-2}$ * Female						0.00147								-0.000155		
						(0.00544)								(0.00412)		
Number of Pro-punishment Cases								0.00516**								0.00121+
* Female								(0.00190)								(0.000628)
Number of Cases w/ Any Damages								0.00226								0.00167
* Female								(0.00263)								(0.00117)
N	5398935	4832139	5398935	4832139	5398935	4832139	5398935	5398935	4334364	4092008	4334364	4092008	4334364	4092008	4334364	4334364
R-sq	0.099	0.096	0.099	0.096	0.098	0.096	0.099	0.099	0.059	0.059	0.059	0.059	0.059	0.059	0.059	0.059

Notes: All regressions use MORG CPS and are Ordinary Least Squares. Heteroskedasticity-robust standard errors in parentheses. Observations are clustered at the circuit-year level. These regressions control for age, sex, race, educational attainment, marital status, circuit and year-fixed effects, as well as the fraction of female judges, the fraction of Democratic appointees, and docket size. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment damages decisions. Major Supreme Court decisions, Meritor, Harris, and Faragher, are coded as 1 for all circuits in the years during and following the decision and coded as 0 or 1 depending on the circuit split previous to the decision. Additionally, we include a dummy indicating whether the circuit is mentioned as being part of a circuit split. The dummy is coded as 1 if either the Supreme Court decision or the Westlaw direct history of the decision contains information about the circuit's position. Individual outcomes are defined in Tables 2-5.

+ Significant at 10%; * Significant at 5%; ** Significant at 1%