UC Berkeley
Law and Economics Workshop

Title

Insiders and Outsiders: Does Forbidding Sexual Harassment Exacerbate Gender Inequality?

Permalink

https://escholarship.org/uc/item/24z191b3

Author

Chen, Daniel L.

Publication Date

2010-06-01

Insiders and Outsiders:
Does Forbidding Sexual Harassment Exacerbate Gender Inequality?

Daniel L. Chen and Jasmin Sethi[1]

June 2010

ABSTRACT

This paper tests an insider-outsider model of harassment and involuntary unemployment. We exploit random assignment of appellate judges to three-judge panels and the fact that a judge's gender and party of appointment predict outcomes in sexual harassment litigation to demonstrate a causal relationship between appellate decisions creating precedent in sexual harassment law and subsequent labor market outcomes. Consistent with an insider-outsider model of involuntary unemployment, forbidding sexual harassment encouraged entry of outsiders and reduced gender inequality along the dimensions of quantity, price, and quality, but these ameliorative effects on gender inequality are reduced for women previously in the labor force.

Keywords: Sexual Harassment, Gender Discrimination, Inequality   **JEL codes:** J81, K31, J31, J71

I. Introduction

Sexual harassment is claimed to be a major impediment to female labor force participation in both developed and developing countries.[2] Making the work environment friendlier to women has been one of the more dramatic labor market changes in the past half-century. Forbidding sexual harassment may have ameliorated gender inequality by opening up job opportunities in previously harassing work environments. Since forbidding sexual harassment may have been a consequence of the increase in female labor force participation, establishing a causal relationship between sexual harassment law and labor market outcomes is challenging. Social scientists have long speculated on the relationship between court-made law and socio-economic conditions; methods to evaluate the nature of these relationships may help judges who are interested in the broader empirical consequences of their decisions. As Judge Richard Posner writes, "[judicial] opinions lack the empirical support that is crucial to sound constitutional adjudication,"[3] and as Justice Breyer writes, "I believe that a different interpretive approach that undervalues consequences, by undervaluing related constitutional objectives, exacts a constitutional price that

---

[2] http://www.nytimes.com/2009/09/16/world/asia/16ladies.html

[3] "I would like to see an entirely different kind of constitutional theorizing. It would set itself the difficult-although, from the perspective of today's theorists, the intellectually modest-task of exploring the operation and consequences of constitutionalism. It would ask such questions as, what difference has it made for press freedom and police practices in the United States compared to England that we have a judicially enforceable Bill of Rights and England does not? How influenced are judges in constitutional cases by public opinion? How influenced is public opinion by constitutional decisions? … Does the Court try to prevent the formation of interest groups that might obtain constitutional amendments that would curtail the Court's power or abrogate some of its doctrines, or to encourage the formation of interest groups that will defend the Court's prerogatives? And what role do interest groups play in constitution-making and -amending? … In the reception of Court decisions by the media and through the media the public? *Above all, what are the actual and likely effects of particular decisions and doctrines?* Did Brown v. Board of Education improve the education of blacks? Did Roe v. Wade retard abortion law reform at the state level? What effect have the apportionment cases had on public policy? Did the Warren Court's decisions expanding the constitutional rights of criminal defendants contribute to the increase in the crime rate in the 1960s and 1970s and provoke a legislative backlash, increasing the severity of sentences? These questions have not been entirely ignored, but the literature on them is meager, and law professors have contributed very little to it. Exploring these questions would be a more fruitful use of academic time and brains than continuing the 200-hundred-year-old game of political rhetoricizing that we call constitutional theory. Some of these questions might actually be answerable, and the answers would alter constitutional practice more than theorizing has done or can do. Thus I am in radical disagreement with Dworkin, who insists that cases in which facts or consequences matter to sound constitutional decisionmaking are "rare"." (emphasis added) (Posner 1998).

is too high."[4]

This paper uses a natural experiment provided by the random assignment of appellate judges to three-judge panels and the fact that a judge's gender and party of appointment predict decisions in sexual harassment cases to estimate the causal impact of forbidding sexual harassment on gender inequality. Sexual harassment law is primarily court-made. Between 1982 and 2002, over 250 appellate cases addressing sexual harassment were decided in the United States. Our empirical strategy exploits the fact that judges are randomly assigned to their cases within each circuit, and the fact that Democratic appointees, particularly male Democratic appointees, are more likely to decide in favor of sexual harassment plaintiffs and Republican appointees, particularly female Republican appointees, are more likely to decide in favor of sexual harassment defendants. Because judicial composition of sexual harassment appellate panels is unlikely to be correlated with subsequent labor market outcomes other than through sexual harassment decisions, the random assignment of female Republican appointees or male Democratic appointees to three-judge panels creates exogenous variation in appellate precedent forbidding sexual harassment that can be used to estimate the causal impact of court-made sexual harassment law on gender inequality.[5]

---

[4] "Since law is connected to life, judges, in applying a text in light of its purpose, should look to consequences, including contemporary conditions, social, industrial and political, of the community to be affected.", "Why should courts try to answer difficult federalism questions on the basis of logical deduction from text or precedent alone? Why not ask about the consequences of decision-making on the active liberty that federalism seeks to further? Why not at least consider the practical effects on local democratic self-government of decisions interpreting the constitution's principles of federalism—principles that themselves seek to further that very kind of government?", "I have urged attention to purpose and consequences. [My argument] identifies consequences as an important yardstick to measure a given interpretation's faithfulness to these democratic purposes. In short, focus on purpose seeks to promote active liberty by insisting on interpretations, statutory as well as constitutional, that are consistent with the people's will. Focus on consequences, in turn, allows us to gauge whether and to what extent we have succeeded in facilitating workable outcomes which reflect that will.", "I believe that when a judge candidly acknowledges that in addition to text, history, and precedent, consequences also guide his decision-making, he is more likely to be disciplined in emphasizing, for example, constitutionally relevant consequences rather than allowing his own subjectively held values to be outcome determinative. In all these ways, I think a focus on consequences will itself constrain subjectivity." (Breyer 2004).
[5] This is true as long as we control for the composition of the circuit pool of judges available to be assigned.

In the following sections, we present an analysis of data from the Current Population Survey and data on sexual harassment appellate decisions collected by the authors as well as by other authors. Section 2 discusses theoretical and practical aspects of sexual harassment law, emphasizing how labor lawyers and human resources consultants greatly exaggerated the risk of sexual harassment lawsuits after major appellate decisions to motivate employers to make human resources policy changes. Insiders would no longer be able to harass outsiders in order to capture economic rents. Outsider women would enter the labor force since they can now compete for jobs previously dominated by insider men and women who tolerated sexual harassment.

Section 3 establishes that composition of the judicial panel is indeed related to sexual harassment appellate decisions. Republican appointees, particularly female Republican appointees, are more likely to vote in favor of sexual harassment *defendants* while Democratic appointees, particularly male Democratic appointees, are more likely to vote in favor of sexual harassment *plaintiffs*. Thus, the less frequently female Republican appointees are assigned and the more frequently male Democratic appointees are assigned, the more pro-plaintiff will be sexual harassment precedent, making it easier for subsequent sexual harassment plaintiffs to bring and win suit.

A variety of evidence presented in Section 3 establishes that the random assignment of judges differentially impacts appellate decisions creating precedent in sexual harassment law. In Section 4, we consider whether females living in circuits and years with more precedent favorable to sexual harassment plaintiffs see improvements in labor market outcomes relative to males. Two-stage least squares estimates using this variation suggest that forbidding sexual harassment increase employment status, hours worked, earnings, and managerial status for

females and decrease them, to a lesser extent, for males.

We also look at the effect of highly cited cases and contrast them with infrequently cited cases. Since many circuit-years did not experience new precedents in sexual harassment law, we restrict our attention to circuit-years that had at least one sexual harassment appellate decision. Pro-plaintiff sexual harassment decisions in circuits and years that are highly cited increase female labor market outcomes by up to three times more than do the pro-plaintiff sexual harassment decisions in circuits and years that are infrequently cited. These results suggest that sexual harassment appellate decisions actually creating precedent are the decisions that ameliorated gender inequality.

We then estimate several models making different assumptions about the longevity of the effect of sexual harassment precedent. We find that pro-plaintiff appellate decisions ameliorate gender inequality, primarily two or three years after the appellate decisions are made. Furthermore, we find that gender inequality is not related to appellate decisions before they are made, a finding that supports the validity of our empirical design. In addition, pro-*defendant* sexual harassment appellate decisions decrease female labor market outcomes. These results are robust to modeling sexual harassment precedent as having a cumulative effect and hold even after controlling for the number of sexual harassment appellate decisions, despite the fact that the number of sexual harassment appeals may be affected by previous years' pro-plaintiff decisions, which could bias the estimates towards zero. As a falsification exercise, we find that sexual harassment law does not affect race inequality. Taken together, these results are further evidence that sexual harassment law affects inequality and are inconsistent with theories of legal precedent that considers law only as a product as opposed to producer of societal trends.

In Section 5, we present the evidence that is consistent with an insider-outsider model of

harassment and involuntary unemployment: forbidding sexual harassment encouraged the entry of outsiders and had smaller beneficial effects for insider women previously in the labor force. First, sexual harassment law reduced gender inequality for the overall population, but when the sample is restricted to those in the labor force, the effects are smaller or even reversed. Second, using worker characteristics to predict high labor force attachment, women who "should" be in the labor force are more positively affected by sexual harassment law than women who "should not" be in the labor force. Third, we decompose the effect of sexual harassment law on the gender wage gap by controlling for other labor force outcomes. While the increase in managerial status and hours worked explain a sizeable fraction of the change in the wage gap, the change in gender wage gap disappears when controlling for employment status or when restricting the sample to individuals reporting non-zero wages.

In Section 6, we explore the channel through which sexual harassment law has its labor market effects. To see whether sexual harassment law as an interpretation of Title VII introduced by feminist legal scholars (MacKinnon 1979; Schultz 1998) had practical ramifications distinct from those of gender discrimination law, we compare the effects of sexual harassment precedent with gender discrimination precedent. The effects of sexual harassment law change little when controlling for gender discrimination law. Moreover, gender discrimination law appears to have had little effect, suggesting that harassment, not simply discrimination, is an important mechanism in involuntary unemployment.

Importantly, the effect of damages awarded in sexual harassment suits essentially disappears when controlling for the number of pro-plaintiff decisions. This suggests a practical implication for civil rights lawyers and labor lawyers, if they want to have the largest social impact, they might not stop at settlement but push for a pro-plaintiff decision on the books, since

if published damages do not have an effect, unpublished settlement also might not have an effect.

Finally, the effect of sexual harassment law is primarily observed among large firms with at least 25 employees, consistent with the legal and ethnographic research on labor lawyers and human resources consultants translating appellate decisions to human resources policy changes for firms of sufficient size. The effects are also somewhat larger in blue-collar industries.

Observing the insider-outsider model of involuntary unemployment empirically is difficult. Previous tests of the insider-outsider model use cross-sectional analyses or focus on the correlation between firm productivity, labor turnover costs, wages and lay-offs rather than on the causal consequences of requiring insiders to cooperate with outsiders (for a summary of the empirical research, see Lindbeck and Snower 2001). Our findings supporting the insider-outsider model of harassment and involuntary unemployment combined with the existing survey evidence indicating higher job satisfaction resulting from the presence of sexual harassment law (Baker, Jackson, and Newman 2003) suggest that other prohibitions on identity-based (Akerlof and Kranton 2000) harassment may be welfare-improving for targeted groups with respect to their economic consequences.

II. Background

At a practical level, sexual harassment law, which is primarily court-made, and the imposition of direct and indirect litigation costs has impacted firm behavior in at least four 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 complaints 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 akin to 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 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).

Sociologists and legal ethnographers (e.g., Dobbin and Kelly 2007, Edelman 1992, 2002, Bisom-Rapp 2001a, 2001b, Edelman, Abraham, and Erlanger 1992, Edelman, Uggen, and Erlanger 1999) have documented how labor lawyers and human resources consultants greatly exaggerated the risk of sexual harassment suit subsequent to major appellate decisions, in human resources management publications and in law review articles, such as reporting that 90% of women experienced sexual harassment, when later surveys found that only a small fraction actually did. Millions of dollars were spent on training programs and establishing grievance procedures aimed to reduce the risk of lawsuit more than to reduce the incidence of harassment. Rapid change in case law added to the uncertainty. Indeed, more grievance procedures were established than maternity leaves, even though the law mandated maternity leaves in no uncertain terms and grievance procedures were not part of a bright-line rule until 1998, when the Supreme Court said grievance procedures greatly reduced the liability faced by firms. Interviews of hundreds of businesses and firms, in a representative sample across the United States of business with at least 50 employees, uncovered, for example that firm size is not correlated with the timing of adoption of sexual harassment grievance procedures and training programs,

suggesting that these information consultants were quite effective in translating appellate decisions to human resources policy changes (Dobbin and Kelly 2007). Our own discussions with labor lawyers indicate how fearful employers are of Title VII suits and how many sexual harassment cases tend to be personal allegations. This could cause risk-averse employees to change behavior, in a way exceeding that of gender discrimination, ADA, or maternity mandate lawsuits, whose resolution can turn on evidence regarding disparate impact that is more likely to be statistical in nature.

Forbidding sexual harassment would have significant labor market consequences in an insider-outsider model of harassment and involuntary unemployment (Lindbeck and Snower 1988, Schultz 1990). In this model, while harassment is allowed, 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 this insider-outsider 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.

An alternative view suggest that sexual harassment law may have been a tax on the hiring of women, making it more costly to hire women. The law, like an unfunded mandated benefit, may act like a tax on labor demand (Summers 1989).[6] Similar to the unfunded mandate in ADA, all of the direct and indirect costs of sexual harassment law on firms - from establishing internal infrastructures conducive to complaint to marginal costs associated with each female worker who has some probability of filing a complaint or becoming a litigant - may have been passed onto women and lowered their wages and employment 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 or through the offering of fringe benefits packages designed to appeal to some workers but not others.

Assuming that the effects of sexual harassment law were predominantly experienced by women, forbidding harassment may further lower female wages by increasing the supply of female labor by making it more pleasant for women who would be willing to work for lower wages in work environments that previously allowed harassment. If employees value the benefit at cost, the resulting equilibrium will result in the same level of employment but with the full

---

[6] These theoretical effects are described and analogized to those of accommodation mandates (Jolls 2000 and 2001). "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)

cost reflected in lower wages (Basu 2004). If there are wage rigidities, then the cost of the benefit cannot be reflected in wages and thus, unemployment may result (Summers 1989).

Whether forbidding sexual harassment exacerbates gender inequality is a priori ambiguous and recent empirical work has found that similar social policies regulating labor markets, such as the ADA and maternity mandates, had detrimental effects on the groups they were intended to protect (ADA: Jolls and Prescott 2004, Acemoglu and Angrist 2001; Employment protection: Autor, Donohue, and Schwab 2006; Maternity mandates: Gruber 1994). Sexual harassment law may have been a unique situation where the mandated benefit mechanisms are weakened since it may be difficult to know in advance who is going to be a sexual harassment plaintiff and men could bear some of the cost of the mandated benefit as potential harassers.

A methodologically related literature exploits the random assignment of judges to cases to identify the impact of judicial decisions on individual outcomes (Kling 2006; Chang and Schoar 2007). This paper also contributes to the literature on the gender wage gap (Blau and Kahn 2006; Card and DiNardo 2002; Weinberger and Kuhn 2006; O'Neill 2003; Black and Strahan 2001), anti-discrimination law (Basu 2003; Hellerstein, Neumark, and Troske 1997; Neumark and Stock 2006; Beller 1979; Eberts and Stone 1985; Acemoglu and Angrist 2001; Jolls and Prescott 2004; Chay 1998), and prejudice (Charles and Guryan 2007, Charles, Guryan, and Pan, 2010), and as such contributes to the debate on how much of gender inequality in labor markets is due to unobserved physiological differences, labor market choices, or discrimination (Summers 2005).

III. Design of Study

A. Data

Our empirical analysis draws on four sources of data on sexual harassment cases - three established datasets as well as our own data collection. We focus on published opinions since we are interested in the establishment of precedent. 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. 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. The second dataset is from Boyd, Epstein, and Martin (2010), 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. We restrict this data to the 90 federal appellate cases between 1982 and 2002. Our fourth dataset is 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. 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 251 cases.[7] Figure 1 plots the growth in sexual harassment cases, number of pro-plaintiff decisions, and number of pro-defendant decisions during this time period. Figure 2 plots the growth in total damages awarded in sexual harassment cases. When 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

---

[7] One reason our sample is smaller than the CJP dataset is that we restrict to cases brought by employees for sexual harassment in the workplace.

method. Figure 3 shows the variation in sexual harassment and gender discrimination cases over time.

We use the Merged Outgoing Rotation Groups (MORG) Current Population Survey (CPS) for information on individual employment outcomes, including weekly earnings, amount of time worked, employment status, and management status. (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, 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.) The MORG provides point-in-time measures of the variables of interest, 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 use the March sample of the Current Population Survey when examining issues regarding firm size and industry.

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 Judicial Center.[8] Figure 4 charts the growth in the probability of having a panel with a female judge as well as the rise and fall in the probability of having a panel with a Democratic appointee during the Reagan, Bush, and Clinton years. We also obtain a measure of annual circuit workload - the number of federal appeals terminated by fiscal year, which we also display in Figure 2 - from Federal Court Management Statistics.[9] Finally, we obtain data on the presence

---

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

of firm-level sexual harassment policies from Dobbin and Kelly (2007), a national sample of 389 workplaces interviewed in 1997 on the history of human resources practices dating back to 1965.

B. Identification Strategy and Specification

The correlation between court-made law and economic outcomes is generally difficult to interpret since the causality may run in both directions and the relationship may reflect omitted variables.[10] The basic specification considers changes in sexual harassment law measured with appellate decisions establishing precedent in different circuits and years and studies their effects on individuals living in those circuits over time. Consider the following linear specification for the effects of sexual harassment law on individual outcomes:

$$Outcome_{ict} = \beta_0 + \beta_1 X_{ict} + \beta_2 C_c + \beta_3 T_t + \beta_4 C_c \cdot Year_t + \beta_5 W_{ct}$$
$$+ \beta_6 W_{ct} \cdot Sex_{ict} + \beta_7 Law_{ct} + \beta_8 Law_{ct} \cdot Sex_{ict} + \beta_9 Sex_{ict} + \varepsilon^0_{ict} \tag{1}$$

where $Outcome_{itc}$ represents, in turn, employment status (no vs. part or full time employment), hours worked last week, log of the weekly real earnings, or management status (administrators, officials, public administration, other executive, or management related occupations) of individual i in circuit c and year t, Law_{ct} represents the number of pro-plaintiff sexual harassment appellate decisions in circuit c and year t[11], X_{ict} represents a set of individual controls (age, race, marital status, and educational attainment, which enter as dummies with the exception of age), C_c and T_t represent circuit and year fixed effects, C_c*Year_t represents circuit-specific time trends to allow different circuits to be on different trajectories with respect to outcomes, and W_{ct} represent characteristics of the pool of judges available to be assigned and docket size, the number of

---

[10] Momentous judicial decisions may be caused by rather than be causes of political or socioeconomic changes. For example, judges relied on personal values, heavily influenced by larger historical forces, in *Brown v. Board of Education* (Klarman 2004).

[11] With about one sexual harassment decision per circuit per year, we did not consider quadratic or non-monotonic functions of the number of pro-plaintiff decisions.

appellate terminations minus the number of sexual harassment decisions, in circuit c and year t. The key coefficient of interest is β_8 on the interaction of Law_{ct} and Sex_{ict}. Since employment outcomes are serially correlated, ε_{ict} is not *i.i.d.* Hence, all specifications cluster standard errors at the circuit level, although the results are basically the same when we cluster at the circuit-year level.

Since we are concerned that $Outcome_{itc}$ and Law_{ct} may be correlated for unobserved reasons, we exploit the fact that appellate judges are randomly assigned to three-judge panels and the fact that Democratic appointees, particularly male Democratic appointees, are more likely to decide in favor of sexual harassment plaintiffs and Republican appointees, particularly female Republican appointees, are more likely to decide in favor of sexual harassment defendants. A number of papers have documented the effect of judges' demographic background on sexual harassment cases (Farhang and Wawro 2004, Epstein 2007; Peresie 2005; Schultz and Petterson 1992 on employment discrimination cases; note that we are focusing on appellate precedent, not the filing of all cases—some papers find no effect of judicial background on civil rights and employment discrimination case outcomes in lower courts perhaps because lower court judges have less discretion in applying appellate precedent: Nielsen, Nelson, and Lancaster 2010; Ashenfelter, Eisenberg, and Schwab 1995). In different studies, having a female judge increased the probability of a pro-plaintiff decision in a three-judge panel from 22% to 41% and at the individual-level, Republican appointees vote for plaintiffs at a rate of 37% compared with Democratic appointees, who vote for plaintiffs at a rate of 52%. 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 appointment, clustering standard errors at the circuit level and controlling for circuit and year fixed effects and the probability of a panel being assigned a judge of the type under

consideration, shows that Democratic appointees are 11% more likely to vote in favor of sexual harassment plaintiffs while female judges are not more likely to vote in favor of sexual harassment plaintiffs and are somewhat less likely to vote pro-plaintiff when also controlling for party of appointment (Table 1 Panel A). This suggests that judges may be voting more along party lines than by gender (Dixon 2010). Indeed, female Republican appointees are 12% less likely to vote pro-plaintiff while male Democratic appointees are 11% more likely to vote pro-plaintiff. At the case level, having a female Republican appointee on a three-judge panel reduces the chances of a pro-plaintiff decision by 15%, but having two or more male Democratic appointees (one judge may influence colleagues' decisions, but two judges would win by the force of majority) increases the chances of a pro-plaintiff decision by 17% (Table 1 Panel B).

We can use the relationship between a judge's personal attributes and legal decision-making to instrument for changes in sexual harassment law in a circuit-year. The first stage regression is:

$$Law_{ct} = \varphi_0 + \varphi_1 Z_{ct} + \varphi_2 C_c + \varphi_3 T_t + \varphi_4 C_c Year_t + \varphi_5 W_{ct} + \eta_{ct} \qquad (2)$$

where Z_{ct} represents the instruments, the number of sexual harassment appellate panels with at least one female Republican appointee and the number of sexual harassment appellate panels with at least two male Democratic appointees in circuit c and year t. The identification assumption is $E(\varepsilon_{ict} Z_{ct}) = 0$. The most important threat to this assumption is that the instruments, panels with at least one female Republican appointee and panels with at least two male Democratic appointees, may be correlated with omitted factors that affect gender inequality in labor market outcomes. It is therefore important to control for composition of the judicial pool in each circuit as well as docket size to address unobserved factors influencing the quantity of litigation and how often judges with a particular demographic background will be assigned to

these cases. Figure 4 illustrates some of the intuition behind the identification strategy – the smooth lines show the probability a panel will be assigned a female judge and the probability a panel will be assigned a Democratic appointee, both of which are approximately equal to the proportion of the judicial pool that is female or a Democratic appointee. The jagged lines represent the actual proportion of panels that are assigned a female judge or Democratic appointee (the figure masks the greater variation between actual and expected proportions since it displays the average values across circuits). Controlling for the probability of a panel being assigned a particular type of judge, the actual proportion of panels with that type of judge is random. Our identification strategy exploits the deviation from the expected proportion of panels with a particular type of judge: the actual proportion of panels with a particular type of judge, conditional on the expectation, is correlated with the establishment of pro-plaintiff precedent but is uncorrelated with social trends and legal developments in other areas of law that would otherwise be correlated with the establishment of precedent.

Because we concerned about omitted factors that may be correlated with our instruments and with gender inequality, we interact gender with the legal institutions characteristics W_{ct} in equation (1). Since the number of sexual harassment panels may be positively correlated with both the number of pro-plaintiff sexual harassment decisions as well as the instruments, the number of sexual harassment panels with certain types of judges, but negatively correlated with gender inequality, we examine whether pro-defendant sexual harassment decisions have the opposite effect of pro-plaintiff sexual harassment decisions. Second, the number of sexual harassment panels may also be associated with trends in gender inequality, so we explore whether one year before the sexual harassment precedent displays differences in gender inequality. Third, the number of sexual harassment appeals may be influenced by legal

developments in other areas of law, such as gender discrimination, which may have independent effects on gender inequality, so we test for this using the number of pro-plaintiff gender discrimination cases. Fourth and fifth, we successively control for a dummy variable indicating whether there were any sexual harassment appellate panels and the number of sexual harassment appellate panels, in case docket size does not sufficiently control for omitted factors associated with both the quantity of litigation and gender inequality.

We should expect to see an effect if judges follow precedent, pro-plaintiff decisions in appellate courts on the margin make it easier for subsequent sexual harassment plaintiffs to bring and win suit (for an example of pro-plaintiff decisions, see the doctrine section of the Data Appendix for a list of major developments), and firms respond to appellate decisions. While the first two assumptions are less contested, the first bears further examination. Using a national sample of workplaces and their human resources policies (Dobbin and Kelly 2007), we find that of the 6 human resources policies related to sexual harassment, when significantly affected by sexual harassment law, they are positively affected, and of the 19 human resources policies not related to sexual harassment, when significantly affected, they are negatively affected (Appendix Table D). Using a Bonferroni adjustment to standard errors to account for the fact that these policy outcomes are among a family of hypotheses (Savin 1984, Duflo, Glennerster, Kremer 2007), only the presence of a training program about sexual harassment remains statistically significantly affected by pro-plaintiff sexual harassment decisions by 1 or 2 percentage points out of an average of 34% of establishments having sexual harassment training programs from 1982-1997. Using a distributed lag specification, pro-plaintiff decisions increase the presence of sexual harassment policies after two or three years while pro-defendant decisions decrease them by around 5 percentage points out of an average of 54% of establishments having sexual

harassment policies during this time period (Appendix Table E; the coefficients in Column 2 and the 95% confidence intervals are graphically displayed in Figure 9).

The basic idea behind the identification strategy can be illustrated in a series of figures. Figure 5 illustrates the first stage specification. Each circuit-year observation is a potential treatment or control, where treatment is defined as the existence of a panel with at least two male Democrat appointees and the non-existence of a panel with a female Republican appointee. The shading indicates the 95% confidence interval. Treatment has an effect on pro-plaintiff decisions in the current year but not before or after, consistent with the identification assumption. Figure 6 displays the raw data by circuit, plotting the number of pro-plaintiff decisions and wage inequality trends, simply calculated by subtracting the average male wage from the average female wage. Improvements in female outcomes generally come hand-in-hand with pro-plaintiff sexual harassment decisions. Figure 7 displays wage inequality and treatment, where treatment is defined as the non-existence of a panel with a female Republican appointee who are generally pro-defendant. The treatment and control dots 'pull' the wage inequality trend towards them, consistent with pro-plaintiff decisions improving female outcomes. The male Democratic appointee information is not displayed since this instrument has more power as the number of panels with male Democratic appointees rather than the existence of panels with male Democratic appointees. The regression of change in wage inequality on treatment or lagged treatment and the regression of wage inequality on treatment with year fixed effects are around 15% in statistical significance. The remainder of the paper corroborates the casual inspection of the raw data with regression analyses.

The results are also similar when we use state fixed effects instead of circuit fixed effects. This addresses the possibility that state laws or state interpretation of federal laws affect the way

firms respond to appellate precedents. In addition, to the extent our data miscodes pro-plaintiff and pro-defendant decisions, conflates momentous and trivial decisions, or contains many workers in national firms with branches in different circuits enacting the same firm-wide policies across circuits, this would bias our estimates to zero, as would the tendency for labor mobility to cause employment outcomes to converge, the influence of precedent in one circuit to another, the ability for potential plaintiffs to choose jurisdictions to file suit, and issues of case selection correlating pro-plaintiff appellate decisions when the trial courts are extremely pro-defendant (see, e.g. Priest and Klein 1984; Eisenberg 1990).

Note that firms are unlikely to respond to the appointment of a particular type of judge to the circuit pool since there are many kinds of cases that could affect firm behavior. We also do not find a relationship between gender inequality and the proportion of the judicial pool that is a particular type. Nor do we find a relationship between pro-plaintiff decisions and the proportion of the judicial pool that is a particular type, a finding that is to be expected under the Priest-Klein model of case selection—if appellants expect a favorable judicial assignment, weaker cases may be filed; if appellants expect an unfavorable judicial assignment, only stronger cases may be filed. We conduct several checks for randomization of judges. Appendix Table B displays the results for orthogonality between judicial characteristics and case characteristics as determined by the lower court. Appendix Table C displays the test for serial correlation in the characteristics of judges assigned to sexual harassment cases. These checks are important because the decision to publish may introduce non-randomness. For example, the decision not to publish may be a compromise among judges who disagree about the correct outcome (Law 2005, Wald 1999). If Democratic appointees publish and Republican appointees choose not to publish sexual harassment decisions, then a correlation may arise between the egregiousness of the sexual

harassment case and the judicial panel composition, which reintroduces possible endogeneity between social trends related to the egregiousness of sexual harassment cases and the panel composition of published cases. Examining whether characteristics of the case determined by lower courts are unrelated to the composition of the appellate panel provides an assessment of this concern. Moreover, if panel composition significantly affects the decision to publish, panel composition of published cases would be serially correlated.[12]

C. First Stage

Table 1 documents the relationship between pro-plaintiff sexual harassment appellate decisions and the composition of sexual harassment panels. Panels A and B display the relationship at the judge-level and the case-level as discussed in the previous section. Panel C displays the relationship at the circuit-year level. The female Republican and male Democratic instruments have the predicted relationship with the number of pro-plaintiff decisions. The F-statistic of joint significance for both instruments in Column 3 is 22.9 when controlling for docket size and in Column 6 is 2.7 when controlling for the number of sexual harassment panels. A dummy variable indicating whether there were no cases in that circuit-year is also included to minimize mechanical correlation, since both the number of pro-plaintiff decisions and the number of panels with a particular judicial composition would equal 0. The F-test in Column 6 is admittedly weak. The identification strategy uses only gender and party of appointment. It may be the case that using additional demographic characteristics or the history of a judge's

---

[12] Another potential source of non-randomness is the decision by parties to settle after the panel composition is announced. Parties typically do not see the panel composition before they file an appeal. Appellate judges are revealed to parties very late in the process, usually after briefs are filed. Parties are unlikely to settle after filing briefs, since the relatively short amount of time between learning the judges' identities and the judges' decision (Revesz 2000, Jordan 2007) impose small additional costs relative to the cost of litigation prior to learning the judges' identities. Many decisions are based solely on the briefs since in many cases there is no oral argument. However, to the extent plaintiffs settle when they discover the judges' identities are unfavorable to their winning the case (Wagner and Petherbridge 2004), this would tend to bias the first stage towards zero.

votes (in sexual harassment, in other areas of law, or even when joined by certain colleagues) as ideology score would strengthen the first stage (Fischman 2007). There is a power trade-off, however, between finer gradations in categorizing judges and whether those categories of judges vote in a manner that is statistically significantly different from the voting behavior of other categories of judges. When we use a LASSO technique for selecting instruments (Bai and Ng 2009; Belloni, Chernozhukov, Chen, and Hansen 2010), we get comparable instruments (the top three instruments are at least 2 Democratic appointees, at least 1 female Republican appointee, and at least 1 male Democratic appointee) and two-stage least squares estimates.

IV. Estimating the Impact of Sexual Harassment Law on Gender Inequality

A. Basic Findings

Forbidding sexual harassment increased female employment status, hours worked, wages, and management status relative to males. Table 2 indicates that a pro-plaintiff sexual harassment appellate decision increases female employment relative to males by 1.1 percentage points in the likelihood of working part-time or full-time (Column 4), 0.3 in hours worked last week (Column 8), 0.06 in log real weekly earnings (Column 12), and 0.3 percentage points in the likelihood of being a manager (Column 16).[13] To get a sense of the magnitude of 1.1 percentage points in the likelihood of employment and 0.06 points in log real weekly earnings, our estimates are in line with related studies. The Equal Employment Opportunity Act increased black employment shares by 0.5-1.1 points per year and narrowed the black-white earnings gap by 0.11-0.18 log points per year (Chay 1998), state laws barring race discrimination increased black men's relative earnings by 0.28% per year (Neumark and Stock 2006), and a standard deviation increase in median male sexism in a state is associated with a 0.031 log point decrease in female

---

[13] Probit specifications for employment and managerial status give similar results.

wages relative to males (Charles, Guryan, and Pan 2010). The average circuit-year experienced 0.67 pro-plaintiff sexual harassment appellate decisions, as indicated in the summary statistics displayed in Appendix Table A.

To understand what 1.1 percentage points means in terms of its economic significance, given that 81% of men are employed and 65% of women are employed on average across all circuits and years (documented in lower panel of Appendix Table A), 0.67 * 0.011 / (0.81 - 0.65), or 4.6%, is the reduction in inequality created by sexual harassment law. The other coefficients can be interpreted similarly and the percent reduction in inequality range from 1.2% in wage inequality, 1.8% in hours inequality, to 8.6% in managerial inequality. OLS estimates are displayed in the first two columns for each employment outcome while IV estimates are displayed in the second two columns. The estimates are similar, suggesting that to the extent omitted factors motivate certain judges to select cases or legal categories to influence, using the fact that appellate judges are randomly assigned to cases significantly addresses this source of endogeneity even in an OLS specification. Figure 8 displays the relationship between the number of pro-plaintiff decisions and gender inequality (calculated from regressing the employment variable on observed characteristics fully interacted with a dummy for being female—basically an Oaxaca decomposition of gender inequality into contributing factors, multiplying the coefficients on the interaction terms with the observed characteristics, and averaging across all individuals in a circuit-year). Positive effects of sexual harassment law, statistically significant at the 5% level, are found for each employment outcome except hours worked. The figure also suggests that the results would be robust to restricting to circuits and years with sexual harassment cases, which is what we find in the following section. The results are also robust to dropping each circuit one at a time and controlling for composition of the

circuit pool of judges in an interval as opposed to continuous manner. The consistency across controls suggests that remaining omitted variables bias may be small (compare Columns 3 and 4, 7 and 8, 11 and 12, and 15 and 16).

B. Precedent

Do appellate decisions really establish precedent? According to one view, appellate courts only decide issues of new law while lower courts decide issues of fact. Appellate courts are continually finding new distinctions with which to expand or contract the space under which a sexual harassment perpetuator would be liable (e.g. switching from a reasonable person standard to a reasonable woman standard for determining whether sexual harassment occurred; see the Appendix for a list of major doctrinal developments). For an example of such a distinction in tort liability, borrowed from Gennaioli and Shleifer (2007), suppose a dog biting a man imposes a tort liability on the owner. A new distinction would be, if the dog was on a leash, then the owner is not liable, i.e. a pro-defendant decision decreasing the space for which the owner is liable. A subsequent pro-defendant decision could be, even if the dog was not on a leash, if the dog bite occurred in a forest, the owner is not liable. In this manner, by adding distinctions, appellate courts continually expand or restrict the space under which liability occurs, making it easier or harder for subsequent plaintiffs to bring and win suit.

Practically speaking, many appellate decisions are often not establishing a new legal standard, simply affirming lower court opinions, or reversing them if the lower court was wrong about the existing legal standard. We proxy for the establishment of precedent using subsequent citation and examine whether highly cited cases have a larger influence on labor markets. Since many circuit-years did not experience new precedent in sexual harassment law, we restrict our attention to circuit-years that had at least one sexual harassment appellate decision. In spite of

23

the fact that this omits half of the circuit years, pro-plaintiff sexual harassment decisions in circuits and years that are highly cited (i.e. above the median in citations per case) increase female labor market outcomes by up to three times more than do the pro-plaintiff sexual harassment decisions in circuits and years that are infrequently cited as shown in Table 2.

C. Longevity of the Effect of Sexual Harassment Precedent

1. Distributed Lag

The impact of appellate precedent may vary over time. Firms may need time to adjust to a new legal regime; alternatively, the effects of a law change may fade as expectations adjust. We build on our basic model with a distributed lag specification that includes five years of lags of the law and one lead. The use of leads helps assess whether trends in labor market inequality precipitate sexual harassment precedent. If so, the labor market effects of the law observed in the previous sections could be due to underlying improvements in gender inequality and not the law itself. Further, the number of sexual harassment appeals may be correlated with omitted factors that also affect gender inequality, so we examine whether pro-defendant sexual harassment decisions have the opposite effect of pro-plaintiff sexual harassment decisions. Since Figure 8 displays a large number of circuit-years with low gender inequality and no appellate cases, including a dummy variable indicating there were no sexual harassment appellate decisions prevents a mechanical correlation between pro-plaintiff decisions, pro-defendant decisions, and gender inequality.

The distributed lag specifications displayed in Table 4 generally show the strongest effect of the law in the second or third year after a pro-plaintiff decision but no effect in the year before the pro-plaintiff decision and a weaker effect by the fourth and fifth year after a pro-plaintiff decision. In the OLS specification (Columns 1, 4, 7, and 10), the magnitudes of the coefficients

24

are comparable, in sum, to the effect from the basic specification and, e.g., the coefficient of 0.0029 in Column 1 for employment status suggests that the typical number of pro-plaintiff decisions results in 0.37% reduction in inequality with two years lag; the IV specification (Columns 2, 5, 8, and 11) gives somewhat larger coefficients. The effect of pro-plaintiff decisions on managerial inequality one year before the decision is positive and statistically significant in the OLS specification[14] but not in the IV specification, which is consistent with the random assignment of judicial background characteristics to sexual harassment panels being uncorrelated with trends in gender inequality. The coefficients in Column 2 are graphically displayed along with the 95% confidence intervals in Figure 10. The pro-defendant specifications (Columns 3, 6, 9, and 12) show negative effects on gender inequality, opposite in sign from the pro-plaintiff specifications and in magnitude in between the OLS and IV pro-plaintiff specifications. Pro-defendant decisions also appear to have a longer-lasting effect than pro-plaintiff decisions.

2. Cumulative

If the effects of new precedent are more permanent, then the distributed lag specification inappropriately assumes the effects disappear after five years. We can also measure precedent using the cumulative number of pro-plaintiff and pro-defendant sexual harassment decisions. This specification admittedly imposes a parametric assumption where early pro-plaintiff decisions have a much larger effect than recent decisions; on the other hand, this may match reality where early decisions setting new precedent have more influence than later ones. To cumulate the circuit-by-year varying variables (number of pro-plaintiff decisions, number of pro-

---

[14] One reason for this may be that the hiring of female managers responds more quickly to sexual harassment litigation. Firms may respond to major lower court decisions while parties are appealing the trial court decision.

defendant decisions, docket size, and instruments), these are simply summed; the judicial pool characteristics are calculated as a weighted average probability over all years where the weights are the number of sexual harassment panels in a particular year.

The cumulative law specifications displayed in Table 5 indicate that each pro-plaintiff decision permanently increases the probability a female has any employment relative to the probability a male has any employment by 0.46 percentage points (Column 2), in the IV specification, which is a bit larger than the OLS specification (Column 1). To interpret 46 basis points, we must consider that in 1982, 79% of men were employed and 57% of women were employed. Since there were 168 pro-plaintiff and 83 pro-defendant decisions across 12 circuits, we calculate 168 / 12 * 0.0046, or 0.06, for the cumulative effect of pro-plaintiff decisions and 83 / 12 * -0.0027, or -0.023, for the cumulative effect of pro-defendant decisions. Adding these two values and dividing by 0.79 − 0.57, the original amount of inequality between men and women, leads to estimating the cumulative effect of sexual harassment law as reducing the inequality in the employment status between men and women by 17% during this time period. Calculations for the reduction in other forms of employment inequality give similar estimates. If sexism is a channel through which sexual harassment law erodes gender inequality, then these estimates are comparable with the finding that a standard deviation in median male sexism explains 6.3% of the mean female-male offer wage gap in 1977 and 52% of the cross-state standard deviation of gender offer wage gaps (Charles, Guryan, and Pan 2010), employing a methodology to estimate the effects of prejudice (Charles and Guryan 2008).

3. Percentage

The most important threat to the identification assumption is that the instruments, panels with at least one female Republican appointee and panels with at least two male Democratic appointees, may be correlated with omitted factors that affect gender inequality in labor market outcomes. Even controlling for composition of the judicial pool, examining the effect of pro-plaintiff vs. pro-defendant decisions, gender inequality one year before the precedent, and to address omitted factors influencing the quantity of litigation, controlling for docket size, a dummy variable indicating there are no sexual harassment panels, and in a subsequent section, gender discrimination precedent, we could still worry that the number of appellate sexual harassment cases be correlated with gender inequality two or three years after the appellate decision, perhaps due to the confluence of unobserved economic forces which affect the filing of employment discrimination cases (see, e.g. Donohue and Siegelman 1991, 1993, 1995, and 2005).

Controlling for the number of sexual harassment appellate decisions addresses this endogeneity concern, but introduces another. Last year's sexual harassment pro-plaintiff decisions may affect the following year's number of sexual harassment appeals. Putting a variable that is caused by pro-plaintiff decisions on the right-hand side leads to an underestimate of the effect of pro-plaintiff decisions. It may also make the lead coefficients less interpretable since the denominator of the lead variable, the future number of sexual harassment appeals, can be affected by the current year's pro-plaintiff decisions. It is also not clear whether firms respond more to the number of new pro-plaintiff precedents or the percentage of new pro-plaintiff precedents, which would mechanically discount the decisions that occur in circuit-years with many sexual harassment appeals.

With these caveats in mind, the percentage specifications in Table 6 show weaker but still statistically significant effects two or three years after the pro-plaintiff precedents are made (Columns 5, 8, 11). The coefficient of 0.375 in Column 4 for hours worked suggests that the typical proportion of pro-plaintiff decisions, 0.67, results in 0.375 * 0.67 / (34.33 - 22.78), or 2% in the reduction in inequality with one-year lag. The IV estimates give larger but statistically weaker estimates than the OLS estimates in either the pro-plaintiff or pro-defendant specifications, which again give opposite effects. Though the first stage when controlling for the number of sexual harassment panels is somewhat weak, the IV estimates do not explode although they do give statistically significant effects for pro-plaintiff decisions one year before the decision in some specifications.

4. Race

As a check for whether other forms of inequality, such as race inequality, affect both gender inequality and sexual harassment appeals and whether the methodology is such that sexual harassment precedent is spuriously correlated with any kind of inequality, not just gender inequality, we run the basic specification on the inequality between whites and non-whites in Table 7. No effects are found on any employment dimension.

V. Evidence for the Insider-Outsider Model of Harassment and
Involuntary Unemployment

Thus far, we have shown positive effects of sexual harassment law on female employment outcomes overall. We now turn to some evidence for the insider-outsider model of harassment and involuntary unemployment. We show that forbidding sexual harassment 1) particularly encouraged entry of outsiders, 2) had the greatest effects for those who should be in the labor

force, and 3) had ameliorative effects on gender inequality that are reduced for insider women previously in the work force.

Table 8 displays the estimated effects of sexual harassment law for the entire population as well as those in the labor force. When the analysis is restricted to the labor force we find a negative effect of sexual harassment law on employment status, hours worked last week, and earnings, statistically significant for hours worked (Columns 2, 6, and 10). The effect on management inequality is basically unchanged as compared to the effect for the entire population (Columns 13 and 14). These results suggest that costs borne by insider women are primarily in the hours worked and perhaps there was a compensating movement towards managerial positions that dampened adverse wage effects.

Since forbidding sexual harassment affects the probability of employment, the decision to be in or out of the labor force may also be endogenous to sexual harassment law. Stratifying by this decision should bias any estimated effect towards zero. To see how, consider that the effect of sexual harassment law could be captured on two dimensions: the decision to enter the workforce as well as employment outcomes (wages, hours, and management). Suppose forbidding sexual harassment encouraged entry into the workforce as well as increased employment outcomes but primarily for those who entered the workforce. If we look only at the sample of people in the workforce, and in the extreme case, where everyone who entered the work force experienced labor market improvements (and no one already in the work force experienced negative labor market consequences), then if we estimate the effect of sexual harassment law only on this sample, it would be biased to zero because there would be no difference in employment outcomes in jurisdictions where sexual harassment is greatly forbidden versus jurisdictions where sexual harassment is only lightly forbidden. However, the

management effects are stable and the hours and employment effects become negative, suggesting that sexual harassment law not only increased the well-being of outsiders but imposed some costs on insiders.

Nevertheless, we address the potential endogeneity in another way. We consider the effect of sexual harassment law on individuals who *should* be in the labor force, based on demographic characteristics. We regress not-in-labor-force status on demographic characteristics (except gender) and compute a predicted probability of being in the labor force. Older, non-minorities, married, and more educated individuals are more likely to be in the labor force. When we stratify on who should be in the labor force, we find that sexual harassment law has a larger ameliorative effect on gender inequality for those who should be in the labor force (Columns 3, 7, 11, and 15) than for those who should not be in the labor force (Columns 4, 8, 12, 16) based on observed characteristics. Both pieces of evidence taken together suggest that outsider women are entering the labor force at the expense of insider women.

As further evidence of the incidence of sexual harassment law, we decompose the effect of sexual harassment law on the gender wage gap by successively controlling for other employment outcomes. Column 1 in Table 9 displays the effect of sexual harassment law on the gender wage gap without additional employment controls. When managerial status is controlled for, the effect on the gender wage gap drops by 20% in Column 2. The effect further drops when hours worked last week is controlled for in Column 3. The effect disappears when employment status is controlled for and the R-square becomes greater than 0.9 in Column 4. These patterns remain when these employment outcomes are controlled for individually in Columns 5 and 6. Finally, when we include only the individuals who report some non-zero wages, the effect of sexual harassment law is not statistically significant in Column 7.

Taken together, these results suggest that forbidding sexual harassment in large part encouraged the entry of outsider women, who then obtained part or full time employment and received wages. Some of the insider women lost out in hours worked while others were promoted to managerial status. Insider men lost the most in terms of employment status, but their loss is smaller than the gain by outsider women.

VI. How Does Sexual Harassment Law Impact Gender Inequality?

A. Sexual Harassment vs. Gender Discrimination

In the previous sections, we explored the impact of sexual harassment law. 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. If doctrinal developments in sexual harassment law are due to developments in gender discrimination doctrine and if gender discrimination law has independent effects on gender inequality, we may overestimate the effect of sexual harassment law if we do not control for gender discrimination law. On the other hand, if employers would like to hire fewer women because of the costs of the mandated benefit, they may be unable to because of pre-existing gender discrimination law. Further, by examining the effects of sexual harassment law and gender discrimination law, separately instrumented for with the random assignment of judges to the respective case types, we can see whether the methodology has enabled the identification of causal effects of distinct areas of appellate law.

Since our data on gender discrimination cases only includes cases from 1995-2002, we restrict our analysis in Table 10 to only those years. Further, as described in the Appendix, the gender discrimination cases are more precisely categorized as gender discrimination cases

without elements of sexual harassment, which may be under-inclusive, while sexual harassment cases are those with sexual harassment and perhaps elements of gender discrimination, which may be over-inclusive. Appendix Table F displays the first stage and indicates that having a female or Democratic appointee on a gender discrimination case positively predicts pro-plaintiff decisions, corroborating Peresie (2005). As a robustness check and a check of the exclusion restriction, we regress the sexual harassment instruments on the gender discrimination pro-plaintiff decisions. Both placebo instruments fail to be statistically significant. 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.

Because of the different data collection and different set of years, the results for sexual harassment law are slightly different and it only affects inequality in employment status and managerial status. Nevertheless, sexual harassment law trumps gender discrimination law, which never appears to have an impact on gender inequality in the even numbered columns of Table 10. That court-made gender discrimination law has little ameliorative effect on female outcomes is consistent with the evidence that state equal pay laws for women do not increase employment outcomes for women (Neumark and Stock 2006). Finally, the effect of sexual harassment law changes little when gender discrimination law is also controlled for.

To our knowledge, the finding that the effect of sexual harassment law is empirically very different from that of gender discrimination law is new. Feminist legal theorists interpreting pre-existing anti-discrimination law had practical ramifications for the economic status of women consistent with an insider-outsider model of harassment and involuntary unemployment. For example, discrimination in hiring on the basis of names appearing on resumes need not involve

harassment whereas making victims feel excluded via harassment need not involve obvious disparate treatment in wages or hiring but nevertheless may result in greater disparate impact. Even taking into the account that the categorization of these gender discrimination cases by Sunstein, Schkade, and Ellman (2006) may be under-inclusive and that of sexual harassment cases over-inclusive, at the minimum these results suggest that gender discrimination cases without elements of sexual harassment are not effective at reducing gender inequality, while sexual harassment cases with elements of gender discrimination are. These results open up an interesting avenue of research as to why gender discrimination law may be less effective in reducing gender inequality than sexual harassment law. Are laws with ambiguous scope more effective in instigating social change? Are firms more threatened by the prospect of being sued for sexual harassment than they are for gender discrimination? The fact that sexual harassment litigation tends to be targeted at individuals and gender discrimination litigation is more likely than sexual harassment litigation to be statistical in nature, the fact that individuals are likely more risk-averse than firms who have other ways of smoothing risk, and the fact that information consultants greatly exaggerated the risk of sexual harassment litigation, suggest reasons for why pro-plaintiff sexual harassment appellate decisions had greater impact than pro-plaintiff gender discrimination appellate decisions.

B. Law vs. Economics of Sexual Harassment Law

We now examine exactly through what channel sexual harassment law has its labor market effects. Is it the law or the economics associated with the law that motivates behavioral change? Scholars have long debated whether sanctions associated with the law alone deter behavior (Becker 1968) or whether the law in and of itself sends messages about right and wrong (Tyler

and Huo 2002) and whether this channel may be more effective than tougher sanctions. Empirical evidence shedding light on these questions is lacking (Kaplow and Shavell 2002; Hurd 1999).[15] To explore these questions, we examine the effects of damages awarded in sexual harassment cases, number of pro-plaintiff precedents, and a measure of doctrine via Supreme Court decisions that resolve previous Circuit splits.

We find that while damages alone do ameliorate gender inequality as shown in Columns 1, 4, 7, and 10 of Table 11, they do not when the number of pro-plaintiff decisions is also controlled for. Appendix Table F Columns 7-9 indicate that the number of sexual harassment damages cases with female judges and the number of sexual harassment damages cases with Democratic appointees positively predict damages awarded in sexual harassment cases. Further, comparing the estimates in Table 2 and Table 11 show that the effect of the number of pro-punishment cases changes little in magnitude when we control for damages.[16]

We capture two significant moments of doctrinal change through the major Supreme Court decisions on sexual harassment law (MacKinnon 2007), although we acknowledge the potential endogeneity of Supreme Court decisions (as investigated in Appendix Table G). 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

---

[15] For more references, see e.g. Experimental work: Bohnet, Frey, and Huck (2001), Bohnet and Cooter (2004), Galbiati and Vertova (2008), Tyran and Feld (2004), McAdams and Nadler (2005, 2008); Theoretical work: Cooter (1998), Tabellini (2007), Cooter (2000), Bar-Gill and Fershtman (2004); Field evidence: Funk (2007), Fisman and Miguel (2007), and Frey and Feld (2003).

[16] We also use different measures of damages, such as log damages and the number of cases with any damages awarded.

employer liability.[17] We find that *Harris* increased female employment status while both *Harris* and *Faragher* increased female management status even when controlling for the number of pro-plaintiff decisions as well as the damages awarded in sexual harassment cases.[18]

In sum, sexual harassment law, and in particular, legal doctrine as measured by the number of pro-plaintiff cases, has had 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. Of course, pro-plaintiff sexual harassment decisions may have other financial incentive effects not captured in damages awarded. For example, the cost of litigation and the loss of reputation are other ways for pro-plaintiff decisions to have economic effects. Nevertheless, it appears that monetary sanctions alone do not have an effect and a practical implication for civil rights lawyers and labor lawyers is: if they want to have the largest social impact, they might not stop at settlement but push for a pro-plaintiff decision on the books, since if published damages do not have an effect, unpublished settlement also might not have an effect.

C. Disaggregating the Effects of Sexual Harassment Law by Firm Size and Industry

Larger firms are both more likely to pay attention to human resources consultants as well as be liable for damages. Do workers in larger firms experience greater changes in gender inequality than workers in smaller firms? Using the March CPS dataset (a smaller dataset than the MORG CPS that contains additional variables such as firm size), we examine the differential

---

[17] *Meritor Savings Bank v. Vinson*, 1986, 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. But because of minimal regional variation before *Meritor* we do not use it in our analysis. *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.

[18] The data appendix provides details on how we code *Harris* and *Faragher*.

impact of sexual harassment law for large versus small firms.[19] Large firms are defined as those with over 25 employees. The ameliorative effects on gender inequality are primarily observed among larger firms as shown in Table 12, consistent with the story that labor lawyers and human resources consultants translated appellate decisions for firms of a sufficient size. On some outcomes, the large and small firm effects are statistically significantly different from each other. Further, the effects are somewhat larger and more statistically significant for women in blue collar industries than for women in white collar industries, although the difference is not statistically significantly different. The bulk of plaintiffs (38%) are blue collar and blue collar plaintiffs win more often in district courts, but high-status victims may have more resources to bring suit (Juliano and Schwab 2001), so the effects may offset each other.

VII. Alternative Theories

In this section, we consider several alternative theories to explain the findings in this paper.

A. Hire and Promote Women as Preventive Measure

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. Replacing male with female managers could directly reduce the harassment of female employees, particularly in the context of quid pro quo harassment claims, in which the employee's promotion is impacted by her acquiescence to or rejection of harassment.

While firms may have chosen to hire women or even promote them as a way to change

---

[19] 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.

firm culture and stem lawsuits, why isn't replacing male managers with female managers enough? Why do firms hire outsiders? Moreover, under this hypothesis, raising the wages of insider women should be enough, but their wages actually fall. Furthermore, swapping male managers for females is unlikely to end sexual harassment as the vast majority are co-worker harassment cases. According to the 1999 Society for Human Resource Management Survey, 29% of sexual harassment cases are supervisor cases; other surveys document 51% or 78% of cases involve co-worker harassment (Schultz 2006). In judicial opinions, 59% of plaintiffs name supervisors as harassers, but this may be attributable to plaintiffs being aware of the lower probability of success when coworkers are the harassers, and therefore victims of co-worker harassment file suit less frequently (Juliano and Schwab 2001). This supports the insider-outsider involuntary unemployment theory as opposed to simply hiring female managers as a prophylactic.

B. Tax on Hiring of Men

An alternative view of sexual harassment law is simply that it is a tax on the hiring of men, who were potential harassers. A less sympathetic view is that sexual harassment law mandates a transfer from females to males. If sexual harassment law is a tax on the hiring of men, why didn't insider male outcomes fall relative to insider female outcomes? A simple transfer story is also inconsistent with what appears to be greater improvements in female outcomes than declines in male outcomes.

C. Machismo

The insider-outsider model suggests intentional harassment, but the productivity of females

could have been lower simply due to their unfamiliarity with the machismo culture prevalent before females were hired in substantial numbers. However, if this were the case, productivity should not increase with the elimination of machismo. Consistent with a model where insiders intentionally harass outsiders as part of general rent-seeking behavior, forbidding sexual harassment improved labor market outcomes for females more than they decreased them for males. If males preferred the machismo environment, then eliminating this employment perk should have raised their wages through compensating differentials.

D. Compensating Differentials

Compensating differentials appears to explain some of the findings. Insider women got compensated for having to face sexual harassment, so with sexual harassment law, their wages decline. However, a pure compensating differentials story would not explain why outsider women join the work force and have their wages and employment outcomes increase. Wages inside the labor force would have adjusted downwards for the decrease in sexual harassment and outsider women on the margin would have been indifferent to entry.

E. Change in the Composition of the Female Labor Force

Perhaps in the absence of sexual harassment law, firms chose to hire attractive women who were less productive and after forbidding sexual harassment, firms hire more productive women. Then, insider female wages decline while outsider females increase in employment opportunities and outcomes. This substitution in the composition of the female labor force is somewhat consistent with the analysis examining those who are predicted to be in the labor force presented in Table 8: those who are older, white, married, and more educated are more likely to be in the

labor force and also experience greater benefits from sexual harassment law, to the extent that age and education predict productivity and older married women are considered less attractive. On the other hand, when we control for these demographic characteristics they only reduce the magnitude of the effect of sexual harassment law by a small amount as shown in Table 2. Furthermore, when we disaggregate the effects of sexual harassment law by age, there are no significant differences by age group, suggesting that firms are not at least substituting older women in for younger women. Women of all ages are harassed, so there is no a priori reason to think the insider-outsider dynamic applies only to women of a particular age.

F. Mandated Benefit

While the mandated benefits view is not an alternative theory for the results, explaining why maternity mandates and the ADA had different effects from sexual harassment law is worth mentioning. First, unlike maternity mandates, sexual harassment directly improves the productivity of women by making a better work environment. Second, unlike ADA, it is far more difficult to explicitly determine which women are likely to impose the costs of sexual harassment on a firm. Disabilities are often visible to employers and, therefore, the unfunded mandate of accommodations may have led to calculated decisions to not hire particular disabled workers whereas employers could not make the same calculated decisions vis a vis women. Third, unlike the cost of complying with the ADA or the federal requirement of providing of maternity mandates, the cost of compliance with sexual harassment law could be reduced by not hiring either the group being harassed or the group doing the harassing. The costs of sexual harassment law are quite high, although exact figures for all the direct and indirect costs are difficult to obtain. Some labor lawyers observe that the ADA further required large fixed costs upfront in physical infrastructure, whereas the fear of a Title VII suit is always looming.

VIII. Conclusion

Interpreting anti-discrimination law to forbid sexual harassment has been a key contribution 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 precedent on gender inequality by using the fact that federal judges are randomly assigned to appellate cases along with the fact that gender and party of appointment of judges affect sexual harassment decisions. We find that sexual harassment law does not appear to exacerbate gender inequality. Pro-plaintiff precedent increases female wages and employment relative to that of men. It also increases the proportion of female managers relative to male managers. Pro-defendant precedent decreases female employment outcomes relative to that of men. Sexual harassment decisions are not related to gender inequality before they are made. Any counter-explanation for the effects on gender inequality several years after pro-plaintiff precedents must address why the opposite effects are seen for pro-defendant precedents.

When, however, restricted to people previously in the work force, sexual harassment law worsens some female employment outcomes. The effect of sexual harassment law on the gender wage gap largely disappears when employment status is controlled for. Workers predicted to have high labor force attachment see larger effects of sexual harassment law. 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 increases entry of outsiders, raising their employment and then wages, than with compensating wage differentials or mandated benefits models of sexual harassment.

One of the more surprising results is that the ameliorative effects on gender inequality derive from sexual harassment law, not gender discrimination law, highlighting the practical contribution of feminist interpretation. Interestingly, 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. Subsequent research could examine exactly what kinds of sexual harassment doctrine affect gender inequality and why gender discrimination law is less effective than sexual harassment law, perhaps by examining the consequences of different doctrinal categories of sexual harassment law just as this paper has examined the consequences of different doctrinal categories of gender discrimination. Future work could further examine shifts in occupation, composition of the work force, or even productivity measures, in order to explore all of the ways society responded to sexual harassment law.

We conclude with a note on anti-discrimination law and the regulation of labor markets more generally. Existing models of anti-discrimination law suggest that profit-maximizing firms have their own incentives to not discriminate (Becker 1971, Epstein 1995). Similar arguments could be applied in the context of sexual harassment law. Profit-maximizing firms should have their own incentive to forbid harassment and retain outsider workers at higher productivity, so forbidding sexual harassment with its accompanying litigation and human resources cost should have no effect and could be inefficient. However, there are at least two reasons for why firms might not forbid harassment on their own (Lindbeck and Snower 1988). First, insider employees

may be risk averse and by forbidding harassment, changing the insider profit sharing scheme may impose additional risk on insider employees, who then suffer a utility loss. The firm may be unable to compensate them for this loss. Second, an insider cooperates with entrants if his gains, a share of the additional profit revenue as a result of his cooperation, exceed his losses in market power as his wage falls towards his reservation wage. However, this only happens if the firm relinquishes a share of gross profit, something that may make it a net loser compared to other firms. Then the firm has no incentive to implement the new contract. But a legal regime equalizes the playing field across all firms when no firm by itself would have the incentive to forbid harassment. The fact that female labor market outcomes improve more than male labor market outcomes decline is consistent with the idea that there was some degree of rent capture by insiders and that firms were not profit-maximizing and forbidding harassment on their own in the absence of sexual harassment law.

REFERENCES:

Acemoglu, Daron, and Joshua D. Angrist. 2001. "Consequences of Employment Protection? The Case of the Americans with Disabilities Act." *Journal of Political Economy* 109 (October): 915-57.

Akerlof, George and Rachel Kranton. 2000. "Economics and Identity." *Quarterly Journal of Economics* 115 (August): 715-53.

Altonji, Joseph G. and Rebecca M. Blank. 1999. "Race and Gender in the Labor Market," in O.A. Ashenfelter and D. Card, eds. Handbook of Labor Economics, Volume 3, pp. 3143-3259.

Ashenfelter, Orley, Theodore Eisenberg, and Stewart Schwab. 1995. "Politics and the Judiciary: The Influence of Judicial Background on Case Outcomes." *The Journal of Legal Studies* 24 (June): 257-81.

Autor, David H., John J. Donohue, and Stewart J, Schwab. 2006. "The Costs of Wrongful-Discharge Laws." *The Review of Economics and Statistics* 88 (May): 211-31.

Autor, David H., Lawrence F. Katz, and Melissa S. Kearney. 2005. "Rising Wage Inequality: The Role of Composition and Prices." Working Paper no. 11628 (September), NBER, Cambridge, MA.

Bai, Jushan and Serena Ng. 2009. "Selecting Instrumental Variables in a Data Rich Environment" *Journal of Time Series Econometrics*, 1:1.

Baker, Douglas D., Robert A. Jackson, and Meredith A. Newman. 2003. "Sexual Harassment in the Federal Workplace." *Public Administration Review* 63 (July): 472-83.

Bar-Gill, Oren and Chaim Fershtman. 2004. "Law and Preferences." *Journal of Law, Economics, and Organizations* 20 (October): 331-52.

Basu, Kaushik. 2003. "The Economics and Law of Sexual Harassment in the Workplace." *Journal of Economic Perspectives* 17 (Summer): 141-57.

Basu, Kaushik. 2004. *Prelude to Political Economy: A Study of the Social and Political Foundations of Economics.* U.K.: Oxford: Oxford University Press.

Becker, Gary. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy* 76 (April): 169-217.

Beller, Andrea H. 1979. "The Impact of Equal Opportunity Laws on the Male-Female Earnings Differential." In *Women in the Labor Market*, ed. Cynthia Lloyd, Emily Andrews, and Curtis Gilroy, 203-30. New York: Columbia University Press.

Belloni, Alex., Victor Chernozhukov, Daniel L. Chen, and Chris Hansen. 2010. "Instrument Selection using LASSO" MIT mimeo.

Bisom-Rapp, Susan. 2001a. "Fixing Watches with Sledgehammers: The Questionable Embrace of Employee Sexual Harassment Training by the Legal Profession." *University of Arkansas at Little Rock Law Review* 24 (April): 147–68.

Bisom-Rapp, Susan. 2001b. "An Ounce of Prevention Is a Poor Substitute for a Pound of Cure: Confronting the Developing Jurisprudence of Education and Prevention in Employment Discrimination Law." *Berkeley Journal of Employment and Labor Law* 22 (February): 1–46.

Black, Sandra and Philip Strahan. 2001. "The Division of Spoils: Rent-Sharing and Discrimination in a Regulated Industry." *American Economic Review* 91 (September): 814-831.

Blau, Francine D., and Lawrence M. Kahn. 2006. "The US Gender Pay Gap in the 1990s: Slowing Convergence." *Industrial & Labor Relations Review* 60 (October): 45-66.

Bohnet, I., B. Frey, and S. Huck. 2001. "More Order with Less Law: On Contract Enforcement, Trust, and Crowding." *American Political Science Review* 95 (February): 131-44.

Bohnet, I. and Robert Cooter. 2004. "Expressive Law: Framing of Equilibrium Selection." Working Paper no. RWP03-046 (February), Kennedy School of Government, Harvard University, Cambridge, MA.

Boyd, Christina L., Lee Epstein, and Andrew D. Martin. 2010. "Untangling the Causal Effects of Sex on Judging." *American Journal of Political Science* forthcoming.

Breyer, Stephen. 2004. Active Liberty: Interpreting Our Democratic Constitution. The Tanner Lecture on Human Values delivered at Harvard University November 17-19.

Brown, J. Robert and Allison Herren Lee, Esq. 2000. "The Neutral Assignment of Judges at the Court of Appeals." *Texas Law Review* 78 (April): 1037.

Card, David, and John E. DiNardo. 2002. "Technology and U.S. Wage Inequality: A Brief Look." *Federal Reserve Bank of Atlanta Economics Review* 87 (Third Quarter): 45-62.

Chang, Tom, and Antoinette Schoar. 2007. "Judge Specific Differences in Chapter 11 and Firm Outcomes." Working Paper (June), MIT Sloan School of Management, NBER and CEPR, Cambridge, MA.

Charles, Kerwin K. and Jonathan Guryan. 2008. "Prejudice and Wages: An Empirical Assessment of Becker's *The Economics of Discrimination.*" *Journal of Political Economy* 116 (October): 773-809.

Charles, Kerwin K., Jonathan Guryan, and Jessica Pan. 2010. "Sexism and Women's Labor Market Outcomes." Working Paper (October), University of Chicago.

Chay, Kenneth Y. 1998. "The Impact of Federal Civil Rights Policy on Black Economic Progress: Evidence from the Equal Employment Opportunity Act of 1972." *Industrial and Labor Relations Review* 51 (July): 608-32.

Cooter, R. 2000. "Do Good Laws Make Good Citizens? An Economic Analysis of Internalized Norms." *Virginia Law Review* 86: 1577-601.

Cooter, R. 1998. "Expressive Law and Economics." *Journal of Legal Studies* 27 (June): 585-608.

Dixon, Rosalind. 2010. "Female Justices, Feminism & the Politics of Judicial Appointment: A Re-Examination." University of Chicago Law School mimeo.

Dobbin, Frank and Erin L. Kelly. 2007. "How to Stop Harassment: Professional Construction of Legal Compliance in Organizations." *American Journal of Sociology* 112 (January): 1203-43.

Donohue, John and Peter Siegelman. 1991. "The Changing Nature of Employment Discrimination Litigation." *Stanford Law Review* 43 (May): 983-1033.

Donohue, John and Peter Siegelman. 2005. "The Evolution of Employment Discrimination Law in the 1990s: A Preliminary Empirical Evaluation." Laura Beth Nielsen and Robert L. Nelson, eds., *Handbook of Employment Discrimination Research*. New York: Foundation Press.

Donohue, John and Peter Siegelman. 1993. "Law and Macroeconomics: Employment Discrimination Over the Business Cycle." *University of Southern California Law Review*. 66 (March): 709-65.

Donohue, John and Peter Siegelman. 1995. "The Selection of Employment Discrimination Disputes for Litigation: Using Business Cycle Effects to Test the Priest/Klein Hypothesis." *Journal of Legal Studies* 24 (June): 427-62.

Duflo, Esther, Rachel Glennerster, and Michael Kremer. 2007. "Using Randomization in Development Economics Research: A Toolkit" in T. Paul Schults, and John Strauss (eds.) *Handbook of Development Economics*, Elsevier Science Ltd.: North Holland, 2007 Vol. 4, pp. 3895-62.

Eberts, Randall W., and Joe A. Stone. 1985. "Male-Female Differences in Promotions: EEO in Public Education." *Journal of Human Resources* 20 (Fall): 504-21.

Edelman, Lauren B. 1992. "Legal Ambiguity and Symbolic Structures: Organizational Mediation of Civil Rights Law." *American Journal of Sociology* 97 (May): 1531–76.

Edelman, Lauren B. 2002. "Legality and the Endogeneity of Law." Pp. 187–203 in *Legality and Community: On the Intellectual Legacy of Philip Selznick*, edited by Robert A. Kagan, Martin Krygier, and Kenneth I. Winston. Lanham, MD: Rowman & Littlefield.

Edelman, Lauren B., Steven E. Abraham, and Howard S. Erlanger. 1992. "Professional Construction of the Law: The Inflated Threat of Wrongful Discharge." *Law and Society Review* 26 (March): 47–84.

Edelman, Lauren B., Christopher Uggen, and Howard S. Erlanger. 1999. "The Endogeneity of Legal Regulation: Grievance Procedures as Rational Myth." *American Journal of Sociology* 105 (September): 406–54.

Eisenberg, Theodore. 1990. "Testing the Selection Effect: A New Theoretical Framework with Empirical Tests." *Journal of Legal Studies* 19 (June): 337-58.

Ellman, Lisa Michelle, Cass. R. Sunstein, and David Schkade. 2003. "Ideological Voting on Federal Courts of Appeals: A Preliminary Investigation. AEI-Brookings Joint Center for Regulatory Studies." Working Paper no. 03-9 (September), University of Chicago, Chicago, IL.

Epstein, Richard. 1995. *Forbidden Grounds: The Case Against Employment Discrimination Laws*. Harvard University Press.

Farhang, Sean, and Gregory Wawro. 2004. "Institutional Dynamics on the U.S. Court of Appeals: Minority Representation Under Panel Decision Making." *Journal of Law Economics & Organization* 20 (October): 299-330.

Fischman, Joshua B. 2007. "Decision-Making Under a Norm of Consensus: A Structural Analysis of Three-Judge Panels." Working Paper (May), Tufts University, Boston, MA.

Fisman, Raymond and Edward Miguel. 2007. "Corruption, Norms, and Legal Enforcement: Evidence from Diplomatic Parking Tickets." *Journal of Political Economy* 115 (December): 1020-48.

Frey, B. and Lars Feld, 2003. "Deterrence and Morale in Taxation: An Empirical Analysis." Working Paper no. 760 (April), The Institute of Empirical Research in Economics, University of Zurich, Zurich, Switzerland.

Funk, Patricia. 2007. "Is There an Expressive Function of Law? An Empirical Analysis of Voting Laws with Symbolic Fines." *American Law and Economics Review* 9 (Spring): 135-59.

Galbiati, Roberto and P Vertova, 2008. "How Laws Affect Behavior." Working Papers (November), Bocconi University, Paris, France.

Gennaioli, Nicola and Andrei Shleifer. 2007. "The Evolution of Common Law." *Journal of Political Economy* 115 (February): 43-68.

Goldin, Claudia. 2002. "A Pollution Theory of Discrimination: Male and Female Differences in Occupations and Earnings." Working Paper no. 8985 (June), NBER, Cambridge, MA.

Gruber, Jonathan. 1994. "The Incidence of Mandated Maternity Benefits." *The American Economics Review* 84 (June): 622-42.

Hellerstein, Judith K., David Neumark, and Kenneth R. Troske. 1997. "Market Forces and Sex Discrimination." Working Paper no. 6321 (December), NBER, Cambridge, MA.

Hurd, Ian. 1999. "Legitimacy and Authority in International Politics." *International Organization* 53 (April): 379-408.

Jolls, Christine. 2000. "Accommodation Mandates." *Stanford Law Review* 53 (November): 223-306.

Jolls, Christine. 2001. "Anti-discrimination and Accommodation." *Harvard Law Review* 115 (December): 642-99.

Jolls, Christine, and J.J. Prescott. 2004. "Disaggregating Employment Protection: The Case of Disability Discrimination." Working Paper no. 10740 (September), NBER, Cambridge, MA.

Jordan, Samuel P. 2007. Early Panel Announcement, Settlement and Adjudication, *Brigham Young University Law Review* 55.

Juliano, Ann and Stewart Schwab. 2001. "The Sweep of Sexual Harassment Cases." *Cornell Law Review* 87 (March): 548-602.

Kaplow, Louis and Steven Shavell. 2002. *Fairness Versus Welfare.* Cambridge, MA: Harvard University Press.

Klarman, Michael. 2004. *From Jim Crow to Civil Rights: The Supreme Court and the Struggle for Racial Equality.* New York: Oxford University Press.

Kling, Jeffrey R. 2006. "Incarceration Length, Employment, and Earnings." *American Economic Review* 96 (June): 863-76.

Law, David S. 2005. "Strategic Judicial Lawmaking: Ideology, Publication, and Asylum Law in the Ninth Circuit", 73 *University of Cincinnati Law Review* 817.

Lindbeck, Assar and Dennis J. Snower. 1988. "Cooperation, Harassment, and Involuntary Unemployment: An Insider-Outsider Approach." *The American Economic Review* 78 (March): 167-88.

Lindbeck, Assar and Dennis J. Snower. 2001. "Insiders versus Outsiders." *The Journal of Economic Perspectives* 15 (Winter): 165-88.

MacKinnon, Catharine A. 1979. *Sexual Harassment of Working Women*. New Haven: Yale University Press.

MacKinnon, Catherine A. 2007. *Sex Equality*. New York: Foundation Press.

McAdams, Richard H. & Janice Nadler, "Coordinating in the Shadow of the Law: Two Contextualized Tests of the Focal Point Theory of Legal Compliance," 42 *Law & Society Rev*. 865-898 (2008).

McAdams, Richard H. & Janice Nadler, "Testing the Focal Point Theory of Legal Compliance: The Effect of Third-Party Expression in an Experimental Hawk/Dove Game," 2 *J. Empirical Legal Studies* 87-123 (2005).

Neumark, David, and Wendy A, Stock. 2006. "The Labor Market Effects of Race and Sex Discrimination Laws." *Economic Inquiry* 44 (July): 385-419.

Nielsen, Laura Beth, Robert L. Nelson, and Ryon Lancaster. 2010. "Individual Justice or Collective Legal Mobilization? Employment Discrimination Litigation in the Post-Civil Rights United States." *The Journal of Empirical Legal Studies* (June): 24.

O'Neill, June. 2003. "The Gender Gap in Wages, circa 2000." *The American Economics Review* 93 (May): 309-14.

Peresie, Jennifer L. 2005. "Female Judges Matter: Gender and Collegial Decision making in the Federal Appellate Courts." *Yale Law Journal* 114 (May): 1759-91.

Posner, Richard. 1998. "Against Constitutional Theory." *NYU Law Review* 73 (April): 1-22.

Priest, George L. and Benjamin Klein. 1984. "The Selection of Disputes for Litigation." *The Journal of Legal Studies* 13 (January): 1-55.

Revesz, Richard L. 2000. "Litigation and Settlement in the Federal Appellate Courts: Impact of Panel Selection Procedures on Ideologically Divided Courts," 29 *Journal of Legal Studies* 685 (2000).

Savin, N. E. 1984. *Multiple Hypothesis Testing*. Amsterdam: Elsevier Science Publishers BV, chap. Handbook of Econometrics, Volume II, pp. 827–879.

Schultz, Vicki. 1998. "Re-conceptualizing Sexual Harassment." *Yale Law Journal* 107 (April): 1683-805.

Schultz, Vicki. 1990. "Telling Stories about Women and Work: Judicial Interpretations of Sex Segregation in the Workplace in Title VII Cases Raising the Lack of Interest Argument." *Harvard Law Review* 103 (June): 1749-1843.

Schultz, Vicki and Stephen Petterson 1992. "Race, Gender, Work, and Choice: An Empirical Study of the Lack of Interest Defense in Title VII Cases Challenging Job Segregation." *University of Chicago Law Review* 59 (Summer): 1167-81.

Schultz, Vicki. 2006. "Understanding Sexual Harassment Law in Action: What Has Gone Wrong and What We Can Do About It." *Thomas Jefferson Law Review* 29 (Fall): 101-53.

Sharkey, Catherine M. 2006. "Dissecting Damages: An Empirical Exploration of Sexual Harassment Awards." *Journal of Empirical Legal Studies* 3 (March): 1-45.

Smith, Amanda D. 1997. "'Supervisor' Hostile Environment Sexual Harassment Claims, Liability Insurance and the Trend Towards Negligence." *University of Michigan Journal of Law Reform* 31 (Fall): 263.

Summers, Lawrence, 1989. "Some Simple Economics of Mandated Benefits." *American Economic Review* 79 (May): 177-83.

Summers, Lawrence, 2005. Speech at Conference on Diversifying the Science & Engineering Workforce at the National Bureau of Economic Research, January.

Tabellini, Guido. 2007. "The Scope of Cooperation: Values and Incentives." Working Paper no. 2236 (October), CESifo, Munich, Switzerland.

Tyler, Tom and Yuen Huo, 2002. *Trust in the Law: Encouraging Public Cooperation With the Police and Courts.* New York: Russell Sage Foundation Publications.

Tyran, Jeans-Robert and Lars Feld. 2004. "Why People Obey the Law: Experimental Evidence from the Provision of Public Goods." Working Paper no. 651 (September), CESifo, Munich, Switzerland.

Wagner, R. Polk and Lee Petherbridge, 2004. "Is the Federal Circuit Succeeding? An Empirical Assessment of Judicial Performance," 152 *University of Pennsylvania Law Review* 1105.

Wald, Patricia M. 1999. "A Response to Tiller and Cross," 99 *Columbia Law Review* 235.

Weinberger, Catherine, and Peter Kuhn. 2006. "The Narrowing of the U.S. Gender Earnings Gap, 1959-1999: A Cohort-Based Analysis." Working Paper no. 12115 (March), NBER, Cambridge, MA.

Yardley, Jim. 2009. "Indian Women Find New Peace in Rail Commute," *New York Times* September 15, 2009.

Data Appendix

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.

Gender discrimination cases were derived from searching Lexis for "sex! discrimination." Sexual harassment cases are separated in this sample by the authors of the CJP dataset. Therefore, it is possible that sexual harassment cases include some cases with elements of gender discrimination, but the gender discrimination cases would be without elements of sexual harassment.

The data comes from the Westlaw search: "JURY & AWARD! & (SEXUAL/5 HARASS!) & (EMPLOYEE "TITLE VII")". 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. Damages before 1991 are collected via state laws.

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.

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 female employees for sexual harassment in the workplace.

We use weekly earnings as the outcome measure since annual earnings captures two margins – wages and hours worked.

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 CPS 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 ensure comparability, 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.

According to the BLS, "Persons who are neither employed nor unemployed are not in the labor force. This category includes retired persons, students, those taking care of children or other family members and others who are neither working nor seeking work. Information is collected on their desire for and availability for work, job search activity in the prior year, and reasons for not currently searching."

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.[20] Damages are summed at the circuit-year level.[21] If no case with damages occurred in a circuit year, it is coded as 0.[22]

---

[20] Damages are considered reversed only if the decision is completely reversed.
[21] The sum of damages captures the probability that a sexual harasser is caught as well as the damages he pays, ceteris paribus.

To code 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 circuits mentioned in the circuit split as 0 or 1 for years prior to the decision. 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. 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. 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.

Randomization Check

As randomization check, we use data from Boyd, Epstein, and Martin (2010), 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, three are correlated with having a female on the panel and one is 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.[23]

Test for Serial Correlation

As check for possible serial correlation, in Appendix Table C, we regress the number of pro-plaintiff decisions on the previous year's number of pro-plaintiff decisions controlling for circuit and year fixed-effects, circuit-specific time trends, and time-varying circuit characteristics (the probability of a panel being assigned a female Republican appointee, the probability of a panel being assigned at least two male Democratic appointees, and docket size or the number of sexual harassment panels) and their lagged values. Standard errors are clustered at the circuit level. The number of pro-plaintiff decisions is not serially correlated. The instrument, panels with female Republican appointees, is negatively serially correlated in one specification. However, the outcome variable, female to male employment inequality, is positively correlated, so using this instrument would bias the estimates in a negative direction and our IV estimates should provide an underestimate of the true effect.

Test for Endogeneity of Supreme Court decisions resolving circuit splits

In Appendix Table G, we run a distributed lag specification of the number of pro-plaintiff cases as well as *Harris* and *Faragher*. We find that while the number of pro-plaintiff decisions is generally not correlated with gender inequality in advance of the decisions, *Harris* is strongly and positively correlated with gender inequality one and two years in advance of the decision. There are at least two interpretations of this finding. First, gender inequality may respond to appellate or trial decisions in advance of the Supreme Court resolution of the lower courts' decisions. However, gender inequality would have to move in other circuits with legal rules not favored by the Supreme Court resolution of the circuit split and gender inequality would have to not move in circuits with legal rules favored by the resolution of the circuit split. We therefore favor a second interpretation, which suggests that increasingly female friendly environments caused *Harris* and not vice versa. In particular, the circuits that did not previously have the same legal rule as the one favored by the Supreme Court were already trending towards greater gender equality in the year before the Supreme Court resolved the circuit split.

*Major Doctrinal Developments in Sexual Harassment Law*

---

[22] 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).

[23] 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.

1964 – Title VII – prohibits sex discrimination in employment.

1976 – Williams v. Saxbe – Court recognized sexual harassment as a form of sex discrimination when sexual advances by male supervisor towards female employee, if proven, would be deemed an artificial barrier to employment placed before one gender and not another.

1977 – Barnes v. Costle – US Court of Appeals for the Second District ruled that if a female employee was retaliated against for rejecting sexual advances of her boss is a violation of Title VII's prohibition against sex discrimination.

1980 – EEOC issues guidelines forbidding "sexual harassment" as a form of sex discrimination.

1985 - McKinney v. Dole - US Court of Appeals for the DC Circuit ruled that physical violence, even if it is not overtly sexual, can be sexual harassment if the unwelcome conduct is based on the victim's gender.

1986 - Meritor Savings Bank, FSB v. Vinson – The Supreme Court first recognized "sexual harassment" as a violation of Title VII and established the standards for analyzing whether the conduct was welcome and levels of employer liability.

1988 – Hall v. Gus Construction - US Court of Appeals for the Eighth District finds that when male construction workers "hazed" three female colleagues, even if the conduct was not specifically sexual in nature, was gender based harassment.

1991 - Ellison v. Brady – Changed analysis of conduct from reasonable person to reasonable women test when determining whether actionable sexual harassment occurred.

1991 – Civil Rights Act of 1991 provides for jury trials and for increased damages in Title VII sexual harassment suits.

1993 - Harris v. Forklift Systems, Inc – plaintiff may bring sexual harassment claim without necessarily showing psychological harm. In addition to Meritor, the factors when analyzing whether sexual harassment occurred include:
o Frequency of conduct
o Its Severity
o Whether the conduct is physically threatening or humiliating
o Or is a mere offensive utterance
o And whether the conduct unreasonably interferes with employees work performance
o No Single Factor is Required but Totality of the Circumstances Test

1998 - Faragher v. City of Boca Raton - Supreme Court decision that establishes that an employer is subject to vicarious liability for hostile environment created by a supervisor unless the employer can demonstrate that it exercised reasonable care to prevent and correct promptly any sexually harassing behavior and that the employee unreasonably failed to take advantage of any preventative or corrective opportunities provided by the employer.

1998 - Burlington Industries, Inc v. Ellerth - Companion Supreme Court decision to Faragher that further elaborates that the employer's "Faragher" defense to vicarious liability is not available if the employee suffers a tangible job consequence as result of supervisor's actions.

Figure 1: Sexual Harassment Decisions, 1982-2002

- Number of Sexual Harassment Decisions
- Number of Pro-Plaintiff Sexual Harassment Decisions
- Number of Pro-Defendant Sexual Harassment Decisions
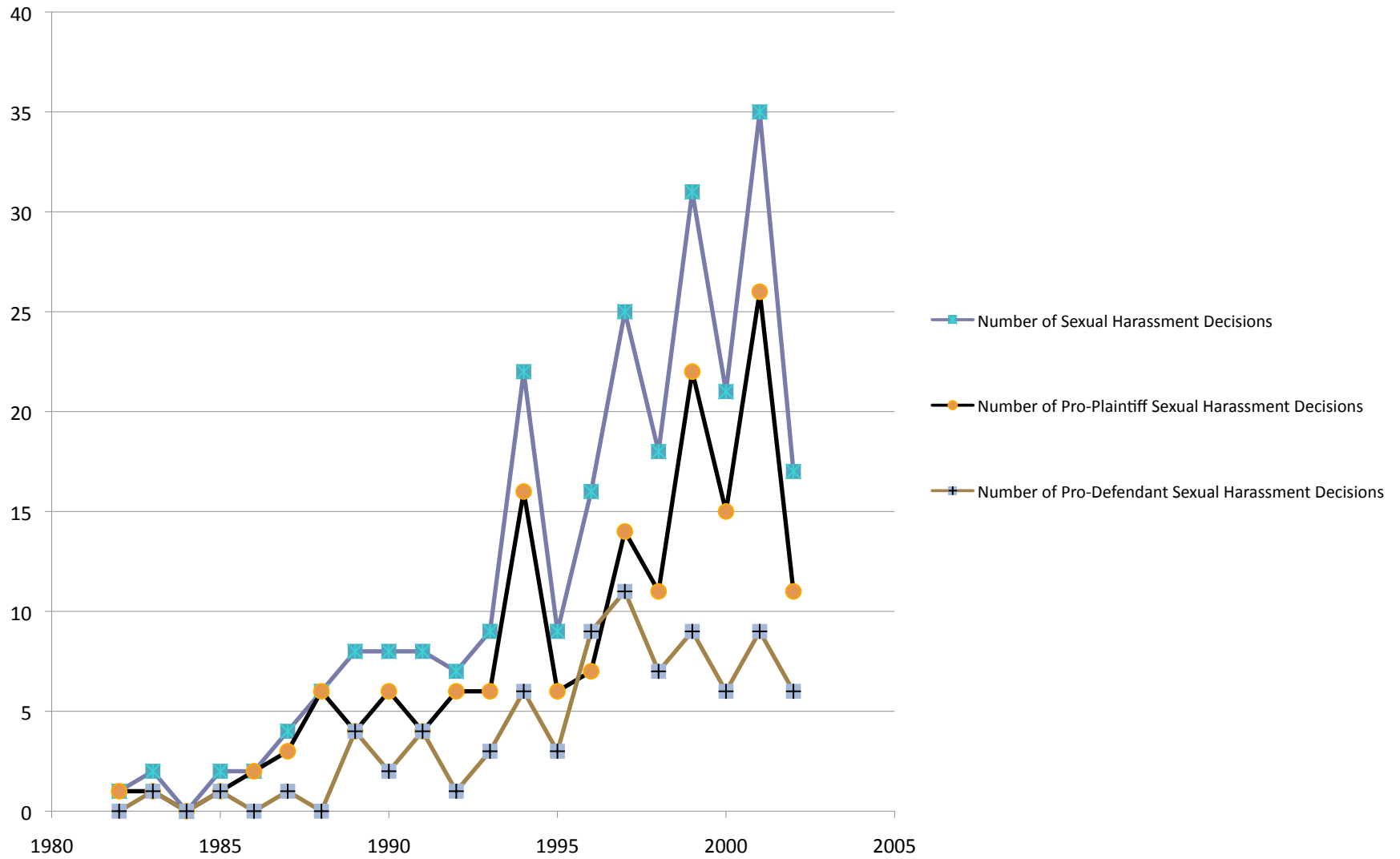
Figure 2: Sexual Harassment Decisions, Sexual Harassment Damages, and Appellate Terminations, 1982-2002
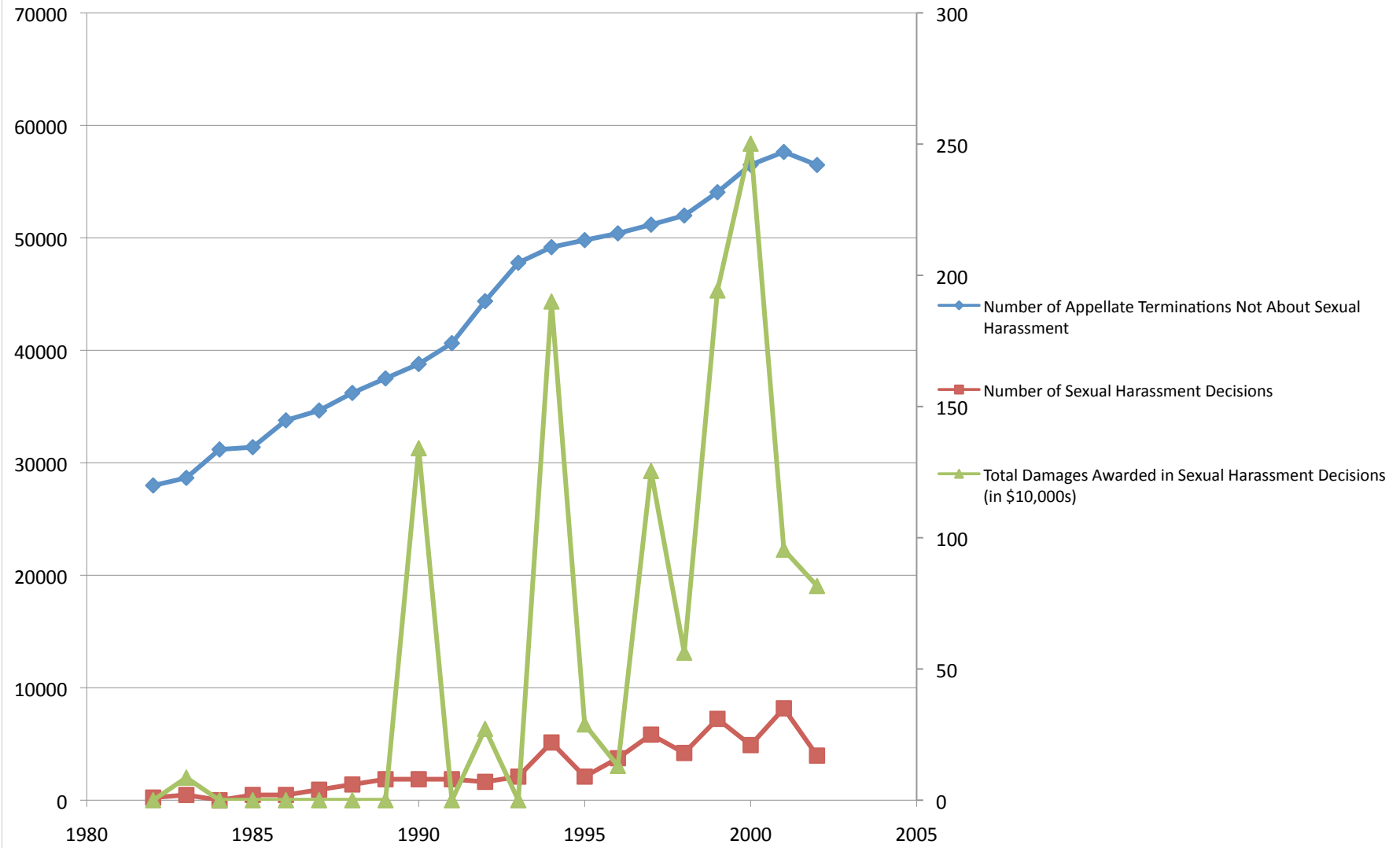
Figure 3: Gender Discrimination and Sexual Harassment Decisions, 1995-2002
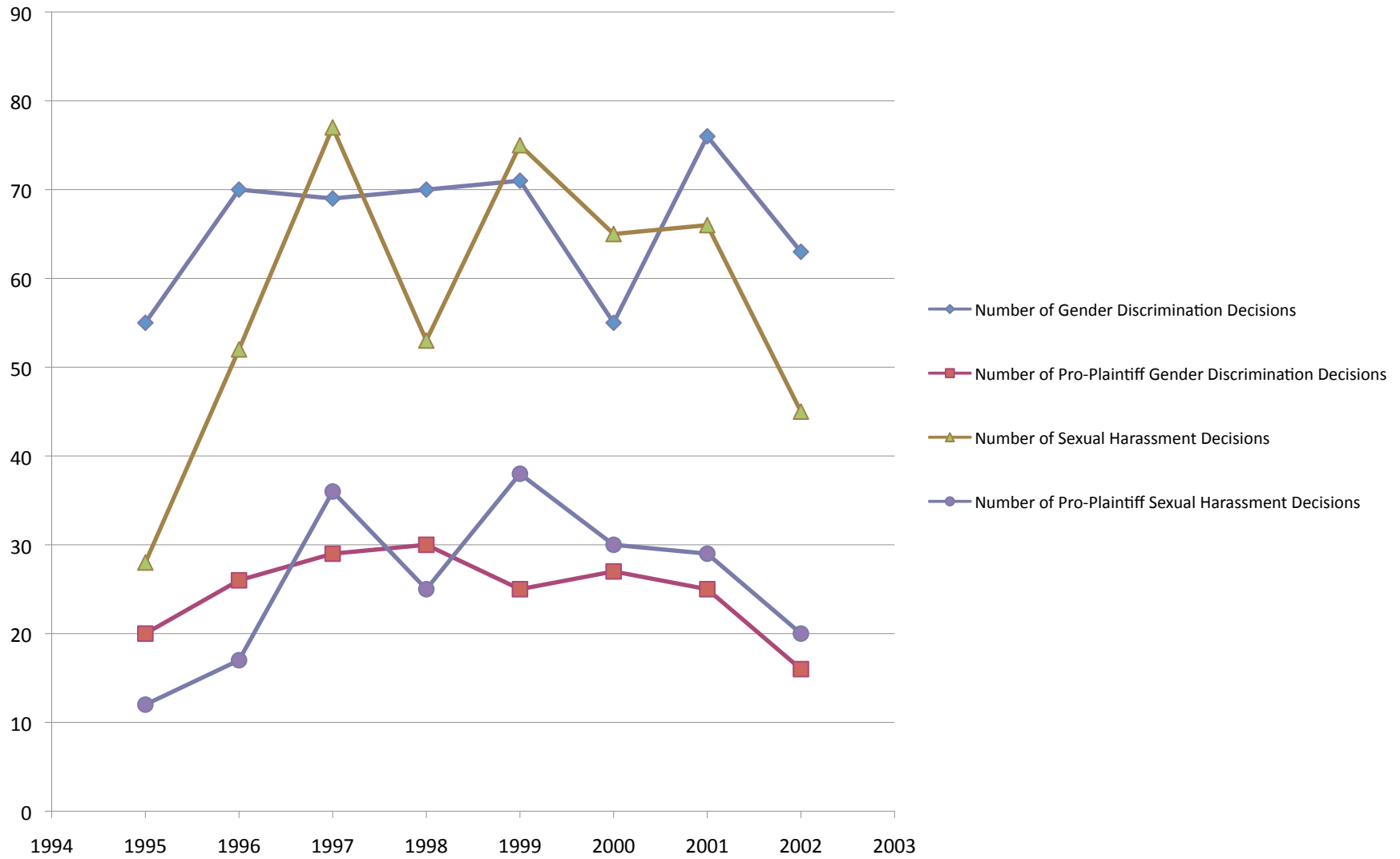
Figure 4: Judicial Composition and Random Assignment, 1982-2002

- Probability of a panel being assigned a female judge
- Probability of a panel being assigned a Democratic appointee
- Actual proportion of panels with a female judge
- Actual proportion of panels with a Democratic appointee

Figure 5

First Stage: Impact of Treatment on Pro-Plaintiff Decisions
Yearly Leads and Lags from 1 Year Before to 5 Years After Treatment
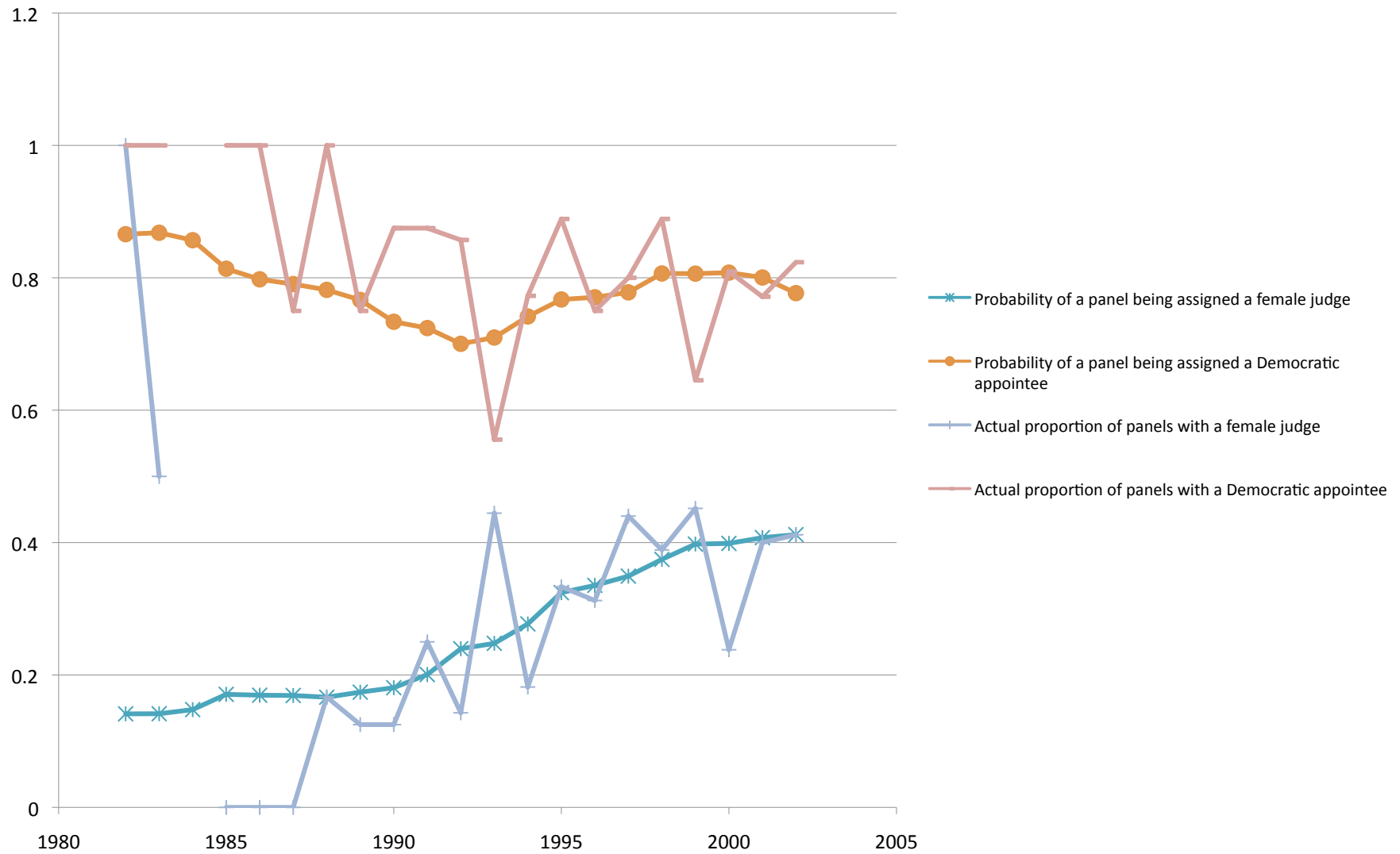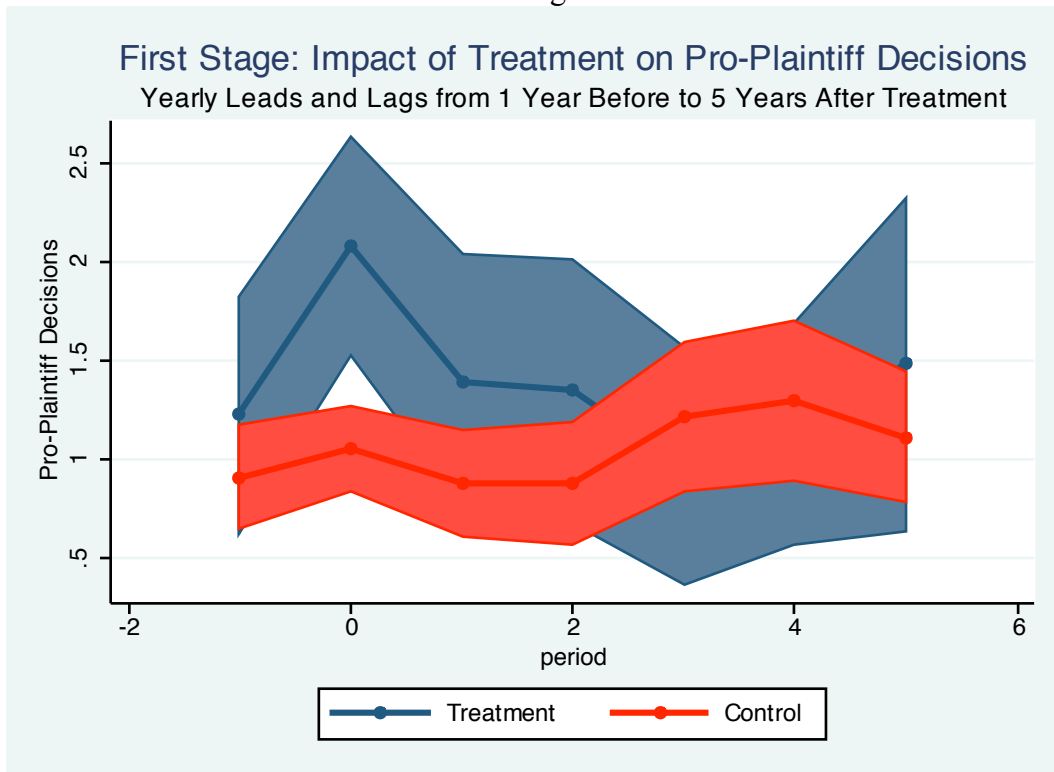
Figure 6

Pro-Plaintiff Decisions and Wage Inequality by Circuit
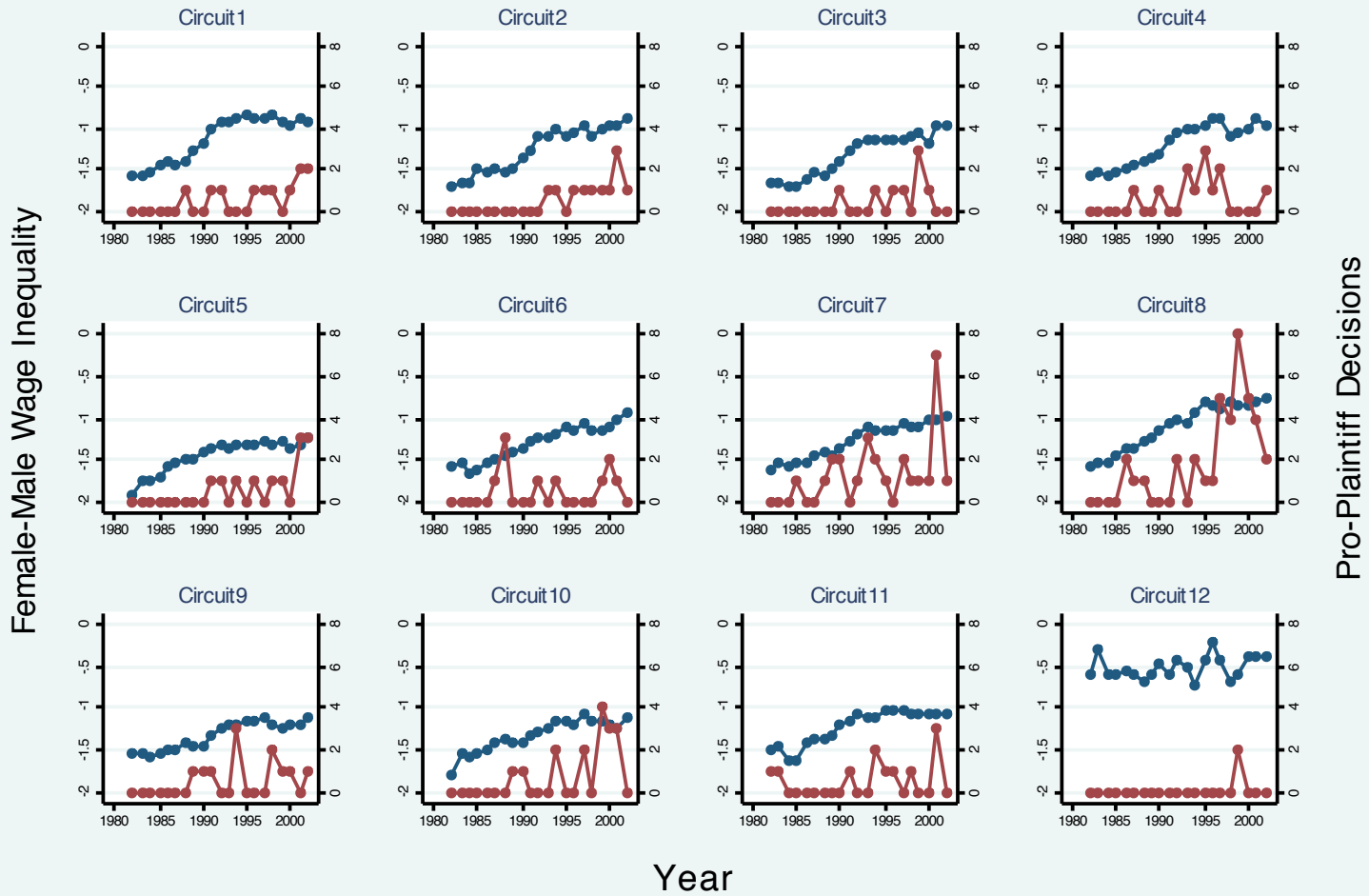
Figure 7

Treatment and Wage Inequality by Circuit
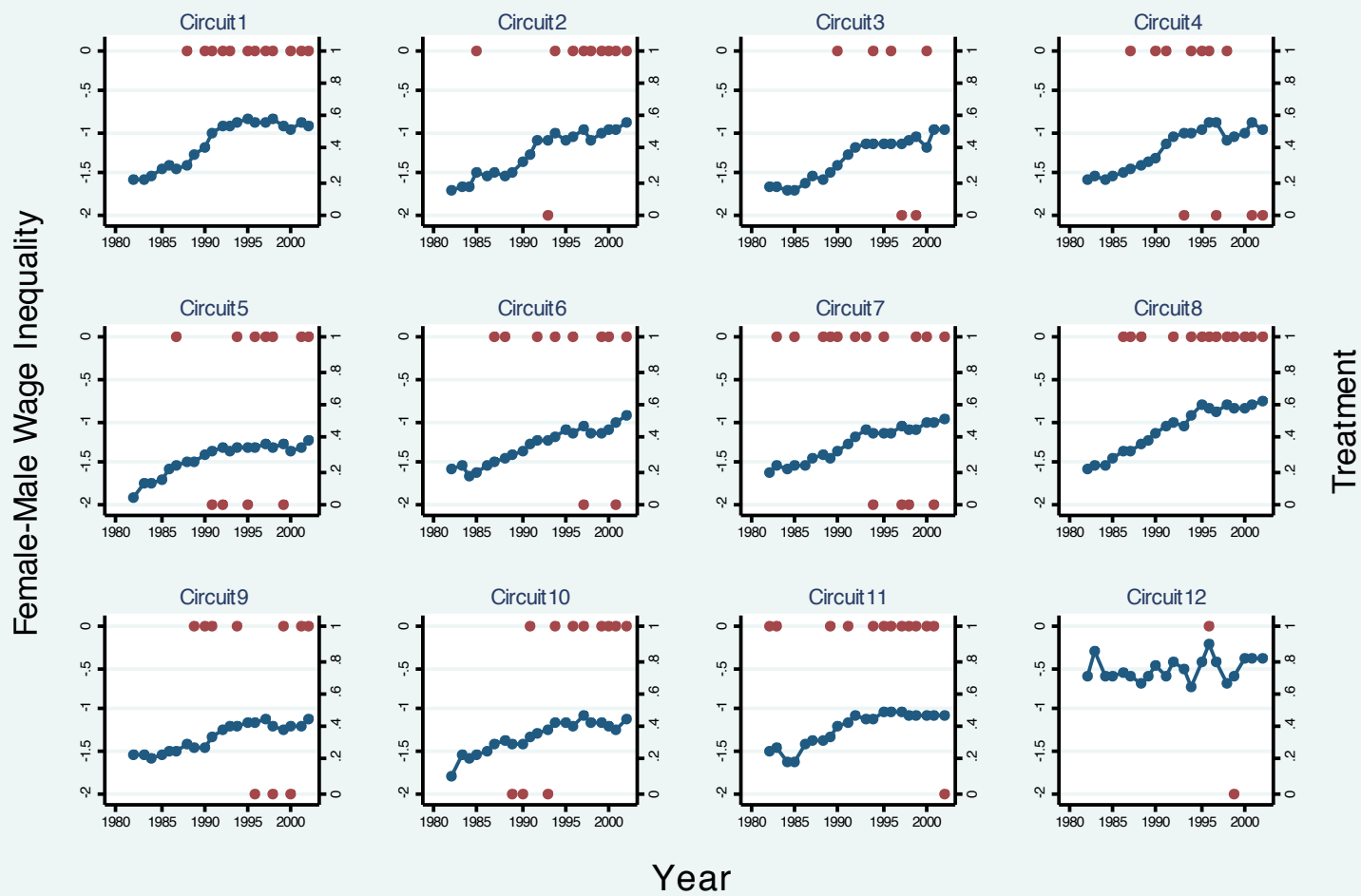
Figure 8

The Effects of Sexual Harassment Law on Employment Inequality

95% CI	Fitted values
Female/Male Employment Inequality	

The Effects of Sexual Harassment Law on Hours Inequality

95% CI	Fitted values
Female/Male Hours Inequality	
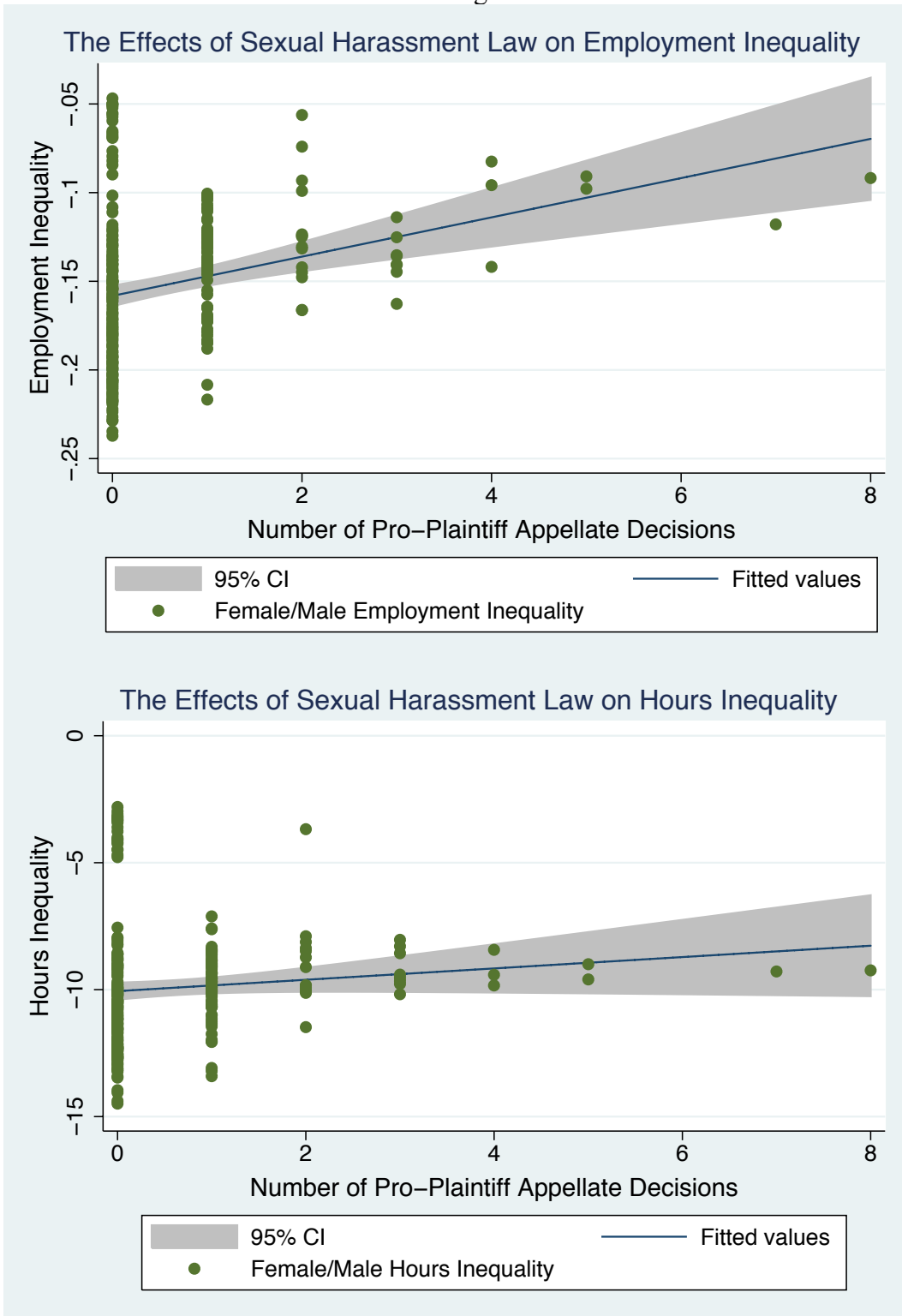
The Effects of Sexual Harassment Law on Earnings Inequality

Earnings Inequality

Number of Pro–Plaintiff Appellate Decisions

95% CI
Female/Male Earnings Inequality
Fitted values
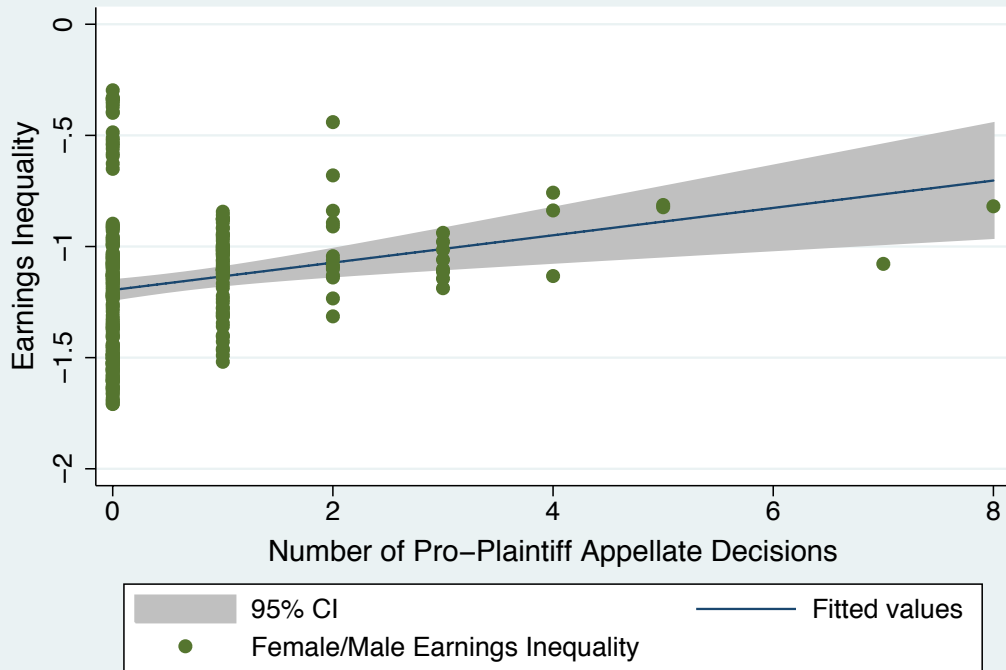
The Effects of Sexual Harassment Law on Management Inequality

Management Inequality

Number of Pro–Plaintiff Appellate Decisions

95% CI
Female/Male Management Inequality
Fitted values
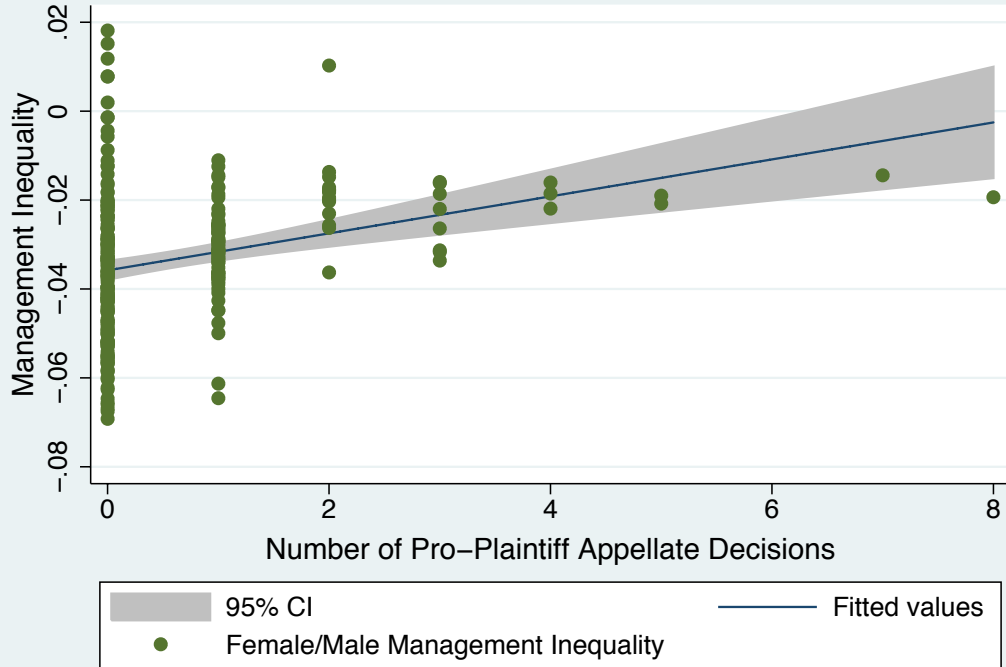
Figure 9: Dynamic Response following Pro-Plaintiff Decisions on Presence of Sexual Hararassment Policies
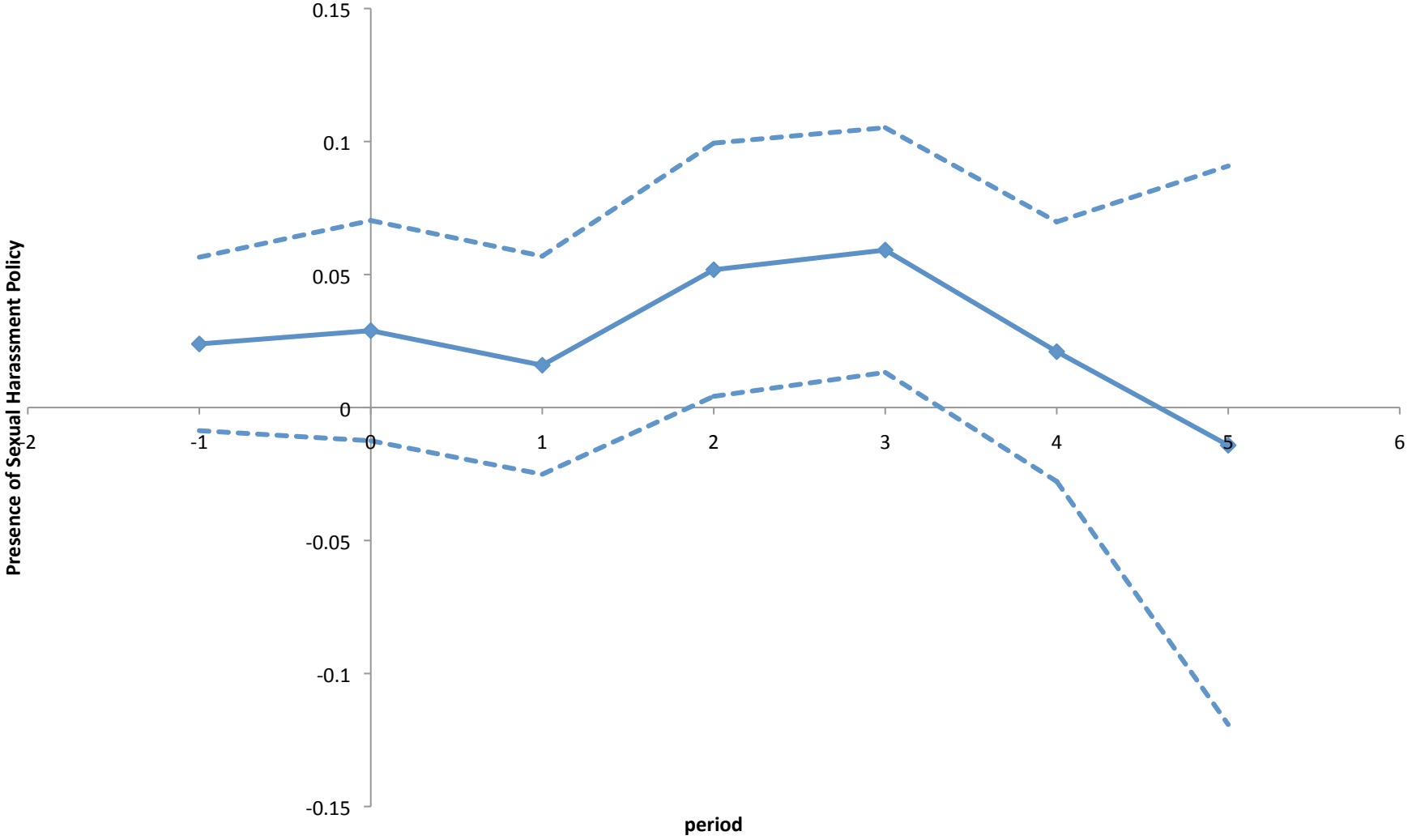
Figure 10: Dynamic Response following Pro-Plaintiff Decisions on Female-Male Employment Inequality
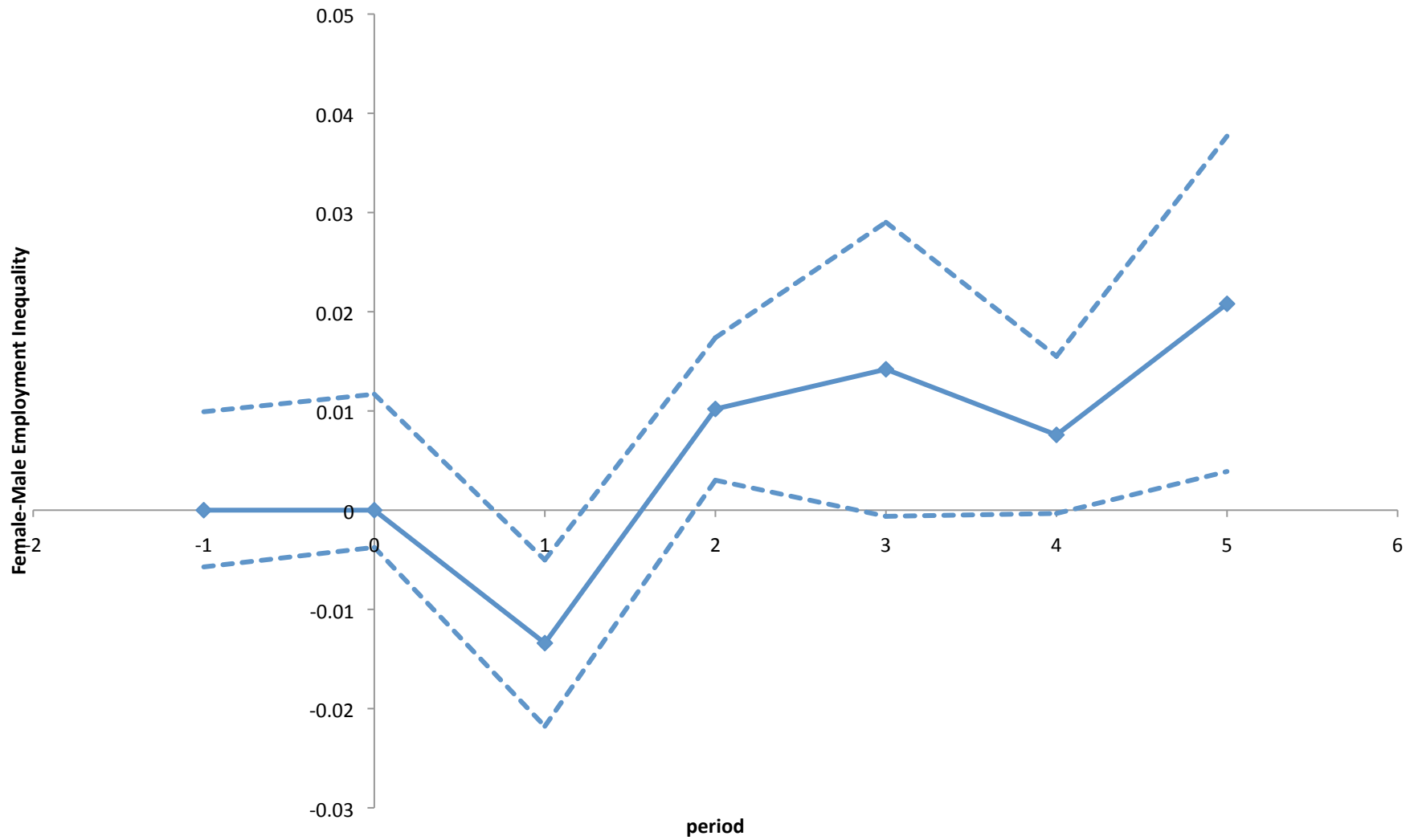
Table 1 -- First Stage: Relationship Between Pro-Plaintiff Sexual Harassment Appellate Decisions and Composition of Sexual Harassment Panels, 1982-2002

Panel A: Judge Level	Pro-plaintiff Decision					
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0230		-0.0600			
	(0.0460)		(0.0479)			
Democratic Appointee		0.112*	0.120*			
		(0.0391)	(0.0413)			
Female Republican Appointee				-0.115+		-0.0832
				(0.0629)		(0.0542)
Male Democratic Appointee					0.114*	0.110*
					(0.0373)	(0.0367)
N	752	752	752	752	752	752
R-sq	0.101	0.114	0.115	0.103	0.113	0.114

Notes: Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. These regressions control for circuit fixed-effects, year fixed-effects, and the probability of a panel being assigned a judge of the type under consideration: female (Columns 1 and 3), Democratic Appointee (Columns 2 and 3), Female Republican Appointee (Columns 4 and 6), and Male Democratic Appointee (Columns 5 and 6).

Panel B: Case Level	Pro-Plaintiff Decision		
	(1)	(2)	(3)
At Least One Female Republican Appointee	-0.150+		-0.124
	(0.0799)		(0.0736)
At Least Two Male Democratic Appointees		0.173*	0.158+
		(0.0740)	(0.0733)
N	251	251	251
R-sq	0.100	0.110	0.116

Notes: Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. These regressions control for circuit fixed-effects, year fixed-effects, and the probability of a panel being assigned at least one female Republican Appointee (Columns 1 and 3) and the probability of a panel being assigned at least two male Democratic Appointees (Columns 2 and 3).

Panel C: Circuit-Year Level	Number of Pro-Plaintiff Sexual Harassment Appellate Decisions					
	(1)	(2)	(3)	(4)	(5)	(6)
Sexual Harassment Panels with At Least One Female Republican Appointee	0.356		0.518	-0.324*		-0.243+
	(0.375)		(0.324)	(0.133)		(0.124)
Sexual Harassment Panels with At Least Two Male Democratic Appointees		0.773**	0.834**		0.261+	0.199
		(0.196)	(0.168)		(0.142)	(0.134)
Normalization	Docket	Docket	Docket	Panels	Panels	Panels
N	252	252	252	252	252	252
R-sq	0.629	0.691	0.711	0.879	0.879	0.883

Notes: Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. These regressions control for circuit fixed-effects, year fixed-effects, circuit-specific time trends, the probability of a panel being assigned a female Republican appointee, the probability of a panel being assigned at least two male Democratic appointees, a dummy for whether there were no cases in that circuit-year to minimize mechanical correlation, and Docket Size in Columns 1-3 or the Number of Sexual Harassment Panels in Columns 4-6. 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 Law on Gender Inequality, 1982-2002

	Employment Status				Hours Worked Last Week			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Pro-Plaintiff	-0.00585**	-0.00543*	-0.00683**	-0.00490*	-0.192**	-0.166*	-0.243**	-0.132+
Appellate Decisions	(0.00144)	(0.00199)	(0.00158)	(0.00160)	(0.0451)	(0.0712)	(0.0624)	(0.0721)
Number of Pro-Plaintiff	0.0114**	0.0104**	0.0114**	0.0101**	0.356**	0.305**	0.378*	0.310*
Appellate Decisions * Sex	(0.00334)	(0.00322)	(0.00314)	(0.00305)	(0.100)	(0.0941)	(0.143)	(0.140)
Sex	-0.171**	-0.163**	-0.171**	-0.163**	-12.68**	-12.23**	-12.69**	-12.23**
	(0.0198)	(0.0176)	(0.0204)	(0.0183)	(0.627)	(0.530)	(0.631)	(0.537)
Normalization	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket
Controls	N	Y	N	Y	N	Y	N	Y
IV	N	N	Y	Y	N	N	Y	Y
N	5409838	5398935	5409838	5398935	5219153	5208419	5219153	5208419
R-sq	0.040	0.098	0.040	0.098	0.079	0.137	0.079	0.137

	Log Real Weekly Earnings				Managerial Status			
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Number of Pro-Plaintiff	-0.0340**	-0.0319*	-0.0422**	-0.0254+	-0.00245**	-0.00163**	-0.00304**	-0.00180**
Appellate Decisions	(0.00774)	(0.0115)	(0.0123)	(0.0123)	(0.000418)	(0.000342)	(0.000700)	(0.000534)
Number of Pro-Plaintiff	0.0673**	0.0598*	0.0649*	0.0551*	0.00412**	0.00314**	0.00419**	0.00302**
Appellate Decisions * Sex	(0.0204)	(0.0195)	(0.0237)	(0.0227)	(0.000966)	(0.000767)	(0.000915)	(0.000798)
Sex	-1.354**	-1.297**	-1.352**	-1.293**	-0.0472**	-0.0416**	-0.0473**	-0.0415**
	(0.122)	(0.109)	(0.128)	(0.115)	(0.00588)	(0.00501)	(0.00615)	(0.00523)
Normalization	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket
Controls	N	Y	N	Y	N	Y	N	Y
IV	N	N	Y	Y	N	N	Y	Y
N	4938394	4927744	4938394	4927744	4338621	4334364	4338621	4334364
R-sq	0.055	0.137	0.055	0.137	0.004	0.062	0.004	0.062

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include circuit fixed-effects, year fixed-effects, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and normalization factor (Docket Size). Additional controls included in even numbered columns are individual controls (age, race dummies, educational attainment dummies, and marital status dummy) and circuit-specific time trends. Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Circuit-by-year controls (judicial pool characteristics and the normalization factor) are interacted with the female dummy as are the measure of sexual harassment law (pro-plaintiff) and instruments. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 3 -- The Effect of Sexual Harassment Law on Gender Inequality: Precedent

	Employment Status			Hours Worked Last Week	
Sample:	Highly Cited	Lowly Cited		Highly Cited	Lowly Cited
			Panels > 0		
	(1)	(2)		(3)	(4)
Number of Pro-Plaintiff	-0.00252*	-0.00551**		-0.0814+	-0.181*
Appellate Decisions	(0.00113)	(0.00162)		(0.0449)	(0.0747)
Number of Pro-Plaintiff	0.00511*	0.00549*		0.174**	0.101
Appellate Decisions * Sex	(0.00221)	(0.00257)		(0.0609)	(0.105)
Sex	-0.172**	-0.126**		-12.54**	-11.18**
	(0.0129)	(0.0121)		(0.464)	(0.463)
Normalization	Docket	Docket		Docket	Docket
Controls	Y	Y		Y	Y
IV	N	N		N	N
N	1420281	1251686		1374408	1208724
R-sq	0.089	0.094		0.126	0.128

	Log Real Weekly Earnings			Managerial Status	
Sample:	Highly Cited	Lowly Cited		Highly Cited	Lowly Cited
			Panels > 0		
	(5)	(6)		(7)	(8)
Number of Pro-Plaintiff	-0.0146+	-0.0299**		-0.00180**	0.000838
Appellate Decisions	(0.00749)	(0.0109)		(0.000371)	(0.000585)
Number of Pro-Plaintiff	0.0288*	0.0245		0.00238**	0.00180*
Appellate Decisions * Sex	(0.0139)	(0.0168)		(0.000710)	(0.000811)
Sex	-1.333**	-1.084**		-0.0488**	-0.0345**
	(0.0864)	(0.0882)		(0.00381)	(0.00401)
Normalization	Docket	Docket		Docket	Docket
Controls	Y	Y		Y	Y
IV	N	N		N	N
N	1299783	1139887		1159394	1063353
R-sq	0.128	0.132		0.061	0.059

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (age, race dummies, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and normalization factor (Docket Size). Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Odd numbered columns restrict to circuit-years that have above median citation per case impact and even numbered columns restrict to circuit-years that have below median citation per case impact. The median citations per case in a circuit-year is 17.3. The sample is also restricted to circuit-years with at least one sexual harassment case. Circuit-by-year controls (judicial pool characteristics and the normalization factor) are interacted with the female dummy as is the measure of sexual harassment law (pro-plaintiff). + Significant at 10%; * Significant at 5%; ** Significant at 1%

	Employment Status			Hours Worked			Earnings			Managerial Status		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Number of Pro-Plaintiff Appellate Decisions$_{t+1}$ * Sex	0.000767 (0.00139)	0.00210 (0.00391)		-0.0327 (0.0547)	0.0525 (0.155)		0.00258 (0.0106)	0.00952 (0.0236)		0.00127* (0.000413)	0.00119 (0.000940)	
Number of Pro-Plaintiff Appellate Decisions$_t$ * Sex	0.000611 (0.000865)	0.00397 (0.00386)		0.0468 (0.0550)	0.187 (0.161)		0.00268 (0.00579)	0.0213 (0.0299)		0.000726 (0.000540)	0.00158+ (0.000806)	
Number of Pro-Plaintiff Appellate Decisions$_{t-1}$ * Sex	0.00139 (0.00118)	-0.0134** (0.00419)		0.0587 (0.0514)	-0.511+ (0.261)		0.00451 (0.00801)	-0.106* (0.0367)		-0.000463 (0.000431)	-0.00162 (0.00143)	
Number of Pro-Plaintiff Appellate Decisions$_{t-2}$ * Sex	0.00290* (0.00121)	0.0102* (0.00359)		0.133* (0.0494)	0.388 (0.220)		0.0197* (0.00711)	0.0812* (0.0300)		0.000453 (0.000440)	0.000510 (0.00120)	
Number of Pro-Plaintiff Appellate Decisions$_{t-3}$ * Sex	0.000660 (0.00196)	0.0142+ (0.00741)		0.0539 (0.0800)	0.762* (0.331)		0.00668 (0.0134)	0.0960 (0.0564)		0.00174* (0.000638)	0.00199 (0.00249)	
Number of Pro-Plaintiff Appellate Decisions$_{t-4}$ * Sex	0.00237 (0.00195)	0.00759+ (0.00396)		0.0542 (0.0831)	0.407* (0.170)		0.0139 (0.0124)	0.0682* (0.0288)		0.00135 (0.000760)	0.00329 (0.00183)	
Number of Pro-Plaintiff Appellate Decisions$_{t-5}$ * Sex	0.000533 (0.00214)	0.0208* (0.00845)		-0.0958 (0.0872)	0.864+ (0.436)		-0.0106 (0.0140)	0.161* (0.0691)		0.0000266 (0.000755)	0.00176 (0.00277)	
Number of Pro-Defendant Appellate Decisions$_{t+1}$ * Sex			-0.0000798 (0.00126)			0.0000210 (0.0628)			0.00430 (0.00751)			-0.00162 (0.00100)
Number of Pro-Defendant Appellate Decisions$_t$ * Sex			-0.000863 (0.00191)			-0.0494 (0.111)			-0.0103 (0.0125)			-0.000829 (0.00147)
Number of Pro-Defendant Appellate Decisions$_{t-1}$ * Sex			-0.00599* (0.00259)			-0.314+ (0.159)			-0.0431* (0.0154)			0.000616 (0.000703)
Number of Pro-Defendant Appellate Decisions$_{t-2}$ * Sex			-0.00633* (0.00273)			-0.284 (0.166)			-0.0356+ (0.0169)			-0.000534 (0.00110)
Number of Pro-Defendant Appellate Decisions$_{t-3}$ * Sex			-0.00399 (0.00253)			-0.146 (0.172)			-0.0285 (0.0175)			0.00122 (0.00161)
Number of Pro-Defendant Appellate Decisions$_{t-4}$ * Sex			-0.00513 (0.00307)			-0.136 (0.164)			-0.0328 (0.0210)			0.000865 (0.00116)
Number of Pro-Defendant Appellate Decisions$_{t-5}$ * Sex			-0.00796** (0.00233)			-0.251 (0.145)			-0.0484* (0.0173)			0.00244 (0.00171)
Normalization	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
IV	N	Y	N	N	Y	N	N	Y	N	N	Y	N
N	3736671	3736671	3736671	3608012	3608012	3608012	3410738	3410738	3410738	3159884	3159884	3159884
R-sq	0.095	0.095	0.095	0.132	0.131	0.132	0.133	0.133	0.133	0.060	0.060	0.060

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (age, race dummies, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and the normalization factors (a dummy for whether there were no cases in that circuit-year and Docket Size). Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Circuit-by-year controls (judicial pool characteristics and the normalization factors) are lagged and interacted with the female dummy as are the measure of sexual harassment law (pro-plaintiff or pro-defendant) and instruments. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 5 -- The Effect of Sexual Harassment Law on Gender Inequality: Cumulative

	Employment Status		Hours Worked		Earnings		Managerial Status	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cumulative Pro-Plaintiff	0.00345**	0.00462**	0.101*	0.157*	0.0193**	0.0271**	0.000646**	0.000834**
Appellate Decisions * Sex	(0.000702)	(0.00130)	(0.0371)	(0.0622)	(0.00405)	(0.00815)	(0.000145)	(0.000171)
Cumulative Pro-Defendant	0.000396	-0.00274	-0.0133	-0.150	-0.000841	-0.0227	0.0000360	-0.0000871
Appellate Decisions * Sex	(0.00174)	(0.00245)	(0.0793)	(0.0931)	(0.0105)	(0.0145)	(0.000680)	(0.000828)
Normalization	Cumulative Docket		Cumulative Docket		Cumulative Docket		Cumulative Docket	
Controls	Y	Y	Y	Y	Y	Y	Y	Y
IV	N	Y	N	Y	N	Y	N	Y
N	5398935	5398935	5208419	5208419	4927744	4927744	4334364	4334364
R-sq	0.099	0.099	0.137	0.137	0.137	0.137	0.062	0.062

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (age, race dummies, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and the normalization factor (Docket Size). Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Judicial pool characteristics are calculated as a weighted average probability over all years where the weights are the number of sexual harassment panels in a particular year and then interacted with the female dummy as are the cumulative measures of sexual harassment law (pro-plaintiff or pro-defendant), instruments, and normalization factor (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. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 6 -- The Effect of Sexual Harassment Law on Gender Inequality: Percentage

	Employment Status			Hours Worked			Earnings			Managerial Status		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Percentage of Pro-Plaintiff Appellate Decisions$_{t+1}$ * Sex	-0.000760	-0.00689		-0.0479	-0.446+		-0.0124	-0.0723+		0.00348*	0.00598+	
	(0.00274)	(0.00474)		(0.0890)	(0.241)		(0.0177)	(0.0388)		(0.00116)	(0.00282)	
Percentage of Pro-Plaintiff Appellate Decisions$_t$ * Sex	-0.0000119	-0.00628		0.0509	-0.450		0.00506	-0.0583+		0.000771	-0.00461	
	(0.00281)	(0.00403)		(0.0959)	(0.332)		(0.0140)	(0.0302)		(0.00105)	(0.00446)	
Percentage of Pro-Plaintiff Appellate Decisions$_{t-1}$ * Sex	0.00740+	0.00569		0.375*	0.225		0.0547*	0.0374		-0.000997	-0.00855	
	(0.00385)	(0.00893)		(0.128)	(0.491)		(0.0217)	(0.0615)		(0.00110)	(0.00745)	
Percentage of Pro-Plaintiff Appellate Decisions$_{t-2}$ * Sex	0.00584	0.0194		0.385*	1.266*		0.0467+	0.169+		0.000855	0.0117*	
	(0.00408)	(0.0110)		(0.153)	(0.500)		(0.0221)	(0.0769)		(0.00110)	(0.00446)	
Percentage of Pro-Plaintiff Appellate Decisions$_{t-3}$ * Sex	0.000818	0.0103		0.137	0.730		0.0174	0.0522		0.00166	0.00775	
	(0.00421)	(0.00921)		(0.172)	(0.649)		(0.0244)	(0.0860)		(0.00178)	(0.00902)	
Percentage of Pro-Plaintiff Appellate Decisions$_{t-4}$ * Sex	0.00466	0.00285		0.119	0.0316		0.0307	0.0403		0.000379	-0.00143	
	(0.00492)	(0.00872)		(0.141)	(0.466)		(0.0286)	(0.0693)		(0.00117)	(0.00394)	
Percentage of Pro-Plaintiff Appellate Decisions$_{t-5}$ * Sex	0.00386	0.00648		0.0595	0.326		0.0143	0.0491		-0.000213	0.00183	
	(0.00356)	(0.0148)		(0.119)	(0.593)		(0.0163)	(0.0931)		(0.00118)	(0.00924)	
Percentage of Pro-Defendant Appellate Decisions$_{t+1}$ * Sex			0.000760			0.0479			0.0124			-0.00348*
			(0.00274)			(0.0890)			(0.0177)			(0.00116)
Percentage of Pro-Defendant Appellate Decisions$_t$ * Sex			0.0000119			-0.0509			-0.00506			-0.000771
			(0.00281)			(0.0959)			(0.0140)			(0.00105)
Percentage of Pro-Defendant Appellate Decisions$_{t-1}$ * Sex			-0.00740+			-0.375*			-0.0547*			0.000997
			(0.00385)			(0.128)			(0.0217)			(0.00110)
Percentage of Pro-Defendant Appellate Decisions$_{t-2}$ * Sex			-0.00584			-0.385*			-0.0467+			-0.000855
			(0.00408)			(0.153)			(0.0221)			(0.00110)
Percentage of Pro-Defendant Appellate Decisions$_{t-3}$ * Sex			-0.000818			-0.137			-0.0174			-0.00166
			(0.00421)			(0.172)			(0.0244)			(0.00178)
Percentage of Pro-Defendant Appellate Decisions$_{t-4}$ * Sex			-0.00466			-0.119			-0.0307			-0.000379
			(0.00492)			(0.141)			(0.0286)			(0.00117)
Percentage of Pro-Defendant Appellate Decisions$_{t-5}$ * Sex			-0.00386			-0.0595			-0.0143			0.000213
			(0.00356)			(0.119)			(0.0163)			(0.00118)
Normalization	Panels	Panels	Panels	Panels	Panels	Panels	Panels	Panels	Panels	Panels	Panels	Panels
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
IV	N	Y	N	N	Y	N	N	Y	N	N	Y	N
N	3736671	3736671	3736671	3608012	3608012	3608012	3410738	3410738	3410738	3159884	3159884	3159884
R-sq	0.095	0.095	0.095	0.132	0.131	0.132	0.133	0.133	0.133	0.060	0.060	0.060

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (age, race dummies, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and the normalization factors (a dummy for whether there were no cases in that circuit-year and number of sexual harassment panels). Controlling for the number of sexual harassment panels turns the measure of sexual harassment law into a measure of the percent of sexual harassment cases that are pro-plaintiff (or pro-defendant). Circuit-by-year controls (judicial pool characteristics and the normalization factors) are lagged and interacted with the female dummy as are the measure of sexual harassment law (pro-plaintiff or pro-defendant) and instruments. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 7 -- Falsification: The Effect of Sexual Harassment Law on Race Inequality, 1982-2002

	Employment Status	Hours Worked Last Week	Log Real Weekly Earnings	Managerial Status
	(1)	(2)	(3)	(4)
Number of Pro-Plaintiff	0.000418	0.0243	0.00601	-0.000241
Appellate Decisions	(0.000858)	(0.0423)	(0.00611)	(0.000438)
Number of Pro-Plaintiff	-0.000817	-0.00431	-0.00971	-0.000602
Appellate Decisions * Non-White	(0.00241)	(0.0971)	(0.0138)	(0.000991)
Non-White	-0.0837**	-3.860**	-0.437**	-0.0281**
	(0.0203)	(0.707)	(0.100)	(0.00350)
Normalization	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y
IV	Y	Y	Y	Y
N	5398935	5208419	4927744	4334364
R-sq	0.098	0.136	0.136	0.061

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (gender, age, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and normalization factor (Docket Size). Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Circuit-by-year controls (judicial pool characteristics and the normalization factor) are interacted with the non-white dummy as are the measure of sexual harassment law (pro-plaintiff) and instruments. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 8 -- The Effect of Sexual Harassment Law on Insiders vs. Outsiders

	Employment Status				Hours Worked Last Week			
Sample:	Population	Labor Force	Hi Attachment	Low Attachment	Population	Labor Force	Hi Attachment	Low Attachment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Pro-Plaintiff	-0.00490*	0.000222	-0.00575**	-0.00446*	-0.132+	0.0434	-0.177+	-0.138
Appellate Decisions	(0.00160)	(0.000524)	(0.00180)	(0.00183)	(0.0721)	(0.0378)	(0.0893)	(0.0846)
Number of Pro-Plaintiff	0.0101**	-0.00105	0.0129**	0.00847*	0.310*	-0.0751	0.433*	0.304
Appellate Decisions * Sex	(0.00305)	(0.000829)	(0.00334)	(0.00346)	(0.140)	(0.0682)	(0.148)	(0.175)
Sex	-0.163**	0.0136**	-0.211**	-0.130**	-12.23**	-6.747**	-16.34**	-9.201**
	(0.0183)	(0.00308)	(0.0259)	(0.0145)	(0.537)	(0.232)	(0.896)	(0.418)
Normalization	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y	Y	Y	Y	Y
IV	Y	Y	Y	Y	Y	Y	Y	Y
N	5398935	4163294	2651129	2747806	5208419	3968429	2540653	2667766
R-sq	0.098	0.035	0.148	0.088	0.137	0.084	0.199	0.121

	Log Real Weekly Earnings				Managerial Status			
Sample:	Population	Labor Force	Hi Attachment	Low Attachment	Population	Labor Force	Hi Attachment	Low Attachment
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Number of Pro-Plaintiff	-0.0254+	0.00230	-0.0346*	-0.0223	-0.00180**	-0.00169**	-0.00316**	-0.00106+
Appellate Decisions	(0.0123)	(0.00421)	(0.0149)	(0.0128)	(0.000534)	(0.000487)	(0.000781)	(0.000581)
Number of Pro-Plaintiff	0.0551*	-0.00496	0.0786*	0.0454+	0.00302**	0.00280**	0.00593**	0.00154
Appellate Decisions * Sex	(0.0227)	(0.00625)	(0.0257)	(0.0244)	(0.000798)	(0.000805)	(0.00109)	(0.00100)
Sex	-1.293**	-0.359**	-1.773**	-0.974**	-0.0415**	-0.0401**	-0.0861**	-0.00285
	(0.115)	(0.0338)	(0.178)	(0.0821)	(0.00523)	(0.00528)	(0.00961)	(0.00344)
Normalization	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y	Y	Y	Y	Y
IV	Y	Y	Y	Y	Y	Y	Y	Y
N	4927744	3687754	2321668	2606076	4334364	3939156	2136392	2197972
R-sq	0.137	0.127	0.200	0.119	0.062	0.061	0.035	0.039

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (age, race dummies, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and normalization factor (Docket Size). Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Circuit-by-year controls (judicial pool characteristics and the normalization factor) are interacted with the female dummy as are the measure of sexual harassment law (pro-plaintiff) and instruments. Columns 2, 6, 10, and 14 restrict to the sample of individuals who are in the labor force; Columns 3, 7, 11, and 15 restrict to the sample of individuals who are predicted to have high labor force attachment; Columns 4, 8, 12, and 16 restrict to the sample of individuals who are predicted to have low labor force attachment. These predictions are based on an OLS regression of not-in-labor force status on individual controls and the cut-off value is at the median, 0.04 probability to be not in the labor force. Older, non-minorities, married, and more educated individuals are more likely to be in the labor force. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 9 -- Decomposing the Effect of Sexual Harassment Law on the Gender Wage Gap, 1982-2002

Sample:	Log Real Weekly Earnings						
	Population (1)	Population (2)	Population (3)	Population (4)	Population (5)	Population (6)	Non-Zero Wages (7)
Number of Pro-Plaintiff	-0.0254+	-0.0200*	-0.00626	-0.000579	-0.000265	-0.00508	-0.00149
Appellate Decisions	(0.0123)	(0.00797)	(0.00427)	(0.00239)	(0.00255)	(0.00401)	(0.00316)
Number of Pro-Plaintiff	0.0551*	0.0451**	0.0137+	0.00343	0.00242	0.00949	0.00649
Appellate Decisions * Sex	(0.0227)	(0.0134)	(0.00678)	(0.00270)	(0.00286)	(0.00641)	(0.00380)
Sex	-1.293**	-0.837**	-0.407**	-0.0461*	-0.0756**	-0.487**	-0.498**
	(0.115)	(0.0633)	(0.0461)	(0.0191)	(0.0208)	(0.0462)	(0.0329)
Managerial Status	N	Y	Y	Y	N	N	N
Hours Worked Last Week	N	N	Y	Y	N	Y	N
Employment Status	N	N	N	Y	Y	N	N
Normalization	Docket	Docket	Docket	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y	Y	Y	Y
IV	Y	Y	Y	Y	Y	Y	Y
N	4927744	3883690	3726194	3726194	4923395	4760743	3444157
R-sq	0.137	0.112	0.727	0.946	0.959	0.830	0.296

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (age, race dummies, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and normalization factor (Docket Size). Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Circuit-by-year controls (judicial pool characteristics and the normalization factor) are interacted with the female dummy as are the measure of sexual harassment law (pro-plaintiff) and instruments. Columns 2-4 successively control for other employment outcomes (managerial status, hours worked last week, employment status) and their interaction with the female dummy. Column 5 restricts to the sample of individuals reporting non-zero wages. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 10 -- Decomposing the Effect of Sexual Harassment Law vs. Gender Discrimation Law, 1995-2002

	Employment Status		Hours Worked Last Week	
	(1)	(2)	(3)	(4)
Number of Pro-Plaintiff Sexual Harassment	0.00252**	0.00192**	0.0350	0.0167
Appellate Decisions * Sex	(0.000502)	(0.000584)	(0.0342)	(0.0298)
Number of Pro-Plaintiff Gender Discrimination		0.00260		0.0895
Appellate Decisions * Sex		(0.00172)		(0.0640)
Normalization	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y
IV	Y	Y	Y	Y
N	1886023	1886023	1829509	1829509
R-sq	0.084	0.084	0.117	0.117
	Log Real Weekly Earnings		Managerial Status	
	(5)	(6)	(7)	(8)
Number of Pro-Plaintiff Sexual Harassment	0.00425	0.00149	0.00225**	0.00199*
Appellate Decisions * Sex	(0.00403)	(0.00408)	(0.000691)	(0.000663)
Number of Pro-Plaintiff Gender Discrimination		0.0130		0.00104
Appellate Decisions * Sex		(0.00996)		(0.000880)
Normalization	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y
IV	Y	Y	Y	Y
N	1728939	1728939	1541695	1541695
R-sq	0.121	0.121	0.060	0.060

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (age, race dummies, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female judge and the probability of a panel being assigned a Democratic appointee), and normalization factor (Docket Size). Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Circuit-by-year controls (judicial pool characteristics and the normalization factor) are interacted with the female dummy as are the measure of sexual harassment law (pro-plaintiff) and instruments. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Table 11 -- The Law vs. Economics of Sexual Harassment Law, 1982-2002

	Employment Status			Hours Worked		
	(1)	(2)	(3)	(4)	(5)	(6)
Damages Awarded in Sexual Harassment	0.00135**	0.000362	0.000199	0.0435**	0.0102	0.00567
Appellate Decisions * Sex	(0.000355)	(0.000232)	(0.000132)	(0.0121)	(0.00700)	(0.00476)
Number of Pro-Plaintiff Sexual Harassment		0.0127**	0.00744**		0.403**	0.243**
Appellate Decisions * Sex		(0.00343)	(0.00208)		(0.117)	(0.0603)
Supreme Court Pro-Plaintiff resolution of			0.0260			0.590
Harris * Sex			(0.0153)			(0.537)
Supreme Court Pro-Plaintiff resolution of			0.0113			0.103
Faragher * Sex			(0.00810)			(0.313)
Normalization	Docket	Docket	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y	Y	Y
IV	Y	Y	Y	Y	Y	Y
N	5398935	5398935	5398935	5208419	5208419	5208419
R-sq	0.098	0.098	0.099	0.137	0.137	0.137

	Earnings			Managerial Status		
	(7)	(8)	(9)	(10)	(11)	(12)
Damages Awarded in Sexual Harassment	0.00817**	0.00235	0.00147	0.000449**	0.000207*	0.000153*
Appellate Decisions * Sex	(0.00217)	(0.00148)	(0.000902)	(0.000125)	(0.0000818)	(0.0000662)
Number of Pro-Plaintiff Sexual Harassment		0.0729**	0.0412**		0.00293**	0.00146**
Appellate Decisions * Sex		(0.0226)	(0.0127)		(0.000807)	(0.000405)
Supreme Court Pro-Plaintiff resolution of			0.125			0.00900**
Harris * Sex			(0.103)			(0.00198)
Supreme Court Pro-Plaintiff resolution of			0.0394			0.00469*
Faragher * Sex			(0.0468)			(0.00213)
Normalization	Docket	Docket	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y	Y	Y
IV	Y	Y	Y	Y	Y	Y
N	4927744	4927744	4927744	4334364	4334364	4334364
R-sq	0.137	0.137	0.137	0.061	0.062	0.062

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (age, race dummies, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female judge and the probability of a panel being assigned a Democratic appointee), and normalization factor (Docket Size). Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Circuit-by-year controls (judicial pool characteristics and the normalization factor) are interacted with the female dummy as are the measure of sexual harassment law (pro-plaintiff) and instruments. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. 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 summed at the circuit-year level. Major Supreme Court decisions, 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. Instruments are the number of sexual harassment panels with at least one female judge, the number of sexual harassment panels with at least one Democratic appointee, number of sexual harassment damages cases with at least one female judge, and the number of sexual harassment damages cases with at least one Democratic appointee. + Significant at 10%; * Significant at 5%; ** Significant at 1%

	Employment Status				Hours Worked Last Week			
Sample:	Small Firms	Large Firms	Blue Collar	White Collar	Small Firms	Large Firms	Blue Collar	White Collar
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Pro-Plaintiff Sexual Harassment	-0.000934	-0.0139*	-0.000855	-0.000981	-0.0762	-0.363+	0.0355	-0.0629
Appellate Decisions	(0.00293)	(0.00485)	(0.000715)	(0.00170)	(0.241)	(0.191)	(0.0736)	(0.106)
Number of Pro-Plaintiff Sexual Harassment	0.00130	0.0277*	0.00542**	0.00233	0.266	0.909*	0.265*	0.187+
Appellate Decisions * Sex	(0.00234)	(0.00983)	(0.00135)	(0.00221)	(0.191)	(0.392)	(0.0800)	(0.0955)
Damages Awarded in Sexual Harassment	-0.0000339	0.000289	-0.0000107	-0.0000543	0.0192	0.00886	0.00575	0.00885
Appellate Decisions	(0.000259)	(0.000289)	(0.0000748)	(0.000135)	(0.0177)	(0.0135)	(0.00626)	(0.00874)
Damages Awarded in Sexual Harassment	0.000576*	-0.000482	-0.000130	-0.0000273	-0.00767	-0.0147	-0.0146+	-0.00376
Appellate Decisions * Sex	(0.000189)	(0.000521)	(0.000122)	(0.000157)	(0.0143)	(0.0238)	(0.00755)	(0.00807)
Sex	-0.0263	-0.0389	-0.0456*	-0.0142	-3.249	-2.700	-5.507**	-3.889+
	(0.0269)	(0.139)	(0.0170)	(0.0156)	(1.862)	(5.520)	(0.987)	(1.840)
Normalization	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y	Y	Y	Y	Y
IV	Y	Y	Y	Y	Y	Y	Y	Y
N	126554	508588	376346	192584	126554	508588	376346	192584
R-sq	0.038	0.133	0.035	0.020	0.088	0.135	0.084	0.070

	Log Real Weekly Earnings				Managerial Status			
Sample:	Small Firms	Large Firms	Blue Collar	White Collar	Small Firms	Large Firms	Blue Collar	White Collar
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Number of Pro-Plaintiff Sexual Harassment	0.0214	-0.0638*	-0.00439	0.00553	-0.00293	-0.00175	-0.000838	-0.00553**
Appellate Decisions	(0.0212)	(0.0217)	(0.00708)	(0.0150)	(0.00262)	(0.00105)	(0.00102)	(0.00146)
Number of Pro-Plaintiff Sexual Harassment	-0.0310	0.133*	0.0377**	-0.000583	0.00166	0.00430**	0.00428**	0.00403
Appellate Decisions * Sex	(0.0233)	(0.0446)	(0.00803)	(0.0153)	(0.00243)	(0.000981)	(0.000787)	(0.00266)
Damages Awarded in Sexual Harassment	-0.00231	0.00129	-0.00114	0.000378	-0.0000534	0.0000536	-0.0000378	0.000268
Appellate Decisions	(0.00204)	(0.00132)	(0.000984)	(0.00136)	(0.000219)	(0.000158)	(0.000129)	(0.000227)
Damages Awarded in Sexual Harassment	0.00587*	-0.00186	-0.000255	0.000609	0.0000887	0.000151	-0.0000117	0.000186
Appellate Decisions * Sex	(0.00212)	(0.00244)	(0.000907)	(0.00149)	(0.000247)	(0.000123)	(0.000112)	(0.000326)
Sex	0.182	-0.471	-0.453*	-0.0966	0.0323	-0.00262	-0.0134	-0.00154
	(0.238)	(0.626)	(0.138)	(0.155)	(0.0223)	(0.0183)	(0.0153)	(0.0347)
Normalization	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y	Y	Y	Y	Y
IV	Y	Y	Y	Y	Y	Y	Y	Y
N	126544	503265	369577	189951	117649	438118	376346	192584
R-sq	0.028	0.212	0.058	0.055	0.048	0.067	0.079	0.040

Notes: All regressions use March CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (age, race dummies, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female judge and the probability of a panel being assigned a Democratic appointee), and normalization factor (Docket Size). Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Circuit-by-year controls (judicial pool characteristics and the normalization factor) are interacted with the female dummy as are the measure of sexual harassment law (pro-plaintiff) and instruments. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. 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 summed at the circuit-year level. Instruments are the number of sexual harassment panels with at least one female judge, the number of sexual harassment panels with at least one Democratic appointee, number of sexual harassment damages cases with at least one female judge, and the number of sexual harassment damages cases with at least one Democratic appointee. Small Firms are those with under 25 employees and Large Firms are those with 25 or more employees. Blue Collar industries are mining, construction, transportation, communications, utilities, manufacturing, agriculture, forestry, fishing, and business, personal, and entertaine service and White Collar industries are public administration, finance, insurance, real estate, wholesale, and retail. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Appendix Table A -- Summary Statistics

Circuit-Year Level	Judicial Pool Characteristics		Sexual Harassment (1982-2002)	Damages (1982-2002)	Gender Discrimination (1995-2002)
Number of Judges	18.50	Number of Panels	0.996	0.357	5.510
	(0.463)		(0.0927)	(0.0470)	(0.392)
Probability of Panel Being Assigned	0.258	Number of Panels Having	0.325	0.131	1.927
At Least One Female Judge	(0.0104)	At Least One Female Judge	(0.0388)	(0.0260)	(0.197)
Probability of Panel Being Assigned	0.784	Number of Panels Having	0.786	0.298	4.135
At Least One Democratic Appointee	(0.00654)	At Least One Democratic Appointee	(0.0721)	(0.0422)	(0.304)
Probability of Panel Being Assigned	0.159	Number of Panels Having	0.115		
At Least One Female Republican Appointee	(0.00684)	At Least One Female Republican Appointee	(0.0237)		
Probability of Panel Being Assigned	0.321	Number of Panels Having	0.206		
At Least Two Male Democrat Appointees	(0.00815)	At Least Two Male Democrat Appointees	(0.0316)		
Docket Size	3611.5	Number of Pro-Plaintiff	0.667	0.290	2.062
	(123.3)	Appellate Decisions	(0.0724)	(0.0428)	(0.165)
		Damages Awarded in 10,000s		4.782	
				(1.076)	
N (circuit-years)	252		252	252	96
Individual Outcomes					
Employment Status - Female	0.645	Log Real Weekly Earnings - Female	3.667		
	(0.000285)		(0.00178)		
Employment Status - Male	0.811	Log Real Weekly Earnings - Male	4.938		
	(0.000243)		(0.00176)		
Hours Worked - Female	22.83	Management Status - Female	0.113		
	(0.0122)		(0.000219)		
Hours Worked - Male	34.28	Management Status - Male	0.135		
	(0.0130)		(0.000228)		
N	5,998,268		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. All CPS estimates are weighted using CPS survey weights. A pro-plaintiff 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 and summed at the circuit-year level. Coefficients displayed are from an OLS regression on a constant.

Appendix Table B -- Randomization Check

Case Characteristics	Panel with Female (1)	Panel with Democrat (2)
Direction of Lower Court Decision	-0.027	-0.054
	(0.048)	(0.051)
P claims employer acted in retaliation	0.051	-0.084
	(0.053)	(0.057)
All plaintiffs are female	-0.011	-0.069
	(0.042)	(0.045)
Title IX claim	0.001	0.019
	(0.014)	(0.015)
Section 1983 claim	-0.033	0.017
	(0.028)	(0.030)
Constructive discharge from employment	0.032	-0.026
	(0.032)	(0.034)
Procedural issues dominate	-0.016	0.013
	(0.033)	(0.035)
P suing under state law	-0.035	0.040
	(0.045)	(0.049)
P claims illegally denied promotion	-0.016	-0.018
	(0.043)	(0.046)
P claims illegally not being hired	0.006	-0.003
	(0.030)	(0.032)
P claims illegally fired	0.025	0.025
	(0.054)	(0.059)
P claims unequal pay	-0.017	-0.072+
	(0.038)	(0.041)
P sued under 14th Amendment	-0.069**	0.014
	(0.024)	(0.026)
P sued under 1st Amendment	-0.037+	0.007
	(0.020)	(0.021)
Damages major point of contention	0.063+	-0.001
	(0.036)	(0.040)
Contains Section 1981 claim	0.043	-0.001
	(0.032)	(0.034)
Contains age discrimination claim	-0.062	-0.026
	(0.039)	(0.042)
Contains pregnancy discrimination claim	0.023	-0.007
	(0.027)	(0.029)
Contains emotional distress claim	0.031	-0.009
	(0.028)	(0.031)
P not victim of harassment	0.045	0.070
	(0.121)	(0.117)
P is appellant	0.032	0.101
	(0.168)	(0.161)

Notes: Heteroskedasticity-robust standard errors are in parentheses. Each row in column 1 represents a separate 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 represents a separate 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, circuit-specific time trends, 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 -- Tests for Serial Correlation

	Number of Pro-Plaintiff Sexual Harassment Decisions		Sexual Harassment Panels with At Least One Female Republican		Sexual Harassment Panels with At Least Two Male Democrat	
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Pro-Plaintiff Decisions$_{t-1}$	0.0434	-0.0872				
	(0.130)	(0.0972)				
Sexual Harassment Panels with At Least One Female Republican Appointee$_{t-1}$			-0.225+	-0.114		
			(0.117)	(0.0771)		
Sexual Harassment Panels with At Least Two Male Democratic Appointees$_{t-1}$					-0.0729	-0.103
					(0.0910)	(0.0852)
Normalization	Docket	Panels	Docket	Panels	Docket	Panels
N	240	240	240	240	240	240
R-sq	0.518	0.872	0.301	0.413	0.342	0.467

Notes: Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. These regressions control for circuit fixed-effects, year fixed-effects, circuit-specific time trends, and time-varying circuit judicial pool characterics (the probability of a panel being assigned a female Republican appointee, the probability of a panel being assigned at least two male Democratic appointees) and normalization factor (Docket Size in Columns 1, 3, and 5 or the Number of Sexual Harassment Panels in Columns 2, 4, and 6); lagged values of the time-varying circuit characteristics are also included. 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 D -- The Effect of Sexual Harassment Law on Human Resources Policies

Human Resources Policy	OLS (1)	OLS with controls (2)	IV (3)	IV with controls (4)
		Number of Pro-Plaintiff Appellate Decisions		
Treats Advances from Co-workers as Part of Sexual Harassment	0.00824 (0.00683)	0.00964 (0.00641)	-0.00105 (0.0120)	-0.00574 (0.0130)
Presence of General Anti-Harassment Policy	0.00562 (0.00704)	0.0118* (0.00500)	-0.000263 (0.0165)	0.00975 (0.00906)
Sexual Comments/Jokes Treated as Part of Sexual Harassment	0.0126+ (0.00597)	0.00721 (0.00654)	-0.00462 (0.0105)	-0.0107 (0.0152)
Presence of Sexual Harassment Policy	0.0114 (0.00646)	0.00861 (0.00639)	0.00640 (0.0120)	0.00270 (0.0128)
Presence of Full-Time Staff Dealing With Sexual Harassment	0.00460+ (0.00232)	0.00174 (0.00179)	0.00392 (0.00493)	0.00220 (0.00396)
Presence of Training Program About Sexual Harassment	0.0148** (0.00424)	0.0131* (0.00450)	0.0163* (0.00565)	0.0119 (0.00911)
Presence of Affirmative Action Officer	0.00282 (0.00375)	0.00302 (0.00388)	0.00136 (0.00768)	-0.00211 (0.00657)
Presence of Affirmative Action Policy	-0.00126 (0.00834)	-0.00276 (0.00519)	-0.00478 (0.00957)	0.000967 (0.00611)
Presence of Benefit Department	0.00425 (0.00274)	0.00368 (0.00264)	0.00287 (0.00440)	0.00422 (0.00545)
Presence of Compliance Review	0.000240 (0.00432)	-0.000224 (0.00335)	-0.00169 (0.00796)	-0.00930 (0.00670)
Presence of Diversity Policy	-0.00726 (0.00463)	0.000972 (0.00210)	-0.0227+ (0.0111)	-0.00612 (0.00548)
Presence of Diversity Staff	-0.000832 (0.00286)	0.00305 (0.00297)	-0.00879 (0.00796)	-0.00432 (0.00491)
Presence of Diversity Training	0.000118 (0.00503)	0.00398 (0.00366)	-0.00202 (0.0104)	-0.00336 (0.0105)
Presence of EEO/AA Department	-0.00384 (0.00260)	0.000499 (0.00158)	-0.00936 (0.00533)	-0.00196 (0.00414)
Presence of Human Resources Department	0.0000615 (0.00459)	0.00226 (0.00448)	-0.0111 (0.00823)	-0.00651 (0.00927)
Presence of Mentoring Program	0.00280 (0.00388)	-0.000838 (0.00236)	-0.00476 (0.00607)	-0.00656 (0.00508)
Presence of Human Resources Professional Association	-0.0000605 (0.00323)	0.000397 (0.00220)	0.00216 (0.00940)	0.00682 (0.0107)
Presence of Union Contract	-0.000821 (0.00192)	-0.00425 (0.00285)	-0.00802 (0.00590)	-0.00590 (0.00478)
Presence of Government Contract	-0.00404 (0.00291)	-0.00568* (0.00218)	-0.0112* (0.00479)	-0.00709+ (0.00333)
Presence of Core Job Training	0.00539 (0.00510)	0.00500 (0.00460)	0.000216 (0.0106)	0.00602 (0.00987)
Presence of Human Resources Attorney	-0.0000241 (0.00633)	-0.00343 (0.00309)	-0.0136 (0.00831)	-0.00626 (0.00361)
Presence of Core Job Union	-0.000620 (0.00226)	-0.00250 (0.00235)	-0.000945 (0.00551)	-0.00141 (0.00451)
Presence of Human Resources Consultant	0.00419 (0.00738)	-0.00104 (0.00435)	0.00322 (0.00884)	0.00495 (0.00831)
Presence of Legal Department	0.00579 (0.00588)	0.00403 (0.00423)	-0.000468 (0.0107)	0.00401 (0.00666)
Presence of Labor Relations Department	-0.00297+ (0.00138)	-0.000906 (0.00173)	-0.00286 (0.00467)	0.00305 (0.00364)

Notes: Heteroskedasticity-robust standard errors are in parentheses. Each cell represents a separate regression of a distinct human resources policy on a the number of pro-plaintiff appellate decisions in a specification analogous to that of Table 2. Observations are clustered at the circuit level. These regressions include circuit fixed-effects, year fixed-effects, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and normalization factor (Docket Size). Additional controls included in even numbered columns are firm-related controls (number of employees, percent of women at location, sex of top executive named, percent women among named executives, age of establishment, dummies for manufacturing, service, and trucking, regional unemployment rate, and percent female in industry) and circuit-specific time trends. 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 E -- The Effect of Sexual Harassment Law on Human Resources Sexual Harassment Policy

	Presence of Sexual Harassment Policy		
	(1)	(2)	(3)
Number of Pro-Plaintiff Appellate Decisions$_{t+1}$	0.0157+ (0.00733)	0.0239 (0.0163)	
Number of Pro-Plaintiff Appellate Decisions$_t$	0.0115 (0.00659)	0.0289 (0.0207)	
Number of Pro-Plaintiff Appellate Decisions$_{t-1}$	-0.000192 (0.00712)	0.0159 (0.0205)	
Number of Pro-Plaintiff Appellate Decisions$_{t-2}$	0.0126 (0.00985)	0.0518+ (0.0238)	
Number of Pro-Plaintiff Appellate Decisions$_{t-3}$	0.0361** (0.00970)	0.0592* (0.0230)	
Number of Pro-Plaintiff Appellate Decisions$_{t-4}$	0.0452** (0.00881)	0.0210 (0.0244)	
Number of Pro-Plaintiff Appellate Decisions$_{t-5}$	0.0000770 (0.0115)	-0.0142 (0.0525)	
Number of Pro-Defendant Appellate Decisions$_{t+1}$			0.000514 (0.0114)
Number of Pro-Defendant Appellate Decisions$_t$			-0.00330 (0.0115)
Number of Pro-Defendant Appellate Decisions$_{t-1}$			-0.0112 (0.0175)
Number of Pro-Defendant Appellate Decisions$_{t-2}$			-0.0345 (0.0215)
Number of Pro-Defendant Appellate Decisions$_{t-3}$			-0.0676** (0.0136)
Number of Pro-Defendant Appellate Decisions$_{t-4}$			-0.0272* (0.0109)
Number of Pro-Defendant Appellate Decisions$_{t-5}$			0.00430 (0.0171)
Normalization	Docket	Docket	Docket
Controls	Y	Y	Y
IV	N	Y	N
N	4014	4014	4014
R-sq	0.262	0.261	0.262

Notes: Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. These regressions include firm-related controls (number of employees, percent of women at location, sex of top executive named, percent women among named executives, age of establishment, dummies for manufacturing, service, and trucking, regional unemployment rate, and percent female in industry), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and the normalization factors (a dummy for whether there were no cases in that circuit-year and Docket Size). Docket Size is the number of appellate

Appendix Table F -- First Stage: Relationship Between Pro-Plaintiff Gender Discrimination Appellate Decisions and Composition of Gender Discrimination Panels

Panel A: Judge Level	Pro-Plaintiff Decision		
	(1)	(2)	(3)
Female	0.0666+		0.0329
	(0.0338)		(0.0306)
Democratic Appointee		0.0955**	0.0882**
		(0.0266)	(0.0255)
N	1363	1363	1363
R-sq	0.076	0.079	0.082

Notes: Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. These regressions control for circuit fixed-effects, year fixed-effects, and the probability of a panel being assigned a judge of the type under consideration: female (Columns 1 and 3), and Democratic Appointee (Columns 2 and 3).

Panel B: Case Level	Pro-Plaintiff Decision		
	(1)	(2)	(3)
At Least One Female Judge	0.111+		0.0842
	(0.0529)		(0.0545)
At Least One Democratic Appointee		0.104**	0.0776*
		(0.0308)	(0.0328)
N	562	562	562
R-sq	0.071	0.069	0.076

Notes: Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. These regressions control for circuit fixed-effects, year fixed-effects, and the probability of a panel being assigned at least one female judge (Columns 1 and 3) and the probability of a panel being assigned at least one Democratic appointee (Columns 2 and 3).

Panel C: Circuit-Year Level	Number of Pro-Plaintiff Gender Discrimination Decisions (1995-2002)						Damages Awarded (1982-2002)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Gender Discrimination Panels with At Least One Female Judge	0.435**		0.0213						
	(0.110)		(0.140)						
Gender Discrimination Panels with At Least One Democratic Appointee		0.462**	0.451**						
		(0.0873)	(0.0853)						
Sexual Harassment Panels with At Least One Female Judge				0.555		0.588			
				(0.318)		(0.519)			
Sexual Harassment Panels with At Least One Democratic Appointee					0.182	-0.0330			
					(0.111)	(0.247)			
Sexual Harassment Damage Cases with At Least One Female Judge							7.870+		-1.970
							(4.203)		(3.899)
Sexual Harassment Damage Cases with At Least One Democratic Appointee								11.18**	12.01**
								(3.248)	(3.315)
Normalization	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket	Docket
N	96	96	96	96	96	96	252	252	252
R-sq	0.528	0.648	0.648	0.456	0.423	0.456	0.344	0.416	0.417

Notes: Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. These regressions control for circuit fixed-effects, year fixed-effects, circuit-specific time trends, probability of a panel being assigned a female judge, the probability of a panel being assigned a Democratic appointee, and Docket Size. Docket Size is the number of appellate terminations in the circuit year minus the number of gender discrimination decisions in Columns 1-6 and the number of appellate terminations in a circuit year minus the number of sexual harassment damages decisions in Columns 7-9. Gender discrimination cases do not include sexual harassment cases. 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, are adjusted for inflation, and are summed at the circuit-year level. Columns 1-6 include a dummy for whether there were no gender discrimination cases in that circuit-year. Columns 7-9 do not include a dummy for whether there were no sexual harassment damage cases since there are relatively few damage cases. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Appendix Table G -- The Effects of Sexual Harassment Law Lags and Leads, 1982-2002

	Employment Status (1)	Hours Worked Last Week (2)	Log Real Weekly Earnings (3)	Management (4)
Number of Pro-Plaintiff Cases$_{t+1}$ * Sex	0.000420	-0.00361	-0.00400	0.00135*
	(0.00132)	(0.0471)	(0.00980)	(0.000510)
Number of Pro-Plaintiff Cases * Sex	0.00323**	0.0937*	0.0200**	0.000422
	(0.000934)	(0.0347)	(0.00636)	(0.000386)
Number of Pro-Plaintiff Cases$_{t-1}$ * Sex	-0.00159	-0.0638	-0.0110	0.000836
	(0.00156)	(0.0494)	(0.00890)	(0.000610)
Number of Pro-Plaintiff Cases$_{t-2}$ * Sex	0.00360	0.0884	0.0236	-0.000745
	(0.00217)	(0.0847)	(0.0149)	(0.000841)
Number of Pro-Plaintiff Cases$_{t-3}$ * Sex	0.00842**	0.226+	0.0497*	0.00287*
	(0.00214)	(0.112)	(0.0180)	(0.00129)
Number of Pro-Plaintiff Cases$_{t-4}$ * Sex	0.00177	-0.0818	0.00159	0.00237
	(0.00420)	(0.130)	(0.0288)	(0.00182)
Harris$_{t+1}$ * Sex	0.0235+	1.052*	0.216*	0.00382
	(0.0116)	(0.378)	(0.0723)	(0.00325)
Harris * Sex	-0.000751	-0.336	-0.0138	0.00394
	(0.00863)	(0.206)	(0.0604)	(0.00366)
Harris$_{t-1}$ * Sex	0.00163	-0.324	-0.0469	0.00283
	(0.00943)	(0.266)	(0.0539)	(0.00281)
Harris$_{t-2}$ * Sex	0.0111	0.501	0.0782	-0.0100**
	(0.0144)	(0.652)	(0.0969)	(0.00301)
Harris$_{t-3}$ * Sex	-0.00223	0.0494	-0.0197	0.00867+
	(0.0121)	(0.527)	(0.0870)	(0.00430)
Harris$_{t-4}$ * Sex	-0.0126*	-0.872+	-0.149**	0.00298
	(0.00515)	(0.413)	(0.0479)	(0.00507)
Faragher$_{t+1}$ * Sex	-0.0113	-0.190	-0.00296	-0.00318
	(0.0109)	(0.480)	(0.0774)	(0.00425)
Faragher * Sex	0.0139+	0.245	0.0299	-0.00371
	(0.00770)	(0.460)	(0.0618)	(0.00569)
Faragher$_{t-1}$ * Sex	0.00648	-0.0567	-0.0102	0.00823*
	(0.0107)	(0.317)	(0.0654)	(0.00333)
Faragher$_{t-2}$ * Sex	-0.000687	-0.0332	-0.00627	0.000865
	(0.00427)	(0.257)	(0.0370)	(0.00284)
Faragher$_{t-3}$ * Sex	-0.00347	0.236	-0.0392	0.00474**
	(0.00520)	(0.205)	(0.0357)	(0.00142)
Faragher$_{t-4}$ * Sex	-0.00633	-0.746	-0.00885	-0.00631
	(0.0122)	(0.447)	(0.0699)	(0.00375)
Normalization	Docket	Docket	Docket	Docket
Controls	Y	Y	Y	Y
IV	Y	Y	Y	Y
N	4009459	3870253	3659743	3396029
R-sq	0.096	0.133	0.135	0.061

Notes: All regressions use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. All estimates are weighted using CPS survey weights. These regressions include individual controls (age, race dummies, educational attainment dummies, and marital status dummy), circuit fixed-effects, year fixed-effects, circuit-specific time trends, judicial pool characteristics (the probability of a panel being assigned a female Republican appointee and the probability of a panel being assigned at least two male Democratic appointees), and normalization factor (Docket Size). Docket Size is the number of appellate terminations in the circuit year minus the number of sexual harassment decisions. Circuit-by-year controls (judicial pool characteristics and the normalization factor) are interacted with the female dummy as are the measure of sexual harassment law (pro-plaintiff) and instruments. While the coefficients and standard errors shown are only for the two-way interactions, all regressions contain the individual elements of each interaction term. Major Supreme Court decisions, 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. Instruments are the number of sexual harassment panels with at least one female judge, the number of sexual harassment panels with at least one Democratic appointee, number of sexual harassment damages cases with at least one female judge, and the number of sexual harassment damages cases with at least one Democratic appointee. + Significant at 10%; * Significant at 5%; ** Significant at 1%