SEXUAL HARASSMENT AND GENDER INEQUALITY IN THE LABOR MARKET*

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We describe how sexual harassment contributes to sex segregation and pay inequality in the labor market. Combining nationally representative survey data and administrative data, we show that both harassment and wages vary strongly and systematically across workplaces. Women self-report more harassment from colleagues and managers in male-dominated workplaces where wages are relatively high, and men self-report more harassment in female-dominated workplaces where wages are low. These patterns imply two ways that harassment may contribute to gender inequality. First, harassment deters women and men from applying for jobs in workplaces where they are the gender minority. A survey experiment with hypothetical job choices supports this mechanism. Respondents are highly averse to accepting jobs in workplaces with a higher harassment risk for their own gender, but less averse when people of the opposite sex are at higher risk. A second way that harassment contributes to inequality is by making workplace gender minorities leave their workplaces for new jobs. An analysis of workplace transitions supports this mechanism. Women who self-report harassment are more likely to switch to new workplaces with more female colleagues and lower pay. *JEL Codes:* J16, J24, J81.

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I. Introduction

Workplaces play an important role for gender inequality in the labor market. Women and men are strongly segregated across workplaces, and this segregation explains 15%–20% of the gender wage gap (Blau 1977; Groshen 1991; Petersen and Morgan 1995; Card, Cardoso, and Klein 2016). We study how gender discrimination in work conditions contributes to this inequality. Sexual harassment is defined as discrimination in civil law and constitutes a highly prevalent workplace problem with severe consequences for the victim. About half of all women become victims at some point in their work lives, and research documents wide-ranging negative consequences for physical and mental health (reviewed by Welsh 1999; McDonald 2012; Fitzgerald and Cortina 2018).[1] The #MeToo hashtag brought this urgent problem to the forefront of the modern-day women's movement.

We argue that sexual harassment contributes to gender inequality in the labor market by imposing costs on men and women whose workplace choices help reduce sex segregation and close the gender wage gap. We underpin this argument with three sets of empirical analysis of Swedish data. Using nationally representative survey data, we document higher harassment risks against workplace gender minorities. We then use a survey experiment to quantify large negative valuations of these risks, especially for workers of the same gender as the harassment victim. The third part of the analysis links the costly harassment risks to workplace wage levels and workplace transitions. Women face higher risks in workplaces with high pay and men in workplaces with low pay, and harassment is associated with transitions that expand segregation and the gender wage gap. Taken together, these results illustrate how sexual harassment reduces women's and men's incentives to become and remain workplace gender minorities. A back-of-the-envelope calculation attributes a sizable share of the gender wage gap to this discriminatory work condition.

1. The mental and physical health consequences range from irritation and anxiety to anger, powerlessness, humiliation, increased risk of alcohol abuse and eating disorders, depression, and posttraumatic stress disorder. Job-related consequences include absenteeism, lower job satisfaction, lower commitment and productivity, damaged interpersonal work relationships, decreased perceptions of equal opportunity, and employment withdrawal.

Our main data source is the Swedish government's nationally representative survey on work conditions ($N = 40{,}000$). We link this survey to annual employer–employee data for the entire employed population. This lets us calculate the exact sex ratio in each respondent's workplace. It also provides data on the wages in each workplace and lets us track survey respondents' workplace transitions over time.

We use the European Commission definition of sexual harassment, which is "unwanted conduct of a sexual nature, or other conduct based on sex affecting the dignity of men and women at work." Two survey questions ask about these experiences over the past 12 months by managers or colleagues. One asks about unwanted conduct of a sexual nature, and the other about conduct based on sex that affects the respondent's dignity. Both describe behaviors that correspond to legal definitions of discrimination and are prohibited by law.[2]

We use the nationally representative survey data to describe how the sexual harassment risks for men and women vary across workplaces with different sex ratios. This analysis advances prior research on smaller data sets with limited coverage by sector or geography and, to our knowledge, a complete lack of nationally representative data for men. Another improvement is our objective and continuous measurement of sex ratios, which improves on the self-reported categorical data used in previous work. Like most earlier research, we find that women's harassment rate grows with the share of men in the workplace (e.g., Gutek and Cohen 1987; Gutek, Cohen, and Konrad 1990; Gruber 1998; Jackson and Newman 2004; Kabat-Farr and Cortina 2014). For men, we find a growing harassment rate with the share of women, a finding that contributes to a smaller body of empirical literature with mixed results (Jackson and Newman 2004; Kabat-Farr and Cortina 2014).

2. Our broad definition of sexual harassment includes unwanted sexual attention, sexual hostility, sexist hostility, and sexual coercion (Section III discusses these concepts in more depth). In Sweden, these behaviors are defined as discrimination by the 1991 Equal Opportunities Act and the 2008 Discrimination Act. In the EU, sexual harassment and/or gender harassment are prohibited by the 2002 antidiscrimination directive, which also defines these behaviors as sex discrimination (Directive 2002/73/EC; see Zippel 2019 for an extended discussion). In the United States, Title VII of the 1964 Civil Rights Act legally mandates employers to prevent any form of workplace discrimination based on gender. The behaviors captured in our survey data fall under the definition of workplace discrimination in this legal framework, since they have the effect of creating an intimidating, hostile, or offensive working environment (Leskinen, Cortina, and Kabat 2011).

To quantify people's valuations of these harassment risks, we ran a survey experiment on employed Swedes ($N = 3{,}987$). This analysis builds directly on a growing literature that quantifies valuations of other work conditions to understand labor market inequalities (Wiswall and Zafar 2018; Mas and Pallais 2017; Maestas et al. 2018). We adopt the same type of hypothetical job choice experiment where respondents choose between fictional job offers with randomized wages and work conditions (Wiswall and Zafar 2018; Mas and Pallais 2017; Eriksson and Kristensen 2014; Maestas et al. 2018; He, Neumark, and Weng 2021).[3]

We incorporate sexual harassment into the experiment by showing respondents vignettes of sexual harassment incidents that took place in the fictional workplaces (as in Hulin, Fitzgerald, and Drasgow 1996). These vignettes mimic the types of anecdotes or rumors that a prospective employee might hear about a potential employer. We verify that the vignettes shift respondents' perceptions about the pervasiveness of sexual harassment in the workplace and by the same margin across subsamples of interest. Importantly, the vignettes let us vary the victim's gender, which allows us to compare job choices among respondents exposed to a harassment victim of their own gender and respondents exposed to a victim of the opposite gender. To make the vignettes more realistic, we match gender of the victim to the typical harassment incident in the survey respondent's own labor market context.

We find a large negative valuation of sexual harassment—the equivalent of a 10% lower wage in the full sample. This large valuation makes sexual harassment a relevant work condition for shaping people's total remuneration from work and is quantitatively similar to the valuations of time/space flexibility in previous research (Wiswall and Zafar 2018; Mas and Pallais 2017; Maestas et al. 2018). We find similar valuations for men and women, but a substantial difference between those who see a victim of their own sex compared to the opposite sex: the negative valuation is equivalent to a 17% lower wage for same-sex victims but just 6% for opposite-sex ones.

Our analysis of wages and workplace transitions is based on the nationally representative survey data linked with employer–employee data. We show that the workplace-specific wage

3. Preregistered at egap.org. ID 20190927AA, https://egap.org/registration/6139.

premium grows with the share of men, implying that women's risk of harassment increases—and men's risk decreases—as workplace wages rise. An extended empirical analysis of workplace transitions adds insights on how harassment further expands sex segregation and wage inequality via this channel. Following survey respondents over time in the administrative data shows a larger probability to leave the workplace among people who self-report sexual harassment. By comparing the departure and destination workplaces in these transitions, we provide novel evidence that transitions after sexual harassment increase sex segregation and gender wage inequality.[4]

We provide rough calculations for how sexual harassment contributes to aggregate gender inequality. Following previous research, these calculations combine wage statistics with statistics on the distributions and valuations of work conditions (e.g., Sorkin 2017; Maestas et al. 2018). The calculations show that harassment imposes a 30% utility "tax" on women for switching employment from the average woman's to the average man's workplace. They also attribute about one-tenth of the raw gender wage gap to sexual harassment.

Our article advances the understanding of the factors that underpin sex segregation and pay inequality in the labor market by documenting the importance of sexual harassment. We argue that in addition to discrimination in recruitment and wages, discrimination in work conditions merits more attention from researchers. More specifically, we broaden the discussion of work conditions by describing how this type of discrimination operates. A recent strand of research describes how gender differences in preferences for family-friendly work conditions propel women into lower-paying jobs (Goldin 2014; Flory, Leibbrandt, and List 2014; Wiswall and Zafar 2018; Morchio and Moser 2020; Le Barbanchon, Rathelot, and Roulet 2021). We show that the discriminatory work condition of sexual harassment operates in a different way. Harassment shapes gender inequality by imposing costs on men and women whose personal workplace choices help break sex segregation and close the gender pay gap. Sex segregation

4. These findings contribute to a literature that has mainly measured turnover as intentions to quit (see Laband and Lentz 1998; Antecol and Cobb-Clark 2004, 2006; Willness, Steel, and Lee 2007). An exception is McLaughlin, Uggen, and Blackstone's (2017) pioneering study of a small sample of young women in Minnesota with similar results to ours.

increases as a product of both men's and women's incentives to avoid costly mistreatment on the job from the opposite sex.

No study of sexual harassment is complete without a detailed discussion of measurement issues and data quality. Our survey data on self-reported harassment has several advantages, including respondent anonymity, data collection by the government rather than workplaces, and a low salience of the sexual harassment questions among a hundred others. These factors encourage respondents to report truthfully and limit social desirability bias. However, a disadvantage is that one of our survey questions explicitly asks respondents about experiences that constitute sexual harassment. This introduces an element of self-labeling known to skew self-reports (Stockdale, Vaux, and Cashin 1995; Ilies et al. 2003). We provide considerable evidence that our results capture variation in actual harassment rather than variation in self-labeling. For example, our results are robust to holding constant demographic traits which previous studies have found to correlate with labeling probabilities. Using additional variables for self-reports of bullying or conflicts with managers or colleagues, we can also reject a general pattern of high self-reports of interpersonal mistreatment among workplace gender minorities.

The empirical case of Sweden provides access to unique data without sacrificing external validity. Workplace sex segregation in Sweden is comparable to other countries. The average Swedish woman has 34% male colleagues and the average man has 67%, which is similar to the proportions in Portugal (24% and 70%, Card, Cardoso, and Klein, 2016) and the United States (40% and 60%, Sorkin 2017). Sexual harassment rates are also comparable, although few data sets offer consistent survey questions across countries. In the European and American Working Conditions Surveys, Sweden has among the highest harassment rates in Europe, but these are still lower than the United States (Bertrand 2020). A recent EU-wide survey on violence against women puts Swedish women's 12-month incidence of workplace sexual harassment at 10%. This is higher than the EU average (6%) but similar to countries like France (10%), Denmark and Malta (11%), and Luxembourg (12%) (authors' calculations). Survey data for men are scarcer, but our 12-month incidence of 4.2% places men's rate at one-third of women's, which replicates the gender gap in other data (e.g., Gutek and Cohen 1987; U.S. Merit Systems Protection Board 1994; Timmerman and Bajema 1999).

II. Workplace Sexual Harassment, Sex Segregation, and Gender Wage Inequality

This section starts by describing expected patterns of sexual harassment across workplace sex ratios. We also explain why preferences against this disamenity are likely to differ between people with high and low harassment risks in the same workplace. Based on these discussions, we add sexual harassment to a workplace utility function and use it to understand how harassment affects sex segregation and wage inequality in the labor market.

II.A. Sexual Harassment Risks across Workplaces

An important feature of workplace sexual harassment is that the risk of harassment is likely to differ between men and women in the same workplace. Women's risk is generally higher, because sexual harassment is fundamentally an expression of gender norms and power relations between men and women (Fitzgerald and Cortina 2018). A norm with particular relevance is that "in sexual behavior, men are expected to initiate and women are expected to respond," which is understood to make men more likely to perpetrate sexual harassment against women than vice versa (Gutek, Cohen, and Konrad 1990, 564).

Although women's baseline harassment risk is higher, the contact hypothesis predicts more intensive harassment against men and women who are gender minorities in their workplace (Gutek, Cohen, and Konrad 1990). This hypothesis builds on two research findings. The first is that most perpetrators of sexual harassment are of the opposite sex of their victim (reviewed by Cortina and Berdahl 2008). The second is that holding perpetrator gender constant, the likelihood of engaging in harassment is relatively constant across socioeconomic traits like education level, age, and social class (e.g., Pryor 1987; Pryor, LaVite, and Stoller 1993; Pina, Gannon, and Saunders 2009). If each opposite-sex person has a constant probability of perpetrating harassment, the harassment risk grows with the proportion of opposite-sex people in the workplace.

Workplace gender minorities are more likely to have supervisors and managers of the opposite sex, which could increase their risk of harassment. Supervisors initiate a sizable share of all workplace sexual harassment (Antecol and Cobb-Clark 2004; Folke et al. 2020). One reason for this is that positions of power and authority lower the threshold for perpetration

because superiors' control over subordinates' career outcomes makes subordinates less likely to report the transgressions of a supervisor (Bargh et al. 1995). This understanding builds on MacKinnon's (1979) groundbreaking research, which established men's power over women in the workplace as a fundamental explanation of the phenomenon of workplace sexual harassment.

A third risk factor for workplace gender minorities is a greater likelihood of working in a counterstereotypical occupation. A person who breaks gender norms in their occupation choice may trigger sexual harassment from coworkers who seek to reestablish norms and protect their own gender identity (West and Zimmerman 1987; Fitzgerald et al. 1997; Akerlof and Kranton 2000; Berdahl 2007). Past empirical studies generally find higher harassment rates for women who are minorities in their occupation (e.g., Gruber 1998). There is less research for men, but some of this work suggests the opposite pattern.[5]

Following the research on contact, gender-power hierarchies, and occupation gender norms, we anticipate the following relationships between workplace sex ratios and harassment risks. We expect women to be at higher risk than men in most workplaces, and their risk will grow with the share of men in the workplace. Men will generally be at lower risk than women, but their risk will grow with the share of women in their workplace. In the most female-dominated workplaces, men's risk might exceed that of women.

The differential risks of harassment for men and women are likely to create a gap in the utility they derive from working in a specific workplace. This situation arises because the preference against the risk of sexual harassment of one's own sex is likely to be larger than the preference against risks for the opposite sex. Risks for one's own sex represent a direct risk of one's own victimization, which has negative consequences for physical and mental health (Welsh 1999; McDonald 2012; Fitzgerald and Cortina 2018). It also represents a worse work environment for one's social in-group (Tajfel 1974). Opposite-sex harassment risks represent a mix of indirect health concerns, including stress from conflicts and tensions in the workplace (reviewed by, e.g., Fitzgerald and Cortina 2018), as well as potential gains such as a

5. This work suggests that men are more harassed in stereotypical male occupations, especially in hypermasculine ones like the military (e.g., Waldo, Berdahl, and Fitzgerald 1998).

culture of camaraderie or positive utility from perpetration that strengthens gender identity (Akerlof and Kranton 2000).

II.B. Theoretical Model

To define individual i's utility from employment in workplace j, we depart from Rosen's (1986) canonical model of compensating differentials. Following Sorkin (2018) we express utility in pay units as

$$U_i = w_j + \rho^g \alpha_j + \epsilon_{ij}, \tag{1}$$

where w_j is the wage, α_j is the level of nonpay characteristics (amenities) provided by the workplace, and ρ^g is the preference for these amenities, which may vary by gender, g. ϵ_{ij} is the individual-specific utility of working in workplace j. The function illustrates how individuals trade off amenities against wages when choosing where to work. The amenity preference ρ^g determines this trade-off. Using this basic model, the sorting of women into lower-paying workplaces can be explained by a stronger preference for costly amenities, such as time-space flexibility.

As outlined already, we expect women and men to have different sexual harassment risks in the same workplace. When adding sexual harassment to the workplace utility function, we therefore add these gender-specific risks as two separate disamenities and define the following gender-specific utility function:

$$U_i = w_j + \rho^g \alpha_j - \pi^g d_j^g - \pi^{-g} d_j^{-g} + \epsilon_{ij.} \tag{2}$$

where d_j^g is the risk of sexual harassment for individual i's own sex and d_j^{-g} is the risk for the opposite sex. π^g and π^{-g} are the corresponding preference parameters that determine the disutility from these risks. Reflecting the foregoing discussion, we expect the preference parameter for one's own sex to be larger than that for the opposite sex, $\pi^g > \pi^{-g}$. This implies that within a workplace, the sex with a greater risk has a greater disutility from sexual harassment.

We can use the utility function (2) to understand how sexual harassment affects sex segregation and wage inequality. To do so, we imagine a labor market where workers choose between two workplaces. By solving for the sex segregation across these workplaces, and for the gender difference in average wages, we

derive comparative statics for if (and how) sexual harassment affects sex segregation and the gender wage gap.

To focus attention on the supply side, we follow Pan (2015) and assume that the size of each workplace is fixed and equal (i.e., each hires 50% of the workers). Each workplace sets the wage needed to attract 50% of the workers. Employers are nondiscriminating and offer men and women the same wage. Amenities are exogenous and we set them to $a_1 = 0$ and $a_2 > 0$.

The labor force is made up of 50% women and 50% men.[6] Each person chooses the workplace from which they derive the largest utility. The gender-specific harassment risks are linear functions of the share of men in the workplace, $d_j^f = \gamma^f m_j$ and $d_j^m = \gamma^m m_j$. To make men and women sort into different workplaces, we assume a greater amenity preference for women, $\rho^f > \rho^m$, and for notational simplicity we set men's amenity preference to zero ($\rho^m = 0$). The difference in workplace-specific utilities, $\epsilon_{i1} - \epsilon_{i2}$, is uniformly distributed between -0.5 and 0.5.

We solve for the equilibrium level of sex segregation, which we define as the difference in the share of men and women in workplace 1, $m_1 - f_1$. Online Appendix A1 shows the step-by-step solution for arriving at this equilibrium level of segregation as:

$$m_1 - f_1 = \frac{\rho^f \alpha_2}{1 - (\gamma^f - \gamma^m)(\pi^g - \pi^{-g})}. \tag{3}$$

This equation shows that sex segregation will grow in women's preference for the amenity ρ^f and the quantity of this amenity offered by workplace 2, α_2. It will also increase with the expression $(\gamma^f - \gamma^m)(\pi^g - \pi^{-g})$, which captures how the gender gap in the (dis)utility from sexual harassment changes with the share of men in the workplace. The first component of this function is $(\gamma^f - \gamma^m)$, which captures how the relative risk for men and women changes with the workplace share of men. It is positive if women's risk relative to men's increases with the share of men in the workplace. The second component, $(\pi^g - \pi^{-g})$, is the difference in preferences for own-sex and opposite-sex risks. It is positive if the preference against risks for one's own sex are larger than the preference against risks for the opposite sex.

6. This setup has the convenient property that the share of men in a workplace is equal to the share of the men who work in this workplace and the probability that a man chooses to work there.

If the relative risk of sexual harassment was unrelated to the workplace sex composition ($\gamma^f = \gamma^m$) or there was no difference in the preference against own-sex and opposite-sex risks ($\pi^g = \pi^{-g}$), sexual harassment would not affect sex segregation. In this case, sex segregation would be entirely driven by women's greater amenity preference and equal to $\rho^f \alpha_2$. Our equilibrium solution shows that sexual harassment amplifies segregation caused by other factors by creating costs for breaking that segregation. While our model relies on the initial segregation to be caused by gender differences in amenity preferences, it could equally well be caused by wage discrimination, discrimination in hiring, or gender differences in productivity.

The amplification mechanism generated by sexual harassment is observationally equivalent to men and/or women preferring not to work with the opposite sex, as in Goldin (2015) and Pan (2015).[7] Our framework can thus be viewed as providing an alternative mechanism for why such a preference would arise.

We define the gender wage gap as the difference in average wages between men and women:

$$\bar{w}^m - \bar{w}^f = (m_1 - f_1) * (w_1 - w_2), \tag{4}$$

where the first component is our measurement of sex segregation $(m_1 - f_1)$ determined by equation (3). Sexual harassment increases the size of this component by making women more likely than men to select the lower-paying workplace, as described already. The second component is the wage-difference between workplaces 1 and 2. Under realistic risk slopes for sexual harassment, that is, that women's risk increases more than men's with the share of opposite sex workers, harassment will increase this gap. The average rate of harassment will be higher in the male-dominated workplace 1, forcing it to pay a higher relative wage to attract half of the workers. With (unrealistic) risk slopes that make the effect of sexual harassment on the second component negative, the effect on the first component dominates in magnitude and the overall effect on the gender wage gap will be positive, which we show in Online Appendix A1.

So far, our model has assumed that workplaces pay men and women the same wage. In Online Appendix A1 we relax this assumption, which is equivalent to introducing wage

7. We show this formally in Online Appendix A1.

discrimination because our framework does not include any gender difference in productivity. The alternative model shows that wage discrimination against women in the male-dominated workplace leads to an increase in sex segregation that is further amplified by sexual harassment.

This section highlights the importance of three specific factors for the role of sexual harassment in workplace sex segregation and the gender wage gap. The first is sexual harassment risks for men and women across workplace sex compositions. Based on the previous discussion, we expect both women's and men's risks to grow with the share of opposite-sex colleagues. The second factor is whether negative preferences against sexual harassment are larger when harassment risks target one's own sex compared with the opposite sex, as the theory assumed. Jointly, these two factors make sexual harassment a disincentive for men and women to enter and remain in workplaces where they help break sex segregation. The third factor is the relationship between workplace sex composition and wages. If male-dominated workplaces offer higher wages, as our model assumed, women's sexual harassment risks will be larger in high-paying workplaces and men's in low-paying ones. Such a pattern in wages and harassment would imply that sexual harassment reinforces the gender wage gap by reinforcing workplace sex segregation.

III. Sexual Harassment And Workplace Sex Ratios

III.A. Data on Self-Reported Sexual Harassment

Our data come from the biannual Swedish Work Environment Survey, which is the basis for official reports about the work environment in the Swedish labor market. It is nationally representative of the employed population aged 16–64 and is stratified by age, sex at birth, occupation, industry, and social class.[8] There is, unfortunately, no additional information on gender besides a binary variable. Both respondents and employers are entirely anonymous. Employers do not participate in the data collection and remain unaware that Statistics Sweden has sampled one or more of their workers. Respondents are therefore unlikely to fear retaliation for truthfully reporting on workplace problems.

8. Documentation in Swedish for the survey methodology is accessible via the search term "Arbetmiljö-undersökningen" on Statistics Sweden's homepage, http://www.scb.se.

Because the data are never used to report work environments for individual firms, respondents cannot use the survey to damage their employer's reputation.

We use five survey waves between 1999 and 2007. Since only 2 of the survey's 120 questions are about sexual harassment from colleagues or managers, the topic has a low salience, which should help prevent social desirability bias. The two questions ask about harassment in the past 12 months:

i. In the following question, sexual harassment is defined as unwanted advances or offensive remarks about things that would commonly be associated with sex. Are you the subject of sexual harassment at work from supervisors or colleagues?[9]
ii. The next question concerns whether you have been exposed to other behaviors than the ones above, which have [also] been based on your gender and which have violated your integrity or been degrading. This could include, for example, condescending and ridiculing statements about women or men in general or in your occupation. It could also include that someone, because of your gender, failed to pay attention to you or your statements. Are you exposed to harassment of this kind from supervisors or colleagues at your workplace?[10]

Our main measurement of sexual harassment is a dummy variable that takes a value of 1 for respondents who answer affirmatively to either question. Sensitivity analyses show that our results replicate for other frequency thresholds and for each question separately.

The survey questions cover three of four types of sexual harassment in a common typology (e.g., Fitzgerald et al. 1999,

9. In Swedish: Med sexuella trakasserier menas i följande fråga ovälkomna närmanden eller kränkande anspelningar kring sådant man allmänt förknippar med sex. Är du utsatt för sexuella trakasserier på din arbetsplats från *chefer* eller *arbetskamrater*?

10. In Swedish: Nästa fråga gäller om du är utsatt för andra handlingar än ovan som grundas på ditt kön och som kränker din integritet eller är nedvärderande. Det kan t.ex. vara nedsättande och förlöjligande omdömen om kvinnor eller män i allmänhet eller inom ditt yrke. Det kan även innebära att man på grund av ditt kön inte tar notis om dig eller din mening. Är du utsatt för trakasserier av ovanstående slag på din arbetsplats från *chefer* eller *arbetskamrater?*

building on Till 1980; see Online Appendix Table A1 for more information). The first question covers two types. Unwanted sexual attention is physical behaviors that range from groping to rape, and sexual hostility is nonphysical behaviors such as crude sexual comments, revealing body parts, or showing pornographic images. The second survey question covers a third type, sexist hostility, defined as insulting, degrading, or contemptuous attitudes about women or men in general or in an occupation.[11] This harassment type is sometimes called gender harassment and left out of the definition of sexual harassment, for example in the EU regulations discussed in note 2. The fourth type of harassment is not covered by our questions and is by far the least common type:[12] sexual coercion includes behaviors that link unwanted physical behaviors to professional threats of punishment or promises of rewards (Waldo, Berdahl, and Fitzgerald 1998; Fitzgerald and Cortina 2018).

One of our survey questions defines "sexual harassment" and gives examples. This question requires the respondent to label their own experiences as sexual harassment, a requirement that is also present in the legal definition. It introduces an element of subjectivity in our data, which has important consequences for the content of our variable and interpretation of the results. The probability of labeling an experience as sexual harassment is greater for more severe or frequent events and when the perpetrator is a supervisor (Stockdale, Vaux, and Cashin 1995; Ilies et al. 2003; Antecol and Cobb-Clark 2004, 2006). Our variable will therefore capture events with more expected negative labor market consequences than what would have been the case for a list-based survey instrument without subjectivity, such as the Sexual Experiences Questionnaire (see Online Appendix Table A1). Because women are more likely than men to label

11. The question also includes experiences of being ignored because of one's gender, which is closer to the academic definition of selective incivility than sexual harassment. Andersson and Pearson (1999, 457) define workplace incivility as "low intensity deviant behavior with ambiguous intent to harm the target, in violation of workplace norms for mutual respect. Uncivil behaviors are characteristically rude and discourteous, displaying a lack of regard for others."

12. We can partly verify this in Swedish data. In the 2001 National Violence Against Women Survey, only 0.2% of the employed respondents replied that someone had "threatened to impair your career prospects if sexual favors were refused" in the past 12 months (authors' calculations based on data from Lundgren et al. 2001).

experiences sexual harassment (e.g., Marshall 2005), we run all analyses in separate subsamples by sex at birth.

The subjective element may also be the source of measurement error. For our analysis of workplace sex ratios, we note that previous research finds no difference in the likelihood that women who are gender minorities or majorities in their workplace will consider experiences to be sexual harassment (Antecol and Cobb-Clark 2006). But a minority status might correlate with other sociodemographic traits that affect labeling, like marital status or education (Antecol and Cobb-Clark 2004, 2006). We thoroughly check for this type of measurement error by holding constant fine-grained individual traits for the analysis of workplace sex ratios and for wages and turnover. The stability of our results to these and other sensitivity checks suggests that we are able to capture variation in, and the consequences of, actual harassment, rather than merely people's tendency to label their experiences as such.

III.B. Workplace Sex Ratios

We use Sweden's mandatory ID codes to link survey respondents to administrative data. We define workplaces—and thereby colleagues—with unique combinations of two ID codes, both of which apply to the respondent's largest source of earned income in a year. The Organization ID defines a firm, such as Walmart, and the Workplace ID defines a single building or street address where that firm has operations. People with the same combination of codes work in the same physical workplace—in this example, the same Walmart store. We calculate the sex ratio as the share of men after excluding the respondent. Information on sex at birth comes from the Database for Health Insurance and Labor Market Studies (LISA, based on the Swedish acronym, which is also our data source for occupation codes, wages, and other socioeconomic traits such as year of birth, region of birth, and years of education).

III.C. Sample Size and Representativeness of the Work Environment Survey

The five pooled cross sections of the Work Environment Survey (1999–2007) sampled a total of 73,540 people. Of these, 62,050 responded to the telephone portion of the survey and 46,083 (63%) to the written questions that include the sexual harassment items. We drop 1,043 respondents who did not respond to the

sexual harassment questions. We then drop people in workplaces with fewer than five people in the workforce. The final sample size is 37,439 (year-by-year details in Online Appendix Table A2).

The survey sample is representative of the sociodemographic traits in the general population. In Online Appendix Table A3, columns (1)–(3), we report the means of demographic variables in the administrative data for the five survey years, restricting the data to employed people aged 16–65 to match the survey.[13] Columns (4)–(6) report the means for the survey sample. The comparison shows highly similar proportions of women and people in different categories of age, education level, and income. The only trait that differs in a meaningful way is the share of foreign-born people, which is lower in the survey.

We use probability weights throughout the article to match the survey data to the employed population. The same weighting scheme is used for the Work Environment Survey and the subsequent survey experiment. Our weights are based on dummy variables for sex at birth, age categories (< 30, 30–39, 40–49, 50–59, and 60+), and education level (no tertiary education, at least some tertiary education). These variables exist in both data sets, and the categories are broad enough to give a reliable number of observations for each interaction.

III.D. Descriptive Statistics

The rate of self-reported sexual harassment in the past 12 months is 12.6% for women and 4.2% for men in the pooled survey sample. These rates barely change over time in the five cross sections (Online Appendix Figure A1).[14]

Table I shows how the probability of self-reporting sexual harassment varies across individual and employer attributes and across occupations. Younger women self-report more harassment than older women, and highly educated women report more than those with less education. Men's self-reports barely correlate with age or education but display a stronger correlation with region

13. We use the S*ysstat* variable from the Swedish labor statistics to drop nonemployed individuals.

14. The questions on sexual harassment were incorporated into the Work Environment Survey shortly after the first incorporation of sexual harassment into Swedish law in the 1990s. Awareness of the concept is expected to have grown over time so that more events will be defined, or recalled, by respondents (U.S. Merit Systems Protection Board 1994; Antecol and Cobb-Clark 2004).

TABLE I

SELF-REPORTED SEXUAL HARASSMENT FROM COLLEAGUES OR MANAGERS IN THE PAST 12 MONTHS

		Women	Men
Individual attributes	Age (years)		
	18–35	0.16	0.05
	36–50	0.12	0.04
	51–65	0.08	0.03
	Education		
	Below high-school education	0.08	0.04
	High school	0.11	0.04
	At least some tertiary education	0.16	0.05
	Region of birth		
	Sweden	0.12	0.04
	EU28, excl. Sweden	0.12	0.05
	Outside EU28	0.13	0.11
	Productivity		
	Self-estimated above median†	0.13	0.03
	Self-estimated below median†	0.14	0.06
	AKM individual FE above median	0.16	0.04
	AKM individual FE below median	0.11	0.05
	Family type		
	Married or cohabitant with children	0.11	0.04
	Married or cohabitant without children	0.08	0.03
	Single or divorced with children	0.11	0.04
	Single or divorced without children	0.13	0.06
Employer attributes	Firm size		
	1–10 people	0.12	0.03
	11–25 people	0.11	0.04
	26–100 people	0.12	0.04
	101–500 people	0.14	0.04
	500+ people	0.13	0.05
	Workplace percent male		
	0%–10%	0.05	0.14
	11%–25%	0.10	0.09
	26%–75%	0.18	0.04
	76%–90%	0.24	0.04
	91%–100%	0.24	0.03
Occupational attributes	Average occupational wage		
	Lowest third of the distribution	0.10	0.05
	Middle third	0.15	0.04
	Highest third	0.18	0.04
	Major occupation groups (ISCO-08, single-digit level)		
	Managers, senior officials, and legislators (67% men)	0.14	0.03
	Professionals (42% men)	0.17	0.05
	Technicians and associate professionals (41% men)	0.13	0.05
	Clerks (26% men)	0.13	0.06
	Service and sales workers (16% men)	0.07	0.09
	Skilled agricultural, fishery, and forestry workers (69% men)	0.18	0.03
	Craft and related trades workers (94% men)	0.18	0.03
	Plant and machine operators and assemblers (81% men)	0.20	0.04
	Elementary occupations (40% men)	0.09	0.06
	Armed forces occupations (100% men)	—	0.01

Notes. The table shows summary statistics for five pooled cross sections of the Work Environment Survey (1999, 2001, 2003, 2005, and 2007). Variables are either measured in this survey data or in administrative data linked to survey respondents via a mandatory personal ID code.

†Self-stated productivity is measured on a scale of 1–10 by the survey question "Let's assume that your work capacity, when at its peak, was valued to 10 points. What number of points would you give to your work capacity today?"

of birth. Men born outside of Europe self-report at twice the rate (11%) of men born in Sweden (4%) or Europe (5%). Neither women's nor men's self-reports correlate with marital status, parental status, or productivity at work. Looking at the employer attributes, we observe no correlations with workplace size but a strong correlation with the workplace sex composition, a result we further explore below. Across occupations, women self-report more harassment in higher-paying ones while men's self-reports are uncorrelated with the occupation's average wage level. Comparing rates for one-digit occupation codes, women have the highest rate among plant and machine operators and assemblers (20%), and men have the highest among service and sales workers (9%).

Online Appendix Table A4 displays a more detailed comparison of occupations by listing the three-digit job titles with the highest and lowest rates of harassment (subject to $N > 50$). Strongly male-stereotyped occupations clearly have the highest harassment rates for women: motor vehicle drivers (40%) and engineers and architects (35%). Women's rates are also high among writers and artists (31%) and college and university professors (28%). Men's harassment rates are the highest in care-related, women-stereotyped occupations, which mirrors anecdotal evidence of sexual harassment against men in the Swedish press (Aftonbladet 2002, 2011). These occupations include kindergarten teachers (20%), personal care workers (17%), nursing (17%), and social work (14%).

When comparing self-reports of sexual harassment to self-reports of other forms of interpersonal mistreatment at work, we observe strong correlations between the two. People who self-report harassment are more likely than others to also report bullying and conflicts with colleagues or managers. They are also more likely to report a lack of support or appreciation. Online Appendix Table A5 shows these relationships for seven survey questions and a combined principal component (PC) score for interpersonal (mis)treatment in the workplace.[15]

15. The seven variables for bullying, conflicts with colleagues or supervisors, and lack of support or appreciation from colleagues or supervisors are suitable for a PC analysis according to the Kaiser-Meyer-Olkin test (all statistics are above 0.5 and the overall value is 0.6). There are two relevant PCs in the data (lambda > 1). The first captures a negative interpersonal work environment; all component loadings are above 0.3 and take the correct signs for this interpretation. The proportion of explained variance is 0.32. The second relevant PC has an eigenvalue

III.E. Validating Self-Reported Sexual Harassment as a Disamenity

It is well established that sexual harassment damages a person's health. These effects are generally larger than for other aggressive peer behaviors in the workplace and of considerable size even at lower frequencies of harassment or for behaviors that might be considered less severe (e.g., Schneider, Swan, and Fitzgerald 1997; Sojo, Wood, and Genat 2016). For example, Leskinen, Cortina, and Kabat (2011) find significant reductions in psychological health for women who are subjected to gender harassment alone.

In our data, people who self-report sexual harassment in the past 12 months are more dissatisfied with their jobs and have more feelings of unease when going to work. Linear regressions quantify these gaps to approximately 0.5 standard deviations (results reported in Figure I). These gaps are robust to controlling for three-digit occupations, various sociodemographic traits, the gender of the respondent's closest manager, the share of men in the workplace, and workplace fixed effects. They are more sensitive to a control for the PC score for negative interpersonal work environments. But even after holding constant these correlated forms of mistreatment, people who self-report sexual harassment have about 0.2 standard deviations higher job dissatisfaction and feelings of unease when going to work.

III.F. Sex Ratios and Sexual Harassment Incidence

As already described, women in our data are three times more likely than men to self-report sexual harassment in the last 12 months (12.6% versus 4.2%). We now plot how self-reported harassment varies with the share of men in the workplace (left side of Figure II). These relationships go in the opposite directions for men and women. A 1 percentage point higher share of men in the workplace is associated with a 0.25 percentage point increase in the average likelihood that women will have self-reported sexual harassment in past 12 months. For men, the likelihood of self-reporting harassment declines with the share of men in the workplace. A 1 percentage point increase in

of 1.5 but no straightforward interpretation. Component loadings take different signs, and two are below 0.3.

FIGURE I

Validation of Self-Reported Sexual Harassment as a Disamenity

The figure shows differences in job dissatisfaction and feelings of unease when going to work between people with and without self-reported sexual harassment in the Swedish Work Environment Survey. Job dissatisfaction is measured with the question "Do you feel very dissatisfied or very satisfied with your job?," answered on a five-point scale ranging from "Very satisfied, completely agree" to "Very dissatisfied, completely agree." Unease is measured with responses to the question "Does it happen that you feel unease going to work?" answered on a five-point scale ranging from "Not at all, seldom the last three months" to "Every day." Both variables are transformed to z-scores and regressed on a dummy variable for self-reporting sexual harassment from colleagues or managers in the past 12 months. Occupation fixed effects are at the three-digit level of the Swedish occupation code (SSYK). Female supervisor is a self-reported variable in the Work Environment Survey. Demographics controls are four dummies for marital and parental status, four dummies for age categories, two dummies for having secondary or tertiary education, and two dummies for being born in a different European country or outside of Europe. Interpersonal mistreatment is a PC score for a negative interpersonal work environment (see note 15 for details).

the share of men is associated with a 0.07-percentage-point lower likelihood of self-reporting harassment. Relating this result back to the theoretical model confirms that women's and men's risks of sexual harassment grow with the share of opposite-sex workers.[16]

An extended descriptive analysis reflects on the discussion at the beginning of Section II regarding why sexual harassment risks would vary across workplace sex ratios. We regress the

16. In addition, since women's harassment risk grows more than three times as much, the theoretical condition for sexual harassment expanding the wage difference between male- and female-dominated workplaces is met.

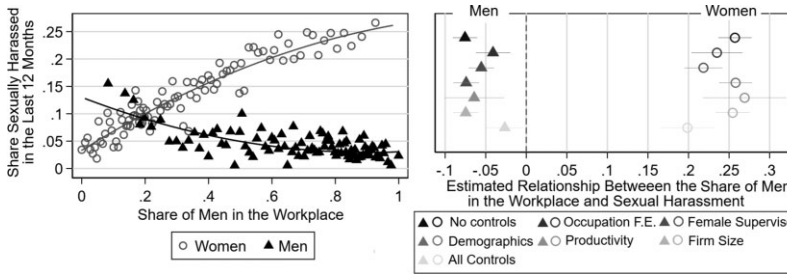

FIGURE II

Sexual Harassment Incidence across Workplace Sex Ratios

The left panel shows binned averages of a binary variable for self-reports of sexual harassment in the past 12 months from colleagues or managers. Each subsample of men and women is split into 100 equally sized bins of the X-variable. The data consists of five pooled cross sections of the Swedish Work Environment Survey. A workplace is defined as a unique combination of plant and organizational ID codes, and we calculate the share of men in each workplace in population-wide register data. The right figure shows estimates from OLS regressions of the dummy variable for sexual harassment on the share of men in the workplace. Productivity is individual fixed effects from an AKM model and self-stated productivity in the Work Environment Survey. Firm size is dummies for five categories, calculated in the administrative data. Other control variables are described in the notes to Figure I. $N = 19{,}975$ for women, and 17,482 for men.

sexual harassment dummy on the share of men in the workplace, add a sequence of control variables, and report the estimated coefficient on the share of men in the right side of Figure II. The first estimate shows that adding fixed effects for three-digit occupation codes accounts for about 50% of the correlation for men and 10% for women.[17] This indicates that gender norms linked to occupation choices play a role in creating harassment risks for workplace gender minorities. At the same time, the remaining relationships mean that "where" people work matters for sexual harassment also after controlling for "what" they work with, echoing the results in Gruber (1998).

We approximate the respondent's position in the workplace's gender power hierarchy by controlling for the gender of their

17. Another indication that gender norms linked with occupational choices matter more for men than for women is the nonlinearity for men in the left side of Figure II. Men generally self-report a low harassment rate, but the rate jumps upward from 8% when the share of women is around 80% to 15% when it exceeds 90%. This pattern is consistent with harassment occurring in response to violations of gender norms, because norms should be the strongest in the most sex-imbalanced workplaces. Online Appendix Figure A2 extends this analysis by replicating Figure II for occupation sex ratios. Again, we see a nonlinear pattern of harassment for men, but not for women.

closest supervisor. This reduces the correlation with the workplace sex ratio by 15% for women and 30% for men. Both men and women self-report more sexual harassment when they have an opposite-sex supervisor. Dynamics of gender and power thus appear to partially explain why workplace minorities, especially men, self-report more harassment.

Neither the control for occupation nor supervisor gender reduces the estimated relationship to zero. This offers suggestive evidence in support of the contact hypothesis, according to which sexual harassment risks grow "mechanically" with the share of opposite-sex workers in a person's everyday work environment.

By adding demographic traits as control variables to the regression, we can comment on measurement error from differential labeling probabilities across socioeconomic groups. Nothing happens to the estimated relationship when we hold the respondents' age, education level, marital and parental status, and immigration background constant. Further substantiating this result for the education variable, we show that although the relationships in Figure II are stronger among men and women with tertiary education, there is also a clear relationship for those with less education (Online Appendix Figure A3). This robustness to sociodemographic traits is important. Workplace gender minorities are more likely to self-report sexual harassment regardless of their demographic traits, contradicting the idea that our results in this section merely reflect systematically different probabilities across sociodemographic groups of labeling an experience sexual harassment.

The fourth and fifth regressions in Figure II capture robustness to firm size and individual productivity. To measure productivity, we follow Abowd, Kramarz, and Margolis (1999) and use individual fixed effects from a wage regression with two-way fixed effects (see Online Appendix A4 for details). Robustness to this control contradicts the idea that gender minorities are, for some reason, less productive at work and blame this (consciously or unconsciously) on interpersonal mistreatment.

The results from several additional analyses bolster our confidence that Figure II captures variation in actual harassment rather than the probability of labeling certain behaviors as harassment. The results look the same when we only use data for the survey question that does not use the term "sexual harassment" (Online Appendix Figure A4). We might worry that gender minorities overreport sexual harassment if feelings of isolation

cause perceptions of more interpersonal hostility. Yet the absence of higher rates of self-reports among these minorities for other forms of mistreatment such as bullying or conflicts contradicts this idea (Online Appendix Figure A5). None of our seven survey questions on mistreatment shows the same pattern of systematically higher rates for gender minorities; nor do they demonstrate inverse relationships that could have indicated that minorities reclassify other forms of mistreatment as sexual harassment.[18]

Several theories would predict underreporting rather than overreporting of sexual harassment among gender minorities. For example, the theory of compensating differentials would predict that people with a relatively small distaste for a specific disamenity would sort into workplaces where it exists (Rosen 1986). Sociologists make similar predictions about the "assimilation" of women minorities (e.g., Timmerman 2005). Women who choose male-dominated jobs are more likely to view sexist behaviors as a nonremarkable part of the ordinary workday, making them less likely to recall these as problematic in a survey.

The Online Appendix contains two final robustness tests. We replicate Figure II for an alternative, higher-frequency cutoff for the sexual harassment variable (Online Appendix Figure A6) and use a more complex set of sample weights that include region of birth and income (Online Appendix Figure A7).

IV. Valuations Of Workplace Sexual Harassment

We use a survey experiment with hypothetical job choices to quantify valuations of sexual harassment risks. An online sample of 5,699 employed Swedish citizens received the survey, and with a 70% response rate our final sample size is $N = 3{,}987$.[19]

18. The PC score for interpersonal mistreatment is higher for men and women in male-dominated workplaces, a pattern that could indicate measurement error in the sexual harassment variable for women gender minorities. But the correlations with other individual survey questions illustrate that this correlation with the PC score comes from a single variable—lack of appreciation from colleagues—rather than a general pattern of higher self-reports across different forms of mistreatment. This makes us reluctant to interpret the correlation as evidence of measurement error, and we lean toward the interpretation that female colleagues are simply more agreeable than male ones.

19. The Swedish Citizen Panel (*Medborgarpanelen*) is an online survey panel maintained by the Department of Political Science at the University of Gothenburg. It has been used in more than 100 social science studies in the past decade.

IV.A. Experiment Design

We present the survey module to participants as "a research study on job choice" and say that it includes three job choices, each one between two fictional jobs. The introductory text for these choices asks respondents to "imagine that you are looking for a job and have two offers. You will see short descriptions of these offers and choose which one you would be most likely to accept. The jobs differ in four attributes: the monthly wage, schedule flexibility, the tasks, and the work environment. Other than this, they are identical to each other. The table below shows your first choice." After this text comes a table describing the two jobs (A and B) with two checkboxes to indicate the choice (Online Appendix Figure A8 contains a screenshot). The design broadly follows the structure introduced by Eriksson and Kristensen (2014) and used by, for example, Wiswall and Zafar (2018), Mas and Pallais (2017), and Maestas et al. (2018).

We randomize the order of the rows and values of the job traits in each job choice table. Values for the first three job traits are drawn with equal probabilities from a list, summarized here and appearing in full in Online Appendix Table A6. Values for the wage were "5% less than your current wage," "approximately the same as your current wage," "5% more," or "10% more."[20] Values for the degree of skill development offered by the job were "low,"

Panel members are invited to take a few surveys per year on their computer, tablet, or phone. Respondents do not receive compensation but are told that they contribute to cutting-edge social science research. There are currently 60,000 panelists, 90% of whom self-selected into the survey by signing up online. The remaining 10% were recruited directly by the university to increase social representativeness, mainly by post or text messages to samples provided by Statistics Sweden, by providing a web link to people who take Swedish public TV's Electoral Compass test, or via civic societies. This type of internet sample is frequently used in stated-preference experiments for job traits (e.g., Mas and Pallais 2017; Maestas et al. 2018). For more information, see https://medborgarpanelen.gu.se/english.

20. These values reflect the distribution of actual wage changes on job switches in the Swedish labor market. We calculated the distribution of year-on-year changes in deflated monthly wages for people who switched jobs from one year to the next in the Swedish Salary Statistics (*Lönestrukturstatistiken*) and over the period of 1996–2015. After dropping outliers according to the 1.5-interquartile range rule, the average wage change was 3.2% with a standard deviation of 11%. Fewer than one-third of the switchers had a negative wage change, and about 60% of the observations fall within our band of −5 to +10%.

"moderate," or "high."[21] Values for schedule flexibility were "entirely flexible," "about one hour of flexibility at the beginning and end of the workday," or "no own influence over the work times."

Too many job traits can dilute the quality of the data by giving respondents too many characteristics to compare. We therefore chose not to include some traits from previous research that vary relatively little between workplaces in the Swedish context. This includes the length of paid vacation, access or cost of health care or child care, and access to part-time work, which is a legal right in Sweden. Labor laws also give less variation in job security, and only 14% of the workforce has a work week exceeding 40 hours (Statistics Sweden 2017).

IV.B. Sexual Harassment

We present vignettes about sexual harassment in the job trait titled the work environment. This trait has four values: (i) a good work environment in which "most people in the work unit seem content with the work environment"; (ii) a null condition of "no particular information"; (iii) a negative vignette in which "some employees in the work unit have had conflicts with the manager"; and (iv) vignettes for sexual harassment. To gain statistical precision, we oversampled the sexual harassment vignettes in the randomization. Each vignette appears with an 11% probability, meaning that respondents have a 33% probability of seeing a sexual harassment vignette in a job and a 22% probability of seeing each one of the other three values.

Several considerations affect how we present the information about sexual harassment. Unlike amenities like schedule flexibility or pay, the law prevents employers from contracting on sexual harassment risks (Basu 2003). Rather than guaranteeing a "zero harassment risk" or "some harassment risk" in a contract, information about sexual harassment usually arrives by word of mouth. Vignettes of harassment incidents mimic this type of imprecise anecdote or rumor that a job seeker might hear about a potential employer (following Hulin, Fitzgerald, and Drasgow 1996). Additional advantages of vignettes include avoiding the term "sexual harassment" and respondents' interpretations of that terminology, as well as the ability to include several harassment

21. Note here that the Swedish word for skill development (*utvecklande jobb*) can refer to technical, interpersonal, or other types of skills.

TABLE II
VIGNETTES FOR SEXUAL HARASSMENT IN THE HYPOTHETICAL JOB CHOICE EXPERIMENT

		Type of harassment		
		Sexist hostility	Sexual hostility	Unwanted sexual attention
Sex ratio of the respondent's workplace	Female-dominated	Women in the work unit have expressed that men are not suitable for the job	A woman in the work unit makes male coworkers uncomfortable by stubbornly discussing her sex life.	A woman has groped a man in the work unit.
	Mixed or Male-dominated	Men in the work unit have expressed that women are not suitable for the job	A man in the work unit makes female coworkers uncomfortable by stubbornly discussing his sex life.	A man has groped a woman in the work unit.

behaviors. Showing several behaviors avoids skewing the results by presenting either a certain type of harassment or a relatively uncommon behavior. We use data collected by the authors with a list-based survey instrument in the United States and Japan to select the most common behavior from the three most common types of harassment. Table II shows these vignettes in full.

Each harassment vignette describes the gender of the victim and the perpetrator, for example that "A man has groped a woman in the work unit." This is where we set the gender of the victim to be either the same as or the opposite of the respondent. To make the situation as realistic as possible, we assign the gender of the victim based on the respondent's own labor market context. This information was collected before the job choice questions as a categorical survey question about the sex composition of the respondent's current workplace (mostly women, about the same proportion of men and women, or mostly men).[22] Drawing

22. All our survey respondents are currently employed, but a small fraction responded "Don't know/Don't have any colleagues" when asked about the sex

directly on our previous findings about risk patterns for women and men in Figure II, we show male victims to respondents in female-dominated workplaces and female victims to respondents in mixed or male-dominated ones. This method assigns 33% of the respondents to see a victim of their own sex and 66% to see a victim of the opposite sex.

The vignettes do not mention an exact numerical risk of sexual harassment against the victim's sex. This makes them more realistic and reduces their salience in the job choice table (the screenshot in Online Appendix Figure A8 shows how even the longest vignette is not striking in length or detail). This lack of detail allows respondents to project their own perceptions of the risk of harassment in the fictional workplaces. To verify that respondents interpret the vignettes as information about a higher harassment risk, we collected additional survey data from the same online panel ($N = 3{,}180$, details in Online Appendix A2).[23] After seeing the original job choice table, the questions pointed to fictional jobs with or without sexual harassment vignettes and asked respondents to rate the likelihood that situations like the one in the vignettes would happen there in the future. The scale ranged from 1 (single event/essentially never) to 5 (recurring problem).

When they had "no information about the work environment," respondents perceived there to be some harassment in the fictional workplace (an average score of 2.5 on the frequency scale from 1–5). The vignettes increased this perceived quantity by nearly one standard deviation to an average of 3.5. The distribution of responses (Online Appendix Figure A15) illustrates that the vignettes shifted a sizable portion of respondents toward the top two categories where harassment was perceived as a recurring problem—48% in the vignettes compared with 11% in the reference category. These results verify that the vignettes shifted perceptions by a meaningful amount. This amount is also highly similar across our subsamples of interest: respondents who saw a harassment victim of their own sex versus the opposite sex, and men versus women (details in Online Appendix Table A9). Different interpretations of the job choice tables in terms of harassment quantities are therefore not a concern when interpreting willingness to pay (WTP) estimates across subsamples.

composition. These respondents were forwarded to a second question asking for the sex composition in "a typical workplace where people with your occupation/education have colleagues."

23. We thank anonymous reviewers for this suggestion.

IV.C. Sample Size and Representativeness

We remove data for job choices where one job has the same or better values on all traits than the other (39% of the observations). This leaves data from 7,439 job tables. Online Appendix Table A3 compares the traits of the respondents to the Swedish workforce at large. Respondents have a similar age distribution as the full workforce except for a strong underrepresentation of the youngest age bracket. They are also somewhat more likely to be men (58%) and to have a tertiary education (77% versus 34%) and a high income (77% versus 25% have a monthly income over 30,000 SEK, ∼US$3,000). We use the weighting scheme described in Section III to correct these imbalances.

IV.D. Calculating the Willingness to Pay

We measure valuations of work conditions using WTP. The experiment generates data at the level of the hypothetical job. Following Hainmueller, Hopkins, and Yamamoto (2014), we create a dummy variable (Y_{itj}) to indicate whether individual i selected a specific job j in table t. We use linear OLS to regress this variable on one dummy for each value of each job trait, leaving out one dummy per trait as the reference category. Standard errors are clustered at the level of the respondent. Writing out the variables used for the WTP calculation and representing the remaining ones as a vector $\boldsymbol{X}_{itj}$, the regression equation is:

$$Y_{itj} = a + \beta_1 W_{itj}^{-5} + \beta_2 W_{itj}^{+5} + \beta_3 W_{itj}^{+10} + \beta_{sh} SH_{itj} + \boldsymbol{\beta_x} \boldsymbol{X}'_{itj} + e_{itj}. \quad (5)$$

For the main analysis, we pool the three harassment vignettes into a single dummy variable (SH_{itj}). The coefficient β_{sh} on this variable shows the percentage point difference in the proportion of people who chose a job with a vignette compared with the reference category of "no information." For the wage trait, we set the reference category to "approximately the same as your current wage." The coefficients on the dummy variables for the other three values of this trait (5% less, 5% more, or 10% more) show the percentage point difference in job choices between each of those values and the reference category. After estimating equation (5), we calculate the WTP as the ratio between the coefficient for sexual harassment and a weighted average of the three wage coefficients. The precise formula is $WTP_{sh} = \frac{\beta_{sh}}{\frac{1}{3}(-\frac{\beta_1}{0.05} + \frac{\beta_2}{0.05} + \frac{\beta_3}{0.10})}$.

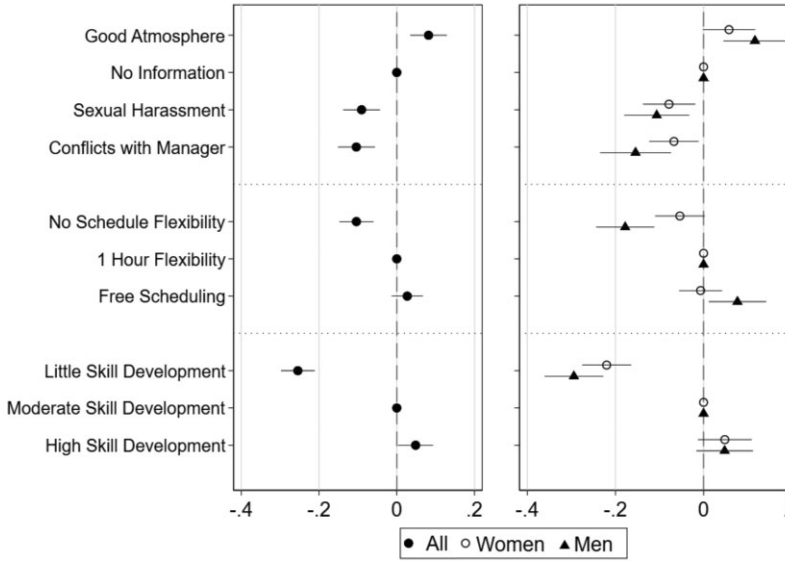

FIGURE III

Willingness-to-Pay Estimates from Job Choice Experiments

95% confidence intervals are adjusted for clustering by respondent. Each respondent provided responses to three stated-preference experiments. WTP estimates are expressed as a percentage of the respondents' wage. Results are weighted using population weights; unweighted counts in the stated-preference estimates are $N = 14{,}516$ in the full sample, whereof $N = 5{,}978$ women and $N = 8{,}538$ men.

It captures the wage change in percent that makes the average respondent indifferent between a workplace with a sexual harassment vignette and one in which they have "no information" about the work environment. For example, a WTP of -0.05 means that people require a 5% higher wage to be indifferent. A more useful way to express the same estimate is that the average respondent's aversion to a workplace with a sexual harassment vignette is equivalent to their aversion to a workplace with a 5% lower wage.

IV.E. Results

Figure III shows the full set of WTP estimates among all respondents and for men and women separately. All estimates are of the expected signs and reasonable in magnitude. Our respondents are willing to pay a 10% lower wage in exchange for schedule flexibility, which is almost identical to the 9% estimate in Maestas et al. (2018).

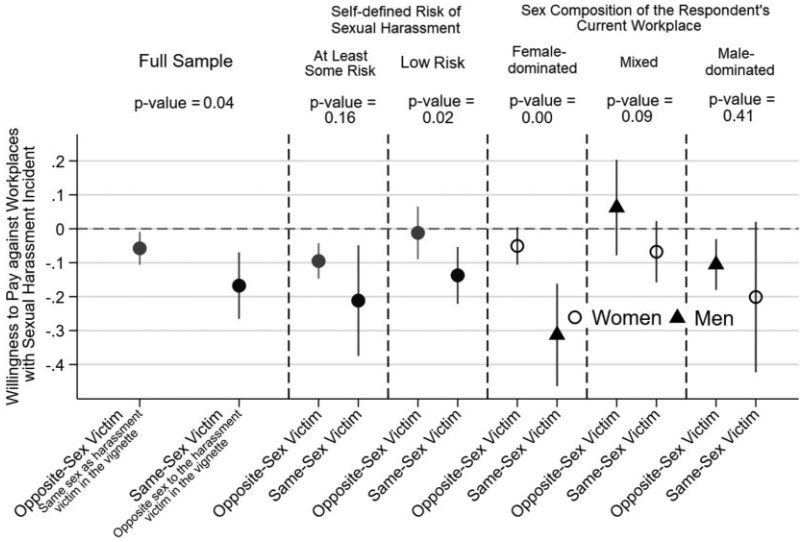

FIGURE IV

Estimates of the Willingness to Pay for Sexual Harassment

In this figure, 95% confidence intervals are adjusted for clustering by respondent. Each respondent provided responses to three stated-preference experiments. Willingness-to-pay estimates from stated-preference experiments are expressed as a % of the respondents' wage. The figure shows WTP estimates for respondents with a perceived "low risk" (45%) and those with a "fairly low risk" or higher (55%) based on answers to the question "Imagine a job offer from a workplace where you know that sexual harassment has occurred. How would you rate your own risk of harassment if you accepted this job?" All results are weighted using population weights. Unweighted counts in the stated-preference estimates are $N = 10{,}050$ for the pooled sample for those who see a opposite-sex victim, and $N = 4{,}808$ for the pooled sample who see a same-sex victim.

The average respondent has a large negative valuation of workplaces where our vignettes signal a greater quantity of harassment. The average respondent's negative valuation of this information is equivalent to a 10% lower wage. Men have a slightly larger valuation than women—10.6% compared with 7.9%—but this gap does not reach statistical significance at the 5% level.

We split the sample to test for different valuations for harassment risks for the respondent's own and opposite sex. We focus on the WTP for sexual harassment and plot this estimate for each group—respondents who see a same-sex victim and those who see an opposite-sex victim—in Figure IV. These results reveal different reactions to the vignettes which, as described

already, convey information about a greater quantity of harassment against either the own or opposite sex. The aversion to this information among those whose vignette features an own-sex victim is equivalent to a 17% lower wage. Those who see a victim of the opposite sex also value the information negatively, but at the equivalent of a 6% lower wage. This difference is statistically significant at the 5% level.

The rightmost section of Figure IV shows gender-separated estimates for respondents who see a victim of their own or opposite sex in each of the three sex composition categories (female dominated, mixed, and male dominated). The results are less precise in these subcategories, but all three display a larger negative valuation among respondents who see a victim of their own sex. We observe the largest estimate for men in female-dominated workplaces and the second-largest estimate for women in male-dominated workplaces.

We use a heterogeneity test and descriptive analysis to comment on possible mechanisms to explain why those who see a victim of their own sex have a larger negative valuation of the increased harassment risk. As discussed in the conceptual framework, two possible explanations include personal health risks and in-group altruism. After making the hypothetical job choices, we asked respondents to assess their personal risk by asking them to "imagine a job offer from a workplace where you know that sexual harassment has occurred. How would you rate your own risk of harassment if you accepted this job?" We split the sample into one category for respondents who consider their risk to be "low" and one category for all others (denoted "At least some risk"). A larger proportion of respondents who see a victim of their own sex belongs to the latter category (64% compared with 50%). The estimated WTP for sexual harassment is also larger among these respondents (middle of Figure IV), suggesting that respondents' own health and safety concerns might help explain the larger WTP for the own-sex risks in the full sample. But we also see a significant and large gap in valuations between those who perceive their risk to be low. This suggests that the larger WTP cannot only be explained by own risks and that in-group altruism may also play a role for why respondents react more negatively to seeing a victim of their own sex in the vignette.

Additional questions in the follow-up survey (Online Appendix A2) provide little evidence of mechanisms linked to respondents' interpretations of the vignettes. Those who see a

victim of their own or opposite sex perceived similar levels of harm to the victims and were equally likely to label the behaviors "sexual harassment" (Online Appendix Table A9). Future research could look more deeply into these and other mechanisms.

IV.F. Reliability of the Survey Experiment Results

Hypothetical job choice experiments have several advantages over observational data. Whereas observational data generally lack information on rejected jobs, our experiment lets us observe both accepted and rejected "offers" (for a formal discussion of this issue, see Wiswall and Zafar 2018). It also allows randomized information on sexual harassment incidents, which would not be ethical in a natural experiment. Moreover, recent research using randomized content in job advertisements shows that the estimated valuations from survey experiments match those obtained in natural experiments for the same job trait (He, Neumark, and Weng 2021; Mas and Pallais 2017). However, our approach may suffer from two types of bias.

The first potential drawback of our method is hypothetical bias. Data from a survey experiment risks being less reliable for choices that are unrealistic or unfamiliar to the respondent (Mas and Pallais 2020). The fact that our vignettes portray a victim from the sex that faces the highest risk in the respondent's own labor market context should make the situations more realistic (compared with, for example, letting men in male-dominated firms evaluate incidents involving male harassment victims). We conduct a more direct test by splitting the sample into groups of people for whom we expect different levels of familiarity. We expect higher familiarity for respondents who are themselves aware of sexual harassment cases in firms in their industry. A survey question about this awareness lets us divide respondents into two subsamples of either "no awareness" (33%) or "awareness of one or more cases" (66%). A second respondent group with higher familiarity should be occupation gender minorities, because these minorities self-report more harassment (as shown in Online Appendix Figure A2). Comparing WTP estimates across these subsamples contradicts hypothetical bias as a source of our results (see the two left-most subsamples in Online Appendix Figure A9). The large gap in valuations between respondents who see victims of their own versus the opposite sex clearly stems

from the subsamples of respondents with the highest familiarity and for whom hypothetical bias should be the smallest.[24]

The second potential concern is social desirability bias. Sexual harassment is a socially sensitive issue that might motivate respondents to give socially desirable answers in a survey situation. Although we cannot rule out this type of bias or quantify its size, we can note several patterns in the data that contradict this type of respondent behavior. Close to half of our respondents choose the job with a sexual harassment vignette when making job choices in which one job has a vignette and the other does not. Our results also show a similar-size WTP for schedule flexibility as previous survey experiments without sexual harassment, and, in our case, the flexibility estimate is highly similar in subsamples of data from job choice tables that include harassment vignettes or not. Moreover, while social convention might push men to protect women, we find a much larger WTP for men who react to a male victim in the vignette. Finally, the estimates for respondents who see a victim of the opposite sex might be particularly informative. This group should, in theory, be just as affected by social desirability bias as those who see a same-sex victim. However, their WTP estimates are less than a quarter as small in both the subsample of respondents who are aware of harassment cases in their industry and among those who claim to place a very large priority on sexual harassment in their own job searches (Online Appendix Figure A9).

Further support for the reliability of our results comes from coherent variation in the WTP estimates across various factors.

24. We observe large point estimates of ∼35% among occupation gender minorities and people who are aware of harassment cases. These are larger than our main estimates, although small sample sizes and large confidence intervals (15%–50%) make it difficult to say by how much. Several factors might contribute to larger valuations. Both groups perceive the vignettes as signaling slightly larger increments of harassment relative to the reference category compared to their counterparts (Online Appendix Table A9). The aware subsample, but not the occupation minorities, perceives higher levels of personal risk than their counterparts, and Online Appendix Table A9 shows that they also perceive the vignettes as more harmful to the victim (0.22 standard deviations) and are more likely to label the portrayed behaviors as "sexual harassment" (0.24 standard deviations). The follow-up survey suggests that respondents' own experiences of victimization may be an additional explanation. We asked people who responded affirmatively to the question about awareness of harassment cases a follow-up question: "Who was the target of the harassment cases that you thought of in the previous question?" Nearly one-fifth of the aware respondents said they thought of cases involving both themselves and others, and 3% thought of case(s) involving only themselves.

People who are gender minorities in their occupation have a larger WTP than those who are gender majorities (Online Appendix Figure A9). People who see themselves as being at higher risk of harassment when entering a workplace where harassment has occurred have a larger WTP than those who perceive less risk (Figure IV). When we ask respondents how they prioritize the three work conditions and wages when they look for jobs, we find larger WTPs for each condition among people who prioritize it more, including sexual harassment (Online Appendix Figure A10).

IV.G. Other Sensitivity Analysis

Online Appendix A3 displays sizable gaps in valuations between respondents who see a victim of their own versus the opposite sex for all three sexual harassment vignettes (Online Appendix Figure A16). Online Appendix Figure A17 shows that differential valuations of the wage trait are not responsible for the subsample results. Online Appendix Figure A18 illustrates that the WTP gap between respondents who see a victim of their own versus the opposite sex is robust to removing the sample weights, restricting the sample to people who show attentiveness in their survey responses,[25] and changing the estimation method to logit, probit, or mixed logit.

V. Sexual Harassment, Wages, And Workplace Transitions

We return to the survey data linked with employer–employee data to analyze how harassment risks and wages vary with the gender composition of the workplace. We then conduct an extended analysis of sexual harassment and workplace transitions. The aim is to study if harassment self-reports are associated with a greater likelihood of leaving the workplace and if these transitions contribute to expanded gender inequality in the labor market.

V.A. Sexual Harassment and Wages

Our wage data come from a mandatory employer survey collected by Statistics Sweden. We calculate the workplace-specific

25. We define attentive and inattentive respondents based on data from strictly dominant job choices, that is, where one job in the job choice table had equal or better values than the other job on all traits. People who correctly (incorrectly) chose the dominant job are defined as attentive (inattentive).

FIGURE V

Sexual Harassment Risks and Workplace-Specific Wage Premiums

The figure shows binned averages of the female–male gap in sexual harassment incidence (circles) and workplace-specific wage premiums (squares) across 5 percentage point increments of the workplace share of men. The data are five pooled cross sections of the Work Environment Survey ($N = 37{,}457$). Sexual harassment is a dummy variable for self-reported harassment from colleagues or managers in the past 12 months, and the left y-axis reports the difference in the average of this variable between women and men. The workplace-specific wage premiums are workplace fixed effects from an estimated AKM model (following Abowd, Kramarz, and Margolis 1999, details in Online Appendix A4). A workplace is a unique combination of plant and organizational ID codes, and we calculate the workplace share of men in population-wide register data.

wage premiums paid by respondents' workplaces in data for the full labor market and using the empirical framework developed by Abowd, Kramarz, and Margolis (1999). This framework estimates workplace fixed effects in a wage regression that also includes individual fixed effects (see Online Appendix A4 for details). The workplace fixed effects (i.e., the wage premiums) capture how much a workplace pays in wages compared to other workplaces with the same occupation structure and workers' socioeconomic traits. We obtain this measure for 70% of the survey respondents' workplaces.

We split the survey sample into 5 percentage point increments of the workplace share of men and plot the average workplace-specific wage premiums in each bin in Figure V (gray squares). The figure also displays the female–male harassment gap in each bin (hollow circles), measured as the proportion of women who self-report sexual harassment in the survey minus the proportion of men who do so.

Workplaces with a larger share of men clearly offer higher wages. Male-dominated workplaces (> 80% men) have an average

workplace-specific wage premium that is 9% (0.09 log points) higher than female-dominated workplaces (< 20% men). These male-dominated workplaces are also where women have the highest harassment risk and men the lowest. Looking at the gender gap in harassment rates, women's rate is 17 percentage points higher in workplaces with more than 80% men. Meanwhile, men's risk is higher than women's by 4 percentage points in female-dominated workplaces with < 20% men.

Online Appendix Figure A11 confirms that the relationship in Figure V looks the same for the raw, average wage of the workplace. Additional sensitivity tests replicate the results for workplace-specific male and female wage premiums (Online Appendix Figure A12) and for split samples of workplaces with a wage premium above or below the full-sample average (Online Appendix Figure A13). Another way of structuring the analysis of sexual harassment and wages considers individual wages rather than workplace-specific wage premiums. The relationships are very similar: women with higher wages (and men with lower wages) are more likely to self-report harassment. An extended analysis in Online Appendix A5 discusses potential reasons for these patterns. A series of wage regressions highlight the importance of occupations while showing less evidence for compensating pay as an explanation (applying recent methodological advances by Bell 2020).

V.B. Sexual Harassment and Workplace Transitions

Determining whether sexual harassment is associated with transitioning from one's workplace requires a method that sidesteps a main data limitation in the Work Environment Survey, which is that we only have a single survey response per person rather than variation in the "treatment" of self-reported sexual harassment over time for the same individual. This fact precludes a difference-in-differences design, which would require within-individual variation in the treatment across years.

By linking each respondent to the administrative data, we can observe where they worked in each year before and after taking the survey. This allows an event study that compares workplace transition probabilities between people who self-reported harassment in the survey and those who did not. This analysis is structured as follows. We set a time window around the survey year and let it range from three years before the survey to five

years after. We then define a *transition dummy variable* for each of these years. Each transition dummy takes a value of 0 if the respondent is employed in the same workplace in that year as when they took the survey, and 1 if they are not. We denote the transition dummy variables T_i^w, where w denotes the yearly time points ranging from -3 to $+5$. The transition dummies measured prior to the survey year capture the cumulative probability of having transferred into the workplace, while the postsurvey dummies capture the cumulative probability of having transferred out of it.

We clean the transition dummies in four ways. First, because we are mainly interested in moves between firms rather than moves between workplaces in the same firm, we code them as 0 unless the person also changes firms. Second, to clean the data from plant closures, mergers, and so on, we code them as missing in years when the surveyed workplace does not exist in the administrative data. Third, to exclude exits due to retirement, we exclude people older than 60 in the survey year. Finally, because we are interested in the respondent's own choice to transition, we set them to missing if they made a transition to nonemployment rather than to a new workplace.[26]

Our event study runs a separate regression for each of the transition dummy variables T_i^w on the dummy variable for self-reporting sexual harassment in the survey.[27] We use the following specification:

$$T_i^w = \alpha + \beta_1 SH_i + \gamma_i + \boldsymbol{\beta}_{\boldsymbol{x}} \boldsymbol{X}_i' + e_i, \tag{6}$$

where our coefficient of interest is β_1, the coefficient on the dummy variable for self-reporting sexual harassment, SH_i. It captures the difference in the average probability of having made a workplace transition for people who self-reported harassment and those who did not. Figure VI reports this coefficient for each regression, that is, for each transition dummy. We always include survey year

26. Following Hotz, Johansson, and Karimi (2018), we define nonemployment as having an annual labor income below the threshold of 0.5 Swedish price base amounts (corresponding to approximately US$2,500). The price base amount is a quantity used in the Swedish social insurance and tax system. It is changed annually to account for inflation. Only 12% of men's transitions and 18% of women's are to nonemployment. Neither men nor women exhibit an increase in such transitions after reporting sexual harassment.

27. Separate regressions for each time point aligns with Jordá's (2005) suggested method for estimating impulse responses.

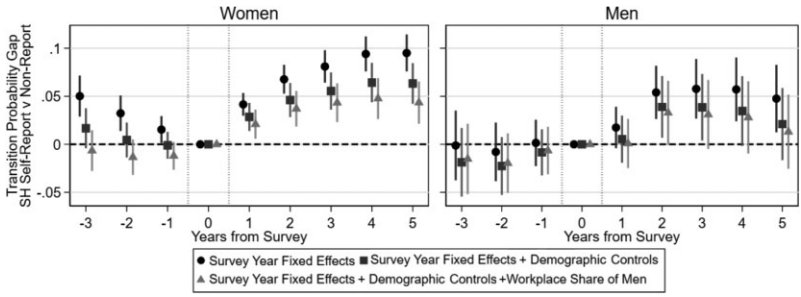

FIGURE VI

Event Study of Workplace Transitions

The figure shows estimated differences in the proportion of employer-to-employer transitions out of the workplace of the Work Environment Survey between people who self-report sexual harassment in that survey or not. The x-axis denotes the number of years passed since the survey. Five pooled cross sections of survey data are matched to annual labor market data from the LISA administrative data via mandatory personal ID codes. Demographic controls from administrative records are four dummies for marital and parental status, four dummies for age categories, two dummies for having secondary or tertiary education, and two dummies for being born in a different European country or outside of Europe. The workplace share of men is calculated from administrative records.

fixed effects, y_i. As a sensitivity check, we run the regression with and without an additional vector of control variables, $\boldsymbol{X}_i'$, which includes demographic traits—age, education, region of birth, marital and parental status—and the workplace share of men.

We observe positive and statistically significant estimates on the dummy for self-reporting sexual harassment in the years following the survey for both men and women. People who self-report sexual harassment are more likely than others to transition to a new workplace. The estimates for women are slightly larger than for men and persist more strongly over time. When including the full set of controls, women who self-report harassment are 4 percentage points more likely to have transitioned to a new workplace within three years of taking the survey. This translates to a 25% increase against the average transition probability of 18% for women in the survey sample.[28] For men, the estimated increase is 3 percentage points and translates to a 15% increase relative to a baseline of 21%.

28. This estimate is smaller than McLaughlin, Uggen, and Blackstone's (2017) finding that young U.S. women who self-report sexual harassment are 50% more likely to have a new job after two years.

Examining how the estimates change when including controls, we see that holding demographic traits constant results in smaller estimates. The age control does most of this work. Young people self-report more harassment and are more likely to change jobs. This control is also the main reason that pretrends in the estimates become less noticeable. Younger people who self-report more harassment are more likely to have recently transitioned into the workplace. The estimates shrink further, but do not go to zero, when we add a control for the workplace share of men. Workplace gender minorities are somewhat more likely to change jobs, but this only accounts for a small part of the relationship between workplace transitions and self-reported sexual harassment. The Online Appendix shows that the estimates for the three-year posttransition dummy also remain significant after adding an even more extensive list of controls (Online Appendix Figure A14).

A quick analysis of respondents' self-reported desire to leave their job for health reasons, also shown in Online Appendix Figure A14, tells us something about mechanisms. People who self-report harassment are 20 percentage points more likely to self-report such intentions in the past 12 months in the Work Environment Survey (a 100% increase compared to a baseline of 19%, and very similar for men and women).[29] This suggests that negative health shocks are a possible mechanism behind our transition results. It also suggests that our transition results reflect a lower benchmark for how sexual harassment affects people's desire to leave their workplace, since realized transitions require both a desire to leave one's job and a new job to go to.

Next we compare workplace transitions for people who self-reported harassment and those who did not. The purpose is to study if transitions after harassment are more likely than other types of transitions to contribute to a greater degree of workplace sex segregation or to a larger gender wage gap. We restrict the data to survey respondents who transitioned out of the workplace where they were employed when they took the survey within three years. We then define four dummy variables for transitions that expand gender inequality—two for women and two for men. For sex segregation, the two dummy variables take a value of 1 if the transition leads to an increase in the share

29. The correlation in our data between the dummy variables for harassment and leave considerations is 0.16 for women and 0.12 for men, which is similar to Willness, Steel, and Lee's (2007) correlation of 0.13 in pooled data from 16 studies.

of same-sex coworkers compared to the surveyed workplace, and 0 otherwise. For the gender wage gap, the women's dummy takes a value of 1 if the transition took her to a workplace with a lower wage premium than the one she left, and vice versa for men. We run four regressions of the form:

$$T_i^3 = a + \beta_1 SH_i + \beta_2 x_i + e_i, \tag{7}$$

where the outcome, T_i^3, is each of the four dummies. As before, we are interested in the coefficient on the dummy variable for self-reporting sexual harassment in the past 12 months, SH_i. Positive estimates of this coefficient indicate that workplace transitions after sexual harassment are more likely than other transitions to expand gender inequality in the labor market. Such a pattern might have two main explanations. Inequality could increase because transitions after harassment are more likely to push workers to leave certain types of workplaces, that is, where they are workplace gender minorities or where wages are higher (for women) or lower (for men). It could also increase because transitions after sexual harassment are different from transitions without harassment, even for people who start at the same point(s) in these distribution(s). We test these two pathways by running two versions of equation (7). One includes a control, x_i, for the initial share of men or, separately, the workplace-specific wage premium, and the other does not.

Table III shows the results from estimating equation (7). Column (1) shows that for women, self-reporting sexual harassment is associated with a 7.7 percentage point higher probability of transitioning to a workplace with a lower share of men, and an 8.4 percentage point higher probability of moving to a workplace with a lower wage premium. Column (2) disentangles the pathways for this result by holding constant the conditions in the surveyed workplace. When doing so, both estimates drop in size and lose statistical significance at the 5% level. This suggests that sexual harassment is associated with transitions that expand gender inequality because women who report harassment transition out of systematically different types of workplaces. By contrast, we find no evidence that women who self-report harassment make different types of transitions conditional on these initial conditions.

The results for men are less statistically precise and may seem counterintuitive. Men who change workplaces after self-reporting sexual harassment are, if anything, less likely to

TABLE III

SEXUAL HARASSMENT AND TYPES OF WORKPLACE TRANSITIONS

Sample: Respondents who transition out of the surveyed workplace within three years.	Women		Men	
	(1)	(2)	(3)	(4)
Dependent variable: Binary indicator for transitions that increase the proportion of same-sex colleagues.				
Self-reported sexual harassment	0.077***	0.001	−0.035	−0.076*
	(0.024)	(0.024)	(0.043)	(0.041)
Initial share of men in the workplace		0.626***		−0.457***
		(0.034)		(0.034)
Observations	2,990	2,958	3,012	2,997
Dependent variable: Binary indicator for transitions that increases the gender wage gap (a decrease in the workplace-specific wage premium for women, and an increase for men).				
Self-reported sexual harassment	0.084**	0.054	−0.113*	−0.117*
	(0.035)	(0.033)	(0.067)	(0.063)
Initial wage premium		1.749***		−1.663***
		(0.131)		(0.153)
Observations	1,461	1,460	1,235	1,234

Notes. The table compares types of workplace transitions between people who self-report sexual harassment in the Swedish Work Environment Survey and people who do not. The data are five pooled cross sections of the survey (1999, 2001, 2003, 2005, and 2007), linked with annual administrative data. The sample is restricted to respondents who transitioned out of the surveyed workplace within three years of taking the survey. Dependent variables are created by comparing the proportion of same-sex colleagues and, separately, the workplace-specific wage premium, in the surveyed and destination workplaces for the individual's transition. Standard errors are in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

transition in ways that expand gender inequality. One possible explanation for this result might be that men who have selected into a low-paying, female-dominated workplaces have relatively strong personal tastes for remaining in a similar workplace. Another explanation that goes outside our simple theoretical model might consist of more restricted labor market options among these men. Switching from a higher- to a lower-paid job is likely easier for female harassment victims than switching from a low- to a high-paid job would be for male ones.

The transitions into lower-paying workplaces for men and women who report sexual harassment corroborates a key result from the hypothetical job choice experiment, namely, that people are willing to take pay cuts to avoid a workplace with sexual harassment. Because we cannot observe the workplace options for people who choose not to make a transition, we cannot calculate the exact pay cut they are willing to take to avoid a workplace where they experience sexual harassment (see Wiswall and Zafar 2018 for a discussion of this limitation with observational data).

Overall, the results on workplace transitions suggest that transitions after sexual harassment reinforce gender inequality in the labor market. Women's transitions lead to more sex segregation between workplaces. And although both men and women's transitions take them to lower-paying workplaces, an aggregate interpretation needs to take account of women's greater likelihood of harassment. Women are three times as likely as men to self-report sexual harassment in our data. This means that across the entire labor market, sexual harassment expands the gender wage gap by producing three times as many negative wage transitions for women as it does for men.

VI. Importance For Aggregate Gender Inequality

To calculate how sexual harassment affects the gender wage gap, we compare how it affects women's relative incentive to work in higher-paying workplaces. This exercise follows Sorkin (2017) and Maestas et al. (2018) by calculating how work conditions alter the utility from wages.

In our utility function in equation (2)[30], workers derive utility from the workplace-specific wage and from sexual harassment. We calculate the sizes of these two components of the utility

30. $U_i = w_j - \pi^g d_j^g - \pi^{-g} d_j^{-g} + \rho^g \alpha_j + \epsilon_{ij}$.

TABLE IV

WORKPLACE-SPECIFIC WAGE PREMIA AND SEXUAL HARASSMENT IN WAGE EQUIVALENTS

	Average man's workplace (1)	Average woman's workplace (2)	Difference (1) − (2)
Workplace-specific wage premium	0.024	− 0.023	0.047
Women's utility from sexual harassment	− 0.038	− 0.026	− 0.012
Men's utility from sexual harassment	− 0.020	− 0.021	0.001
Women's utility from wages and harassment	− 0.014	− 0.049	0.035
Men's utility from wages and harassment	0.044	− 0.004	0.048

Notes. The table shows average wage premiums and wage equivalents of sexual harassment for the average woman's and man's workplace. All values are in log points. The data are the Swedish Work Environment Survey ($N = 26{,}116$). The workplace-specific wage premiums are workplace fixed effects from an estimated AKM model in data for the full national labor market (following Abowd, Kramarz, and Margolis 1999, details in Online Appendix W4). The utility from sexual harassment is calculated by multiplying the sex-specific harassment risks from Figure II with the WTPs against own- and opposite-sex harassment risks in Figure IV.

function for respondents in the Work Environment Survey and present the calculated statistics in Table IV. The top row shows a male-female gap in average workplace-specific wage premia of 0.047 log points. Under our assumption that women and men earn the same workplace wage, we interpret this gap as a 4.7% wage gain from working in the typical man's compared with the typical woman's workplace.[31]

We calculate the (dis)utility from sexual harassment in wage equivalents by multiplying risk levels and risk valuations. We approximate risk valuations for own- and opposite-sex harassment, π^{g} and π^{-g}, with the WTP estimates from the survey experiment.[32] We then approximate harassment risks, d_j^{g} and d_j^{-g}, based on the workplace's share of men (recall the relationships in Figure II).[33] Importantly, when a woman moves from the average

31. In Online Appendix Table A7, we relax this assumption by using gender-specific wage premiums. This analysis shows a similar importance of sexual harassment, while differential wage premia for men and women provide an additional incentive for workplace sorting.

32. The estimates in Figure IV provide the wage equivalent of taking a job in a workplace with a known harassment incident against a same-sex or opposite-sex victim. We assume that the probability of finding out about an incident against a specific sex is equal to the gender-specific harassment risk.

33. We calculate the risk for each 1 percentage point interval of the share of men. For example, in workplaces with 10%–11% men we assign a female risk of 5.2% and a male risk of 25.7%, and for a workplace with 90%–91% men we assign a female risk of 22.4% and a male risk of 2.0%.

woman's to the average man's workplace, the harassment risk for her own gender increases and that for her male colleagues decreases. Her net utility reduction from these changes equates a 0.012 log point lower wage, which offsets part of her wage gain from 0.047 to 0.035 log points. Looking at men, sexual harassment increases their utility gain from the same move, albeit by a small margin of 0.01 log points to a total of 0.048 log points. Comparing women to men, sexual harassment makes the move 0.013 log points less profitable for women and offsets 28% of their relative wage gain ($\frac{0.013}{0.047}$ log points). One interpretation of this number is that sexual harassment creates the same disincentive for women's entry into men's higher-paying workplaces as levying a 28% tax on their wage gains from those employment moves.

To translate the "utility tax" into an effect on the gender wage gap, we make the same assumption as in our general-equilibrium analysis in Section II, namely, that women's sorting into lower-paying workplaces stems from gender-differences in the utility from work conditions. Lower-paying workplaces offer women work conditions from which they derive more utility than men do. In equilibrium, this gender gap in utility from work conditions is equal to the gender gap in average wage premia. In turn, and using our calculation above, removing the male-female gap in disutility from sexual harassment would reduce the gender gap in wage premia by the same amount. With a raw gender wage gap of 0.166 log points in our sample, this would remove 8% of that disparity ($\frac{0.013}{0.166}$). We therefore conclude that sexual harassment—via its effect on the sorting of women into lower-paying workplaces—accounts for nearly one-tenth of women's lower wages.

VII. Conclusions

Sexual harassment from colleagues and managers is a barrier to gender equality in the labor market. This mistreatment is the most severe for workplace gender minorities and imposes costs on these minorities relative to their gender majority colleagues. The disincentive created by sexual harassment to become and remain a workplace gender minority reinforces gender segregation across workplaces, as well as the gender pay gap.

It is well known that women suffer more sexual harassment in the workplace than men. This fact likely underpins the relative

scarcity of research on men's experiences. Our analysis of nationally representative Swedish data linked men's victimization to their employment in female-dominated workplaces. Arguably, this pattern might be particularly salient in the Swedish labor market with its large and female-dominated public sector. For other countries, it might be informative about the future trajectory of harassment against men as public sectors grow. The results can also inform policy makers about potential barriers that keep men from taking these "pink collar jobs of the future" (Delfino 2019).

Our survey experiment generated two conclusions. First, sexual harassment at work creates a large welfare cost for the average person. Second, and equally important, some people in the workplace carry a larger cost than others. In most workplaces, one gender has a greater risk of harassment than the other. Our results demonstrate that people who belong to the higher-risk gender perceive twice the welfare costs from the harassment against them as their gender-majority colleagues do. This insight could help explain why sexual harassment and sex segregation in the labor market are such persistent phenomena. In most workplaces, a high-risk gender with a large willingness-to-pay for reducing sexual harassment will coexist in the workforce and recruitment pool with a low-risk and smaller willingness-to-pay gender. These low-risk workers will depress employers' incentives to invest in harassment prevention. Overcoming this collective action problem will be particularly difficult in the most sex-segregated contexts, arguably motivating outside intervention to shift employers' incentives. This insight matters for sexual harassment, but it might also be relevant for other types of mistreatment against workplace minorities based on race, age, or sexual orientation.

Our analysis underscores the broader business case for preventing sexual harassment by showing ways that harassment could be costly for firms. Victims of harassment are much more likely to have lower job satisfaction, have feelings of unease when going to work, and desire to leave their job for health reasons. They are also more likely to leave their employers and thereby incur search costs for new employees. Our survey experiment complements this result by showing that when information about harassment incidents travels outside company walls, it severely damages the reputation of the workplace among prospective applicants. Conversely, preventing harassment is likely to improve workers' well-being, reduce turnover costs, and expand the recruitment pool. Given the large negative value placed on harassment information among prospective workers, preventing

harassment could even be a more cost-effective way of attracting workers than higher wages.

It is difficult to calculate the effect of sexual harassment on gender inequality in the labor market. Our rough calculation attributed about 10% of the raw gender wage gap to this type of discrimination. Future research could improve on this exercise, for example, by accounting for the effects of harassment on workplace transitions and occupation choices or by adding elements such as information flows or expectations.

A relevant avenue for future research could explore how sexual harassment shapes long-term careers. We find that harassment victimization increases women's likelihood to leave male-dominated workplaces. Obviously, these switches might cut careers short and introduce switching costs from losses to firm-specific human capital. For those who stay behind, continued exposure to the high-harassment environment might depress productivity and career investments. Impact studies over individual career trajectories could begin the analysis in school. Sexual harassment in educational environments might help explain women's systematic dropout patterns from male-dominated tertiary education programs, as recently observed by Astorne-Figari and Speer (2019). In this way, workplace sexual harassment might affect careers through switching patterns over time and by shaping incentives to invest in careers within organizations.

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at the *Quarterly Journal of Economics* online.

DATA AVAILABILITY

Data and code replicating the tables and figures in this article can be found in Folke and Rickne (2022) in the Harvard Dataverse, https://doi.org/10.7910/DVN/OHGOEV.

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