

SEXUAL HARASSMENT AND GENDER INEQUALITY IN THE LABOR MARKET*

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Abstract

This paper offers a comprehensive empirical analysis of sexual harassment in the Swedish labor market. First, we use nationally representative survey data linked with employer-employee data to describe rates of self-reported sexual harassment across occupations and workplaces. The risk of sexual harassment is clearly imbalanced across the sex segregated labor market. In gender-mixed and male-dominated occupations and workplaces, women have a higher risk than men, and men have a higher risk than women in female-dominated contexts. We use a hypothetical job-choice experiment with vignettes for sexual harassment to measure the disutility of sexual harassment risks. Both men and women have an equally high willingness to pay for avoiding workplaces where sexual harassment has occurred. But the willingness to pay is conditional on the sex of the fictional harassment victim. People reject workplaces where the victim is the same sex as themselves, but not where the victim is of the opposite sex. We return to the administrative data to study employer compensation for the disutility of sexual harassment risks. Within workplaces, a high risk is associated with lower, not higher wages. People who self-report sexual harassment also have higher job dissatisfaction, more quit intentions, and more actual quits. Both these patterns indicate a lack of full compensation. We conclude that sexual harassment should be conceptualized as gender discrimination in workplace amenities, and that this discrimination reinforces sex segregation and pay-inequalities in the labor market. *JEL* Codes: J16, J24, J81.

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1. Introduction

In most labor markets, women work in lower-paid occupations and workplaces than men (Blau and Kahn 2017, Card et al. 2015, Morchio and Moser 2020). In this paper, we study sex segregation and wages from the perspective of gender discrimination in work environments. We provide a comprehensive empirical study of sexual harassment, a characteristic of the work environment defined as gender discrimination in civil law. The problem of sexual harassment is both severe and highly prevalent. About half of all women become targets at some point in their work life, and a large literature in sociology, psychology and medicine has documented wide-ranging negative consequences on physical and mental health (reviewed by, e.g., Welsh 1999; McDonald 2012; Fitzgerald and Cortina 2018).¹

This paper has three analytical components. We describe the variation in self-reported sexual harassment across occupations and workplaces, measure how workers value the risk of harassment, and test if employers compensate that risk. We start by describing how the incidence of harassment from colleagues and managers co-varies with sex ratios in occupations and workplaces, and for both men and women. Building on these incidence patterns, we then design a survey experiment that measures the disutility of harassment risks. To study compensation, we compare wages, job dissatisfaction and job separations by levels of predicted and actual sexual harassment. Based on the results from the three empirical sections, we argue that sexual harassment amounts to gender discrimination in workplace amenities. Within most workplaces in the sex-segregated labor market, the harassment risk is systematically higher for the gender minority. This risk of harassment is associated with a substantial disutility, which is not offset by monetary or non-monetary

¹ Reviews of hundreds of empirical studies show consistent evidence of psychological, health, and job-related harms from sexual harassment (see, e.g., reviews by Welsh 1999, McDonald, 2012, and Fitzgerald and Cortina 2018). Mental and physical health consequences range from irritation and anxiety to anger, powerlessness, humiliation, increased risk of alcohol abuse and eating disorders, depression and post-traumatic stress disorder. Job-related factors consistently linked with sexual harassment include absenteeism, lower job satisfaction, commitment and productivity, damaged inter-personal work relationships, decreased perceptions of equal opportunity, and employment withdrawal. For a notable recent example in this literature, see Leskinen et al. (2011).

compensation from the employer. Just like wage discrimination, we can thus construe sexual harassment a tax on the labor supply of the discriminated sex.

Our description of self-reported sexual harassment leverages unique Swedish data that links a nationally representative survey (N=50 000) to administrative data for the whole, employed population. Following the European Commission, we define sexual harassment as “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. One question asks about unwanted conduct of a sexual nature, and the other about conduct based on sex that affects the dignity of the respondent. Both questions ask about behaviors that correspond to legal definitions of discrimination and are prohibited by law.²

Questions of measurement quality are central to any study of sexual harassment. Our survey data, which is the official data used by the Swedish government to track work conditions in the labor market, has more than 100 questions. The two questions about sexual harassment do not stand out in the mix, a low salience that is likely to limit social desirability and demand bias. Swedish employers are also unaware that their workers are sampled for the survey, which precludes any misreporting from intimidation by the employer. Two disadvantages with the survey questions are that they include only a limited number of examples of sexual harassment behaviors, and that the first question explicitly mentions the words “sexual harassment”. Both these things are likely to bias self-reports downward. This bias makes the data less than perfect for pinpointing exact incidence levels, but it remains useful for comparing levels across occupations and workplaces with different sex

² Our broad definition of sexual harassment includes unwanted sexual attention, sexual hostility, sexist hostility, and sexual coercion (Section 2 discusses these concepts in more depth). In Sweden, these behaviors are defined as discrimination in the 1991 Equal Opportunities Act and the 2008 Discrimination Act. Sexual harassment and/or gender harassment are prohibited by the EU’s 2002 anti-discrimination directive, which also defined these behaviors as sex-discrimination (Directive 2002/73/EC). This directive defines sexual harassment as a workplace issue and from the victim’s perspective of “unwanted behavior”. It also separately defines gender harassment as “where an unwanted conduct related to the sex of a person occurs with the purpose or effect of violating the dignity of a person, and of creating an intimidating, hostile, degrading, humiliating or offensive environment” (for an extended discussion, see Zippel 2019). 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 et al. 2011).

compositions. We dedicate considerable effort to discussing misreporting that might be correlated with those sex compositions. Most importantly, we find no evidence that gender-minorities self-report systematically higher rates of other forms of workplace mistreatment from colleagues and managers. We also show that the results are robust to holding constant traits that might correlate with recall or sexual harassment awareness, such as age, education level, and hierarchical position in the workplace.

The description of incidence rates shows that women self-report substantially more harassment than men in occupations and workplaces that are either gender-mixed or male-dominated. Meanwhile, men self-report substantially more harassment than women in workplaces or occupations that are strongly female-dominated. This description of harassment risks corroborates and extends previous research from other academic disciplines. Nationally representative datasets have been rare in this literature, which has mainly relied on data from single industries, a limited number of firms, or small and geographically un-representative samples (see, e.g., Gutek and Cohen 1987; Gutek et al. 1990; Hulin et al. 1996; Gruber 1998; Kabat-Farr and Cortina 2014).

In the second part of the empirical analysis we design a conjoint survey experiment to quantify people's willingness to accept a lower salary in exchange for a lower risk of sexual harassment.³ Approximately 4,000 employed Swedish citizens completed the survey experiment in the fall of 2019. The format was one of fictional job choices, following recent work by Eriksson and Kris 2014; Mas and Pallais 2017; Wiswall and Zafar 2017; Maestas et al. 2018; He et al. 2019. We randomly assign values of the wage, skill development, schedule flexibility, and working environments to each job. When describing the work environments, we inserted vignettes about incidents of sexual harassment in the work unit. An important aspect of this exercise was to assign the sex of the fictional victim and sex of the fictional perpetrator according to the actual patterns in the labor market. This had two purposes: to make scenarios more realistic, and to allow a comparison of respondents for whom the vignette signaled a high harassment risk (because the victim was the same sex as themselves) or a lower risk (because the victim was the opposite sex). To assign respondents the

³ Pre-registered at egap.org. ID 20190927AA, <https://egap.org/registration/6139>.

most realistic sex composition of victim and perpetrator in their current labor market context, we asked a question about the sex ratio of their current workplace before the experiment.

The experiment provides two important findings about the disutility of sexual harassment risks. Respondents had a large disutility from taking a job where sexual harassment has occurred. For both men and women, the average magnitude of this disutility was equivalent to a 10% salary cut. Both men and women hence had an equally large preference for a workplace free from sexual harassment. A second important finding is that the disutility was conditional on the sex of the harassment victim. When the victim was the same sex as the survey respondent, the average disutility was equivalent to a 17% salary cut, but it was just 6% for opposite-sex victims. These findings are important for understanding the structure of disutilities from sexual harassment across the sex segregated labor market. In many workplaces, a gender-minority with a high risk and a high disutility will coexist with a gender-majority that has a low risk and low disutility. We build on this result to discuss employer incentives for preventing sexual harassment or to offer compensating pay.

In the third empirical section we study compensation for harassment risks. We find that within workplaces, a higher risk of sexual harassment is not associated with a higher wage, also after conditioning on proxies for contributions to workplace output. At the individual level, sexual harassment predicts job dissatisfaction, quit intentions, and actual job separations. These relationships remain large even after controlling for socio-economic variables, the sex composition of the workplace, and self-reports of other forms of interpersonal mistreatment. Following Rosen (1986), we argue that job separation is a useful summary measurement of monetary and non-monetary rewards from a job. The results are therefore indicative that the disamenity of sexual harassment is not (fully) compensated in wages and/or other workplace amenities.

One reason for the lack of full compensation for sexual harassment may be the bystander attitudes uncovered in the survey experiment. These bystanders are largely indifferent to sexual harassment in the workplace because people of their own sex are not the main targets. Their presence in workplaces and recruitment pools will therefore likely depress the demand for prevention, especially in the most sex-imbalanced contexts of the labor market. In addition to bystander attitudes, a market

for the amenity of safety from sexual harassment is also unlikely to form due to information frictions. Most people do not report their experiences of sexual harassment to their employer (Cortina et al. 2008). Moreover, even if information would be perfect, explicit negotiations for contracts that exchange sexual harassment risks against wages would be illegal in most countries and morally reproachable to most people (Basu 2003).

A main contribution of this paper is to further our understanding of gender inequality in the labor market. Studies on sexual harassment are rare in economics and have used less-than-perfect data.⁴ In contrast, a rapidly growing literature studies how other work conditions shape gender inequality, most importantly time/space flexibility or commuting distance (recently studied by, e.g., Goldin 2014b; Flory et al. 2014; Wiswall and Zafar 2017; Mas and Pallais 2017; Le Barbachon et al. 2019). The theoretical mechanism in these papers is that women accept lower relative wages to satisfy their preference for the amenity. Our paper highlights a stark difference between this dynamic and the gender inequality produced by the work condition of sexual harassment. Men and women have similar preferences for personal safety from sexual harassment, but the risk of harassment is higher for gender minorities and is not economically compensated by the employer. It follows that across the sex segregated labor market, gender minorities have lower total returns from work than gender majorities. Sexual harassment imposes an extra cost on these minorities, which will disincentivize women from taking male-dominated jobs with higher wages, and men from taking female-dominated jobs with lower wages.

Our results also have implications for the empirical research on compensating pay-differentials. In this literature, sex differences in patterns of workplace transitions are often interpreted as sex differences in amenity preferences (e.g. Sorkin 2017, Lamadon et al. 2019, Morchio and Moser 2020). Our results show that sex differences in transitions could also be attributed to sex

⁴ Economists have provided theory on compensating pay-differentials for sexual harassment (Basu 2003, Bac 2018) and studied such pay-differentials with industry-level data (Hersch 2018). They have also documented correlations between sexual harassment, job dissatisfaction, and quit intentions (Laband and Lentz 1998, Antecol and Cobb-Clark 2006, Antecol et al. 2009, Cottini et al. 2011). In more recent work, Lordan and Pischke (2016) discuss sexual harassment as a potential explanation for women's lower job satisfaction in male-dominated labor market contexts, and Cheng and Hsiaw (2019) provide a theoretical model for reporting incentives.

discrimination in amenities. An additional contribution to this literature is that we highlight the potential role of lack empathy or altruism across social in- and out-groups in preventing the emergence of compensating differentials for workplace amenities. In this regard, sexual harassment may hold similarities with other social environment disamenities like racism, ageism or hostility toward minorities by sexual orientation. When social minorities are targeted with hostile behavior, the materialization of compensating pay-differentials may hinge not only on the degree of suffering among the minority, but also on the degree of indifference among the bystander majority.

Policy recommendations based on our results might also focus on bystander attitudes. Providing safety from sexual harassment requires the investment of significant resources by the employer, including to study the problem, train the workforce, and implement efficient grievance procedures. The failure of low-risk workers to internalize the costs of harassment to high-risk workers will depress employer incentives to make these investments. Policies that affect bystander attitudes, like civility training, bystander training, or high-quality grievance procedures, might therefore be useful to shift the incentives of employers.

Finally, the fact that our empirical data comes from Sweden—a global leader in gender equality—might raise questions about external validity. Perhaps surprisingly, Sweden does not stand out from other European countries in terms of sensitization to sexual harassment or sex segregation of the labor market. The limited cross-country evidence available on sensitization places Swedes in the middle among European countries (The Local, 2017-11-10). The same is true for sex segregation, where Sweden ranks 14th out of 27 EU countries (European Commissions' Expert Group on Gender and Employment 2009). A more relevant difference between Sweden and other countries is the large public sector with its heavily female-dominated jobs. Our results for sexual harassment of men in these jobs may not extend to the labor markets of countries with smaller public sectors.

2. The Argument

Sex-segregation and sexual harassment. Sex-segregation of the labor market is an important phenomenon in any country. In the United States, the median woman works in an occupation with 30% men and the median man in an occupation with 71% men (Gelblum 2019). Numbers are similar

in Sweden, with the median woman has an occupation with 24% men and the median men an occupation with 65% men. There is also considerable segregation between workplaces, even conditional on occupation. Among Swedish workers with the same 3-digit occupation code, there is a 13-percentage point difference in the share of men in the workplace of the average man and the average woman.

A theoretical academic literature from several disciplines predicts that sex segregation in labor market is of fundamental importance for the phenomenon of sexual harassment. Specifically, the more imbalanced an occupation or workplace, the higher the expected harassment rate for the minority gender. Two main theoretical mechanisms underpin this prediction.⁵

Early research established a “contact hypothesis” that related sexual harassment to interactions at work with the opposite sex (Gutek et al. 1990). The hypothesis was founded on the basic insight that a “latent proclivity” to sexually harass another person was largely unrelated to socioeconomic factors like education level, age, or social class (e.g. Pryor 1987; Pryor et al. 1993; Pina, Gannon and Saunders 2009). Given that women are mainly harassed by men, and men by women (reviewed by Cortina and Berdahl 2008), it follows that more contact with the opposite sex during a person’s workday raises the likelihood of encountering a person with the latent proclivity to harass.

Empirically, contact with the opposite sex has mainly been approximated by the sex ratios of occupations and workplaces, which are also the focus of this paper. The sex of a person’s closest supervisor has also been used (e.g. Fitzgerald et al. 1997), blending the contact hypothesis with power-theories of sexual harassment (e.g., MacKinnon 1979). In a supplementary analysis we show that having an opposite-sex supervisor does not account for the correlations we find between sexual harassment and sex ratios of occupations and workplaces.⁶

⁵ We follow the consensus of disregarding the early, now discredited, “natural biological model” that explains sexual harassment as a function of men’s sexual desire for women (e.g. Hoel and Vartia 2018).

⁶ This analysis, together with an analysis of the survey respondent’s own hierarchical position, shows that vertical gender segregation does not confound our empirical findings on sex ratios. Power theories are at the core of the theoretical literature about sexual harassment in the workplace, following the path breaking work of MacKinnon (1979). We acknowledge the importance of these theories but do not discuss them further here since gendered power-relations does not cause omitted variable bias in our analysis.

A second theoretical mechanism is based on gender norms (formulated in economics by Akerlof and Kranton 2000, and in other fields by, e.g. West and Zimmerman 1987; Fitzgerald et al. 1997; Berdahl 2007). In the economic formulation, gender norms give behavioral prescriptions for occupations, which are notable in the form of automatic associations between occupations and gender. For most people, imagining a nurse in their head recalls an image of a women, and a carpenter a man. A person derives utility if they comply with these norms in their occupational choice. But when a person breaks the norm by choosing a counter-stereotypical occupation, they impose a utility cost on both themselves and on opposite-sex people in their surroundings. For these colleagues, sexual harassment can be an instrument to punish the norm-breaker and recuperate some of their lost utility by reestablishing their own sense of self.

Some research has argued that men's experiences of sexual harassment are differently related to gender norms than women's experiences are. While women are nearly always harassed by men, a non-negligible share men's harassment comes from other men (Waldo et al. 1998, McLaughlin et al. 2012). Exposure to male-on-male harassment is expected mainly in hypermasculine jobs and when "effeminate" men fail to fully comply with the hypermasculine gender-role (Franke 1997, Waldo et al. 1998).

Disutility from harassment risks. Conceptualizing sexual harassment as a bad work condition should be uncontroversial. As described above, an extensive empirical literature documents the many ways in which harassment is detrimental to mental and physical health (summarized in footnote 1). Health impacts 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 et al. 1997; Sojo et al. 2016). For example, Leskinen et al. (2011) find significant decrements in psychological health for women faced with "just" gender harassment.

In the main economics theory on work conditions, a work condition that incurs a risk of injury for the worker is called a *workplace disamenity* (following Rosen 1986).⁷ Aspects of the social

⁷ Brown 1980 reviews early research on workplace disamenities in the form of injury risk. More recent examples include Hamermesh 1999; Hersch 1998; Lavetti 2020; and Schmutte and Lavetti 2019.

environment in a workplace are commonly included in lists of relevant disamenities/ amenities in the labor market (Lamadon et al. 2019). It is interesting to consider the structure of the disamenity across the sex segregated labor market, following the prediction above that sex ratios of occupations and workplaces produce harassment risks for the minority gender. The disamenity of sexual harassment is likely to differ between women and men within a workplace. Also, this disamenity difference will not be determined by a conscious choice by the employer but will mostly arise organically as a consequence of contact and gender norms.

Within a workplace, we use the terminology of *potential victims* to denote the gender with a high risk of sexual harassment, and *bystanders* to denote the low-risk gender. Previous research has argued that bystanders with knowledge of sexual harassment incidents suffer substantially from stress in the form of feelings of team conflict and tensions in the workplace (reviewed by, e.g., Fitzgerald and Cortina 2018). While this is true, we argue that these discomforts will be smaller than those of the potential victims, especially when we consider bystanders to also include uninformed low-risk people. These low-risk individuals might also themselves be taking part in the harassment as perpetrators, or gain comradery from the work environment from which the harassment springs.

A more complex theoretical understanding could extend the argument to the potential role of social identities and in-group/out-group relations in producing differential disutilities from the disamenity of sexual harassment within a workplace. Gender is commonly recognized as a main fault line for social identities and an important basis for within-group loyalty and out-group hostility (Tajfel 1974). Across the sex segregated labor market, the potential victims and bystanders of sexual harassment will systematically be of opposite genders. Out-group hostility or lack of out-group altruism, could therefore further reduce bystanders' internalization of the disutility for the (potential) victim. It is also noteworthy that situations of sexual harassment are often ambiguous to the outside observer. Word usually stands against word, and even the best grievance procedures may fail to convince same-sex observers about the harm to the victim.

Sexual harassment as gender-discrimination in work conditions. As discussed above, legal frameworks define sexual harassment as gender discrimination. In this section, we formulate a

broader economic understanding of sexual harassment as discrimination in work conditions. Economists usually define discrimination as a situation where “members of a minority group (women, Blacks, Muslims, immigrants, etc.) are treated differentially (less favorably) than members of a majority group with otherwise identical characteristics in similar circumstances” (Bertrand and Duflo 2017). We applying the same framework to work conditions, arguing that gender discrimination exists when people of have systematically worse work conditions based on their sex or gender, while having similar worker characteristics, wages and non-wage compensation.

In economics, theoretical and empirical research on work conditions has focused on compensating differentials. A compensating differential for a bad work condition will arise because firms with adverse working conditions will, all else equal, have to offer higher wages than those with attractive work conditions. In the case of sexual harassment, it is possible that workers are economically compensated for a higher risk. Within a workplace, members of the high-risk gender could receive higher wages than members of the low-risk gender. They might also be compensated with other attractive amenities. If that were true, sexual harassment would not meet the economic definition of gender discrimination expressed above.

There are several reasons that compensating differentials for the risk of sexual harassment are unlikely to emerge in the labor market. Bystander preferences may be one reason. There are substantial costs of removing the risk of sexual harassment in a workplace, even in heavily sex-imbalanced contexts. Costs fall into three main areas. First, chronic underreporting makes it necessary to research the extent of the problem. Second, efficient prevention often involves mandatory information and training sessions, for example training on awareness, general civility, or bystander responses. Third, grievance procedures must be set up and operated, as accusations are often disputed and complicated to verify. In the theory of compensating wage-differentials, an employer has incentives to make these investments if the costs can be transferred to workers in the form of lower wages (Rosen 1986). But the willingness to accept these costs are likely to be small among bystanders. These low-risk people make up a significant fraction of the workforces and recruitment pools in gender-mixed labor market contexts, and an even greater fraction in sex-

imbalanced ones. It is also unlikely that potential victims will be willing to themselves carry the costs. In gender-mixed workplaces, the risk of harassment is lower for the average potential victim, which could reduce their willingness to invest in prevention; and in sex-imbalanced workplaces where the risk is higher, there are fewer victims to share the cost.

Another reason is the severe information shortage about the work disamenity of sexual harassment among both workers and employers. Compared to a piece of dangerous machinery in the firm, employers are usually unaware that the health-risk of sexual harassment exists in their workplace, or may at least severely under-estimate its extent. An important reason for this is, of course, the chronic under-reporting of sexual harassment experiences among victims: three in four avoid making any formal complaint in their organization (reviewed by Cortina et al. 2008).

On the worker side, information about risks can perhaps be more accurately inferred from accounts about sexual harassment from friends or colleagues (Cortina et al. 2008). But even this information will only capture a small sub-set of the problem. Information quality might also be low because employers have incentives to suppress or distort information to avoid reputational costs. Trivialization of reports, or even retaliation against the victim, are the most common reactions to intraorganizational reports of sexual harassment (Bergman et al. 2002; Cortina and Magley 2003).

Even if workers and employers would have perfect information about harassment, it may be legally dubious to compensate for higher harassment risks with higher wages. Explicit contracts of that sort would be illegal in most countries and employers are legally obliged to minimize, rather than implicitly allow, these work environments. This type of contract would also be morally reproachable to most people and, on top of that, economically inefficient for the broader labor market (Basu 2003).

Given the reasons discussed above, market-interactions between employers and employees are unlikely to put a price on the work disamenity of sexual harassment. A rare exception from this could, arguably, be a situation of extreme labor scarcity and high levels of information. Without extreme labor scarcity, employers would not be pressured to pay higher wages for the existence of sexual harassment. In any sector of the labor market, and in the sex-imbalanced sectors in particular, low-

risk bystanders will be willing to work without extra compensation and hiring the high-risk person at a higher cost would be economically irrational.

Similar to wage or promotion discrimination, discrimination in work conditions will impose a tax on the victims and reduce their total returns from work. In turn, this is expected to increase their likelihood to leave their jobs or to self-select into specific jobs to begin with (see, e.g. Hsieh et al. 2019 for this formulation for wage discrimination). One way to test for amenity discrimination is, therefore, to analyze overall work satisfaction. If victims of sexual harassment receive lower total rents—monetary plus nonmonetary—from their work than non-victims, their job satisfaction should be lower. The lower total returns from work should also manifest itself in a higher probability to leave the job (Rosen 1986).

Amenity discrimination in the form of sexual harassment has implications for sex segregation and the gender pay gap. Because of the relationship between incidence and sex segregation, women face this amenity discrimination when they take jobs in male-dominated occupations or workplaces. For historical reasons, such male-dominated settings also offer systematically higher pay (Goldin 2014a; SOU 2004). A tradeoff thus exists for women, whose total returns from high-paying, male-dominated jobs are lower than men's total returns from those jobs. For men, the reverse is true, as they are disincentivized in taking lower-paid, female-dominated jobs.

Summary of predictions. We summarize the arguments above as four predictions:

- 1) The risk of sexual harassment grows with the share of opposite-sex people in the occupation or workplace.
- 2) A worker suffers disutility from the risk of sexual harassment.
- 3) A worker suffers little or no disutility when risk of the sexual harassment applies to coworkers of the opposite sex.
- 4) Workers do not receive compensation for the risk of sexual harassment.

3. Sex-segregation and Sexual Harassment

Data on self-reported sexual harassment. Our data come from the Swedish Work Environment Survey, which is collected every other year by Statistics Sweden and maintained by the Swedish Work Environment Authority (<https://www.av.se/en/>). It uses a random nationally representative sample of the employed population aged 16–64, stratified by categories of age, sex at birth, occupation, industry, and social class.⁸ Two questions about sexual harassment were asked in the five surveys between 1999 and 2007. Both questions ask about sexual harassment in the last 12 months and at the hands of colleagues or managers:

1. *In the following question, sexual harassment is defined as unwanted advances or offensive remarks about things that would commonly be associated with sexual behavior. Are you the subject of sexual harassment at work from supervisors or colleagues?*⁹
2. *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 at your workplace from your supervisors or colleagues?*¹⁰

Our main measurement of sexual harassment is a dummy variable that takes the value 1 for respondents who answer affirmatively to either one of the two questions, and 0 for people who

⁸ Documentation of the survey methodology (in Swedish) can be available via the search term “Arbetsmiljöundersökningen” at Statistics Sweden’s homepage. The survey has over 120 questions about working environments, ranging from physical features to job autonomy to work-related injuries or discomforts. About 20 questions are asked over the phone and 100 by post, with an online option added in 2011.

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?”

answer “not at all in the last 12 months” to both. In sensitivity analysis, we change the frequency threshold and analyze each question separately.

The validity of the measurement can be discussed from several viewpoints. One is that the questions cover, albeit in a limited way, three out of four types of sexual harassment in a common four category typology (e.g. Fitzgerald et al. 1999, building on Till 1980). The first type is called *unwanted sexual attention* and includes physical behaviors that range from groping to rape. The second is called *sexual hostility* and includes non-physical behaviors such as crude sexual comments, revealing body parts, or showing pornographic images. A third category is called *sexist hostility* and consists of insulting, degrading, or contemptuous attitudes about women or men in general, or in an occupation. Typical forms of sexist hostility are derogatory and hostile comments about men or women in general, or about their inability to perform a certain job (“there is something wrong with men who want to work in a kindergarten”; “women are not intelligent enough to be economists”). Sexist hostility is sometimes called *gender harassment* and left out of the definition of sexual harassment, for example in the EU regulations discussed in footnote 2. The fourth type of sexual harassment, which is not captured by our survey data, is called *sexual coercion* and consists of sexual behaviors that are linked to threats of punishment or promises of rewards (“If you don’t have sex with me, I will ruin your career”). Web Appendix Table W1 gives a more detailed description by listing behaviors for each of the four categories.

Our survey question #1 defines sexual harassment as “unwanted advances or offensive remarks”, and hence includes behaviors that fall under *sexual hostility* (the remarks) and *unwanted sexual attention* (the advances). Question #2 captures *sexist hostility* by asking for experiences with “condescending and ridiculing statements about women or men in general or in your occupation”. The omitted type of harassment, *sexual coercion*, is the least common type by far (Waldo et al. 1998; Fitzgerald and Cortina 2018).¹¹

¹¹ We can, to some extent, verify this using Swedish data. In the 2001 National Violence Against Women Survey, only 0.2% of employed respondents had experienced that someone “threatened to impair your career prospects if sexual favors were refused” in the last 12 months (authors’ calculations based on data from Lundgren et al. 2001).

Apart from excluding sexual coercion, the questions also give a limited number of behavioral examples. This will likely bias incidence rates downward, as respondents need to recall and classify events that fit the descriptions in the questions. Downward bias will also follow from the first question's specific use of the words "sexual harassment", since survey respondents have been shown to associate that term with more serious or frequent incidents than the standard academic definition would include (Stockdale et al. 1995; Ilies et al. 2003). Men are also less likely to label their experiences as sexual harassment (Marshall 2005; McLaughlin et al. 2012). A source of upward, rather than downward, bias is that Question 2 asks about experiences of being ignored because of one's gender. This is closer to the academic definition of *selective incivility* than sexual harassment.¹²

Our measurement has three main strengths. The first is the low salience of the topic of sexual harassment within the Work Environment Survey. The two questions appear as numbers 27 and 28 of about 120 in the different years, which should reduce social desirability or demand bias for these specific questions among the many other topics that are covered. Second, both respondents and employers are entirely anonymous. Employers do not participate in the collection of the survey, and are unaware that Statistics Sweden has contacted one or several of their employees. Respondents are therefore unlikely to feel any threat of repercussion or intimidation if they truthfully report on bad work conditions. Third, because results are (by design) never reported at the level of the firm, respondents cannot use the survey as a vehicle for purposefully hurting their employer's reputation.

Sex ratios of occupations and workplaces. We use Sweden's mandatory ID codes to link survey respondents in the Work Environment Survey to data from tax records and the government's mandatory employer survey on occupations. These two data sources let us compute the sex ratio in the occupations of 81% of the respondents and in the workplace for all respondents.

We define workplaces—and thereby colleagues—as the unique combination of two ID codes, both of which apply to the respondent's largest source of earned income in the survey year. The Organization ID code defines a firm, such as Walmart, and the Plant ID defines a single building or

¹² 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".

street address where the firm has operations. People who have the same combination of these two codes hence work in the same physical workplace—in this example, the same Walmart store.

We calculate the sex ratio in the workplace as the share of men after excluding the respondent. Information on sex at birth is drawn from the Database for Health Insurance and Labor Market Studies (LISA, based on the Swedish acronym; which also is our data source for other socioeconomic traits, mainly year of birth, region of birth, and years of education). We calculate the share of men in the respondent's 3-digit occupation based on the national labor market data and matched at the occupation-year level to each respondent.¹³ Occupation data come from the Swedish Salary Statistics, a yearly mandatory survey collected by the Swedish government for the full public sector and a stratified random sample of the private sector. The LISA database provides an occupation variable based on this survey plus data from other, sector-specific surveys, which puts the proportion of missing data below 20% of the employed population.

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. Out 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 4 or fewer workers. The final sample size is 40,466 (see Table W2 for year-by-year details).

Table W3 summarizes the representativeness of the survey sample in terms of the general population's sociodemographic traits. Columns 1—3 report the means of demographic variables in the administrative data and columns 4—6 in the survey sample. The administrative data is restricted to employed people¹⁴ aged 16–65 and the same five annual datasets that correspond to the survey years. The comparison shows highly similar proportions of women, people in different age categories, education categories, and income categories in the survey and the population. Foreign-

¹³ The Swedish (SSYK) occupation code is highly similar to the 3-digit ISCO code, and there are 186 occupations at the 3-digit level.

¹⁴ The *Sysstat* variable from the Swedish labor statistics is used to drop non-employed individuals.

born people—particularly those born outside Europe—are under-represented in the survey, likely due to the extensive questionnaire in Swedish.

We use probability weights throughout the paper to match the survey data to the employed Swedish 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—60, and 60>), and education level (less than tertiary education, at least some tertiary education). These variables exist in both datasets, and categories are broad enough to give a reliable number of observations for each interaction. For the analysis of self-reported harassment, we conduct sensitivity analysis for adding birth region and income to the weighting scheme.

Descriptive statistics. Figure 1 shows annual rates of sexual harassment for each survey year and by sex at birth. It shows that 10–13% of women and 5% of men self-reported sexual harassment from a manager or colleague in the last 12 months, a gender-difference that is similar in size to previous research (USMSPB 1994; Gutek et al. 1987; Timmerman and Bajema 1999). There is a slight increase in self-reported harassment rates over time. This is likely reflective, at least in part, of an increased awareness of the concept of sexual harassment (see, e.g. USMSPB 1994).¹⁵

[Figure 1 here]

Appendix Table W4 compares the demographic traits of people who did and did not self-report sexual harassment. Self-reported harassment is more common among younger men and women. For women, there is no correlation with tertiary education or region of birth. Men who report harassment are less likely to have tertiary education and more likely to be born outside of EU28. Only a small proportion of the sample (1.5% of women and 4% of men) became parents to a new child in the survey year, and we found no correlation between becoming a parent and reporting sexual harassment. For both men and women, experiences of sexual harassment coincide with other forms

¹⁵ Sexual harassment was only first introduced in Swedish law in the late 90s, after which it was quickly introduced in the Work Environment Survey. Awareness of the concept is expected to grow 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).

of workplace mistreatment and conflict. Harassment victims are more than four times as likely to report experiences of bullying in the last 12 months, and twice as likely to report conflicts with managers or colleagues.

Sex ratios and sexual harassment incidence. Figure 2 presents rates of self-reported sexual harassment for men and women by the share of men across occupations and workplaces. The points in the scatter plots represent binned averages of the dummy variable for sexual harassment from colleagues or managers in the last 12 months.

Women's self-reports of sexual harassment increase strongly with the share of men across occupations and workplaces. In the most male-dominated occupations and workplaces, one in four women report harassment in the last 12 months, compared to just 5% in the least male-dominated contexts. For men, harassment also grows with the share of opposite-sex people the occupation and workplace, but in a non-linear manner. One in ten men self-report sexual harassment when the share of women is around 80%, but the figure jumps to one in five men when the share of women exceeds 90%.¹⁶ There is no indication of a U-shaped relationship where men report more harassment in the most male-dominated environments.

Web Appendix Table W5 displays a ranking of the highest sexual harassment rates across 3-digit occupations. To ensure reliability, we restrict the comparison to occupations with at least 50 survey respondents. Women have the highest rates among 1) motor vehicle drivers, 2) architects and engineers, and 3) writers (journalists) and performing artists. College and University professors rank fourth. Men have the highest rates in 1) pre-primary education, 2) nursing, and 3) other care work.¹⁷

¹⁶ The non-linearity could indicate a larger importance of norms than contact. It could also be explained by more detailed, threshold hypothesis from the theory of "sex role spillover theory". Members of relatively small gender minorities are argued to "stick out" in the workplace and become viewed as women (or men) first, and colleagues later, which increases the risk of sexualization and harassment by the opposite sex (Gutek and Morasch 1982, building on, e.g., Kanter 1977).

¹⁷ Higher rates of harassment in women-dominated sectors correspond to recent reports of the sexual harassment of men in the Swedish media (e.g. Aftonbladet Aug. 2011, Aftonbladet Apr. 2002). Accounts talk of women colleagues who 'cat call' men in the break room, comment on male colleagues' appearance, or say they need to lose weight or get a haircut "now that he's around".

For both men and women, the scatter plots for occupation and workplace sex ratios presented in Figure 2 look similar to each other. This begs the question whether one of the ratios accounts for the other. Including both variables in an ordinary least squares (OLS) regression shows that the workplace sex ratio can partly account for the correlation with the occupation sex ratio. The point estimate on the occupation sex ratio is reduced in half when we add the workplace sex ratio, but remains statistically significant at the 5% level and of an economically meaningful size. Meanwhile, the size and significance of the workplace sex ratio is unaffected in the regression with both variables (see Columns 1–3 in Web Appendix Table W6). Neither of the two sex ratio estimates are affected by adding demographic controls and a dummy variable for being a supervisor (Column 4).¹⁸ In sum, both sex ratios have independent and sizeable relationships with the rate of self-reported sexual harassment from colleagues and managers, and the relationships are not confounded by respondents' demographic traits or hierarchical position in the workplace.¹⁹

[Figure 2 here]

We make four variations to Figure 2 to address potential sources of measurement error. First, we show that the patterns hold for a higher-frequency cut-off for the sexual harassment variable (Web Appendix Figure W1). We then show that the relationships are similar for each of the two sexual harassment question when specified as separate dummy variables (Web Appendix Figures W2 and W3). A third variation targets the potential omitted variable of the gender-power hierarchy in the firm. We show that the sex of the respondent's closest supervisor cannot account for the relationships (Web Appendix Figure W4), and that the same is true for the respondent's own status as a supervisor or employee (Web Appendix Figure W5).

¹⁸ Respondents are defined as supervisors if they reported in the Work Environment Survey that at least some part of their job involved "leading or delegating work for other employees".

¹⁹ In a companion paper, we describe patterns of self-reported sexual harassment from extraorganizational perpetrators such as customers, clients, or patients (Folke and Rickne, 2020). This harassment correlates strongly with the sex-composition of the occupation but not with the sex-composition of the workplace. Both men and women self-report more extraorganizational harassment in female-dominated occupations like nurse, social worker, or elderly care worker—jobs that involve more interactions with workplace outsiders during the workday. An important take-away for the results in this paper regards women in female-dominated occupations. Not all these women have a low risk of sexual harassment, like Figure 2 would suggest. Some are severely exposed to harassment from workplace outsiders, a workplace hazard that largely requires different types of employer interventions than intraorganizational misbehavior.

In the fourth variation we address the potential measurement problem of “false reporting”. Gender minorities might feel unhappy with their work situations and recall, intentionally or unintentionally, more negative events at work. Arguably, if this type of over-reporting was going on in our data, it should apply to more forms of workplace mistreatment. In Web Appendix Figures W6—W9 we show that gender minorities in occupations or workplaces do not self-report more (or less) experiences of bullying, conflicts with colleagues and managers, or appreciation, encouragement and support from supervisors. Notably, several theories would predict under-reporting, rather than over-reporting among gender-minorities. In the theory of compensating differentials, people with the smallest distaste for a specific disamenity sort into workplaces where it exists (Rosen 1986) and would likely be less prone to over-reporting. Sociologists make similar predictions about by considering “assimilation” (e.g. Timmerman 2005). Women who choose male-dominated jobs are more likely to view sexist behaviors as part of the ordinary workday, and would be less likely to recall these—both in general and as particularly problematic—when asked to do so in a survey.

The Web Appendix shows two more robustness tests. Figure W10 replicates Figure 2 with the share of men computed in 4-digit occupation codes and Figure W11 replicates it with a more complex scheme for the sample weights that includes region of birth and income (see the figure note for details).

5. Experiment on Sexual Harassment and Job Choice

We use a hypothetical job choice experiment to quantify the disutility from the risk of sexual harassment in the workplace. We compare this disutility both between men and women, and between people who are *potential victims* or *bystanders* to sexual harassment in the workplace.

Experiment design. The design of our job choice experiment closely follows the method pioneered by Eriksson and Kristensen (2014) and reproduced by, e.g., Wiswall and Zafar (2017), Mas and Pallais (2017), Gelblum (2019), Maestas et al. (2018) and Datta (2019). The survey respondent is told to “*Imagine that you are looking for a job and have two offers. You will see short descriptions*

of these offers and chose 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 two jobs are presented in a table with two columns, one for each job (A and B); and where each row contains one job trait (see screenshot in Figure W12). We randomize both the order of the rows and the values in the cells. Underneath the table, the respondent sees two boxes and clicks the box that corresponds to their preferred job. Each respondent repeats this exercise three times, each time with a new randomized choice. By randomizing both the wage and the work conditions of the fictional jobs, respondents’ choices can tell us how they weigh the two against each other. Our quantity of interest is the utility—measured in terms of a positive or a negative wage change—that respondents place on the work condition of sexual harassment. This quantity is commonly called the Willingness-to-Pay.

Stated-preference experiments have two main advantages. In the specific case of sexual harassment risks, it helps us overcome the ethical infeasibility of randomizing this job trait in the actual labor market. In more general terms, the experiment measures job choices across a spectrum of jobs, whereas observational labor market data generally lacks information on rejected jobs (for a formalized discussion of this issue, see Wiswall and Zafar 2017). We embedded our experiment in the Swedish Citizen Panel and introduced it to respondents as a “study on job choice”. It was administered to an online sample of 5,699 employed Swedish citizens and reached a 70% response rate over a one-month period (30 September—24 October 2019), for a sample size of 3,987.²⁰

²⁰ The Swedish Citizen Panel is maintained by the Department of Political Science at the University of Gothenburg in Sweden. It has been used in over 100 social science studies in the last decade. 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; Datta 2019; Gelblum 2019). For more information, see <https://medborgarpanelen.gu.se/english>.

A major drawback of the method is *hypothetical bias*, which stems from a situation where the fictional choice situation is too unrepresentative of actual job choices. Recent research has mitigated this concern by showing that preference estimates from job choice experiments are (roughly) replicated in field experiments on actual job seekers (He et al. 2019; Mas and Pallais 2017). In our experiment, we ask respondents if they are “*aware of any workplaces in their industry where sexual harassment has occurred*”. We then address the issue of hypothetical bias by showing that our main results derive from the sub-sample of informed respondents, for whom the fictional job choice is more similar to their actual choices.

Apart from sexual harassment, we include three job traits based on a review of prior research coupled with institutional knowledge about the Swedish labor market (discussed further below). The goal was to include the most important traits for job choice without diluting the quality of responses by having too many characteristics to compare. The first trait is the wage, given as a percentage change compared to the respondent’s current wage: “5% more than your current wage”; “10% more than your current wage”; “same as your current wage” and “5% less than your current wage”. These values were chosen to reflect the distribution of actual wage changes upon job switches in the Swedish labor market.²¹ The second trait is skill development, described as the three broad categories of “low”, “moderate” or “high”. Note here that the Swedish term for skill development (*utvecklande jobb*) can refer to either technical, inter-personal, or other types of skills. The third trait is schedule flexibility, motivated by the strong focus on this trait in recent empirical research (see, e.g., small valuations in Mas and Pallais 2017 and He et al. 2019 and large valuations in Eriksson and Kristensen 2014 and Maestas et al. 2019). For this trait we used three values for “No flexibility”; “Flexible start and end time within 1 hour”; and “Complete flexibility”. In the randomization, equal weights were put on each value of each trait.

²¹ We calculated the distribution of year-on-year changes in the deflated monthly wage 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-Inter Quartile 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 close to 60% of the observations fall within our band of -5 to +10%.

We inserted information about sexual harassment into the fourth job trait, the work environment. In addition to sexual harassment, this trait had three other values: (1) a good work environment where “people in the work unit seem content with the work environment”; (2) a null condition of “no particular information”; and (3) a negative cue that “some employees in the work unit have had conflicts with the manager”. Conflicts were included mainly as a red herring to derail respondents from the true purpose of the experiment.

Sexual harassment was included as brief descriptions of incidents rather than using the term “sexual harassment” (following Hulin et al. 1996). These incidents were described as having taken place in the work unit of the potential job offer, i.e. among the respondent’s physically proximate colleagues should they accept the job. In order not to skew the results by representing sexual harassment in a specific way, we used three different vignettes. Each described a highly prevalent behavior among the behaviors that fall under three categories of sexual harassment: unwanted sexual harassment, sexual hostility, and sexist hostility.²² For unwanted sexual attention, the behavior consisted in inappropriate touching, sexist hostility was described as a person stubbornly discussing their sex life, and sexist hostility was described as an attitude that a particular sex was unsuitable for the job.

To classify respondents into potential victims and bystanders, we asked if their current workplace consisted of: (i) mostly women, (ii) about the same proportion of men and women, or (iii) mostly men.²³ People working in a mostly female work environment were shown incidents with male victims and female perpetrators, making male respondents the potential victims and female respondents the bystander, should they accept the job. People in mixed or mostly male work environments saw vignettes with female victims and male perpetrators, i.e. treating women as the potential victims and men as the bystanders. We also measured bystanders and potential victims using a self-classification question (details below).

²² These prevalence rates were measured in a survey of 3,000 employed American women in spring 2019, who answered the Sexual Experiences Questionnaire, a common survey instrument for measuring sexual harassment. Table W2 shows a full list of the sexual harassment behaviors included in the SEQ.

²³ People who responded “Don’t know/Don’t have any colleagues” were asked to provide the sex composition in “a typical workplace where people with your occupation/education have colleagues”.

To gain statistical precision, the sexual harassment incidents were over-sampled in the randomization of values for the work environment trait. The probability to see a vignette for sexual harassment was set to 33%, 11% for each of the three incidents, while the probability for the three other values of the work environment trait was set to 22%.

[Table 1 here]

Respondents were told that fictional jobs were “identical in all other respects” in order to mitigate potential bias associated with omitted job traits. But excluded traits could still inflate the estimates if we force respondents to make choices across traits that would be unimportant in their actual job choices. We acknowledge that this may cause some upward bias in the estimates. We would argue that excluded traits are less important in the Swedish labor market than the included ones. There is relatively little variation between jobs in 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. Labor laws also imply less variation in job security, and only 13% of the workforce has a work week that exceeds 40 hours (Statistics Sweden 2017).

Sample size and representativeness. From the raw data on three job choices per respondent, we remove all choices where one job has equal or better values on all traits than the other. This is true for 39.2% of the observations and leaves data from 7,439 job tables. Table W3 compares the representativeness of the respondents making these choices to the Swedish workforce at large, showing that the representativeness is quite low in terms of education and income. In the internet sample, 77% have completed tertiary education, compared to 34% of the general population, and 77% (vs. 24% of the population) have a monthly income over 30,000 SEK (3,000 USD). The age distribution is more similar, with the exception of a strong under-representation of the youngest age bracket in the internet sample. We use the weighting scheme described in section 3 to correct these imbalances.

Estimating the Willingness to Pay. The experiment generates data at the level of the hypothetical job. Following Hainmueller et al. 2014, we regress a dummy variable (Y_{itj}) for whether individual i selected a specific job j in table t , on a fully saturated dummy variable structure for all job traits,

using OLS and clustering the standard errors at the level of the respondent. The regression equation is:

$$Y_{itj} = a + \beta_1 W_{itj}^{-5} + \beta_2 W_{itj}^{+5} + \beta_3 W_{itj}^{+10} + \beta_{sh} SH_{itj} + \beta_x X'_{itj} + e_{itj}, \quad (1)$$

where the first three variables are the dummy variables for jobs that give a 5 percent lower, 5 percent higher, and 10 percent higher wage than the respondent's current job, using no change as the reference category. The dummy variable for sexual harassment (SH_{itj}) takes a value of 1 if a hypothetical workplace was described as either of the three sexual harassment incidents. We use having "no information" about the work environment as the reference category. The other two values for the work environment are included as dummy variables together with all other remaining dummies in a vector denoted X'_{itj} .

The estimate β_{sh} on the dummy variable for sexual harassment captures the percentage-point impact on the likelihood of choosing that work context compared to the reference category of having no information. The average probability of choosing jobs that entail a wage change of -5%, +5%, or +10% relative to the reference category of a job with a wage that is the "same as your current wage", is captured by the point estimates on these three dummy variables (β_1, β_2 , and β_3). The WTP is the ratio between the point estimate for sexual harassment and a weighted average of the 3 wage estimates. We combine the wage coefficients to produce a joint coefficient for the probability of selecting a job when the wage changes by 1 percentage point, using the formula: $WTP_{sh} = b_{sh} / \left[\left(-\frac{b_1}{5} + \frac{b_2}{5} + \frac{b_3}{10} \right) / 0.03 \right]$. The resulting ratio is the monetary equivalent of the disutility taking a job in a workplace with sexual harassment compared to a job in workplace where there is no information about the work environment.

WTP estimates for sexual harassment. Figure 3 contains WTP estimates for the full sample (left) and for men and women separately (right). All estimates are of the expected signs and reasonable in magnitude. As in previous studies using the same method, they show that differences in job amenities are clear predictors of stated job choices. In other words, respondents are willing to forego substantial earnings for better working conditions.

In the full sample, the size of the WTP for sexual harassment is such that taking a job with sexual harassment equals a 9.05-percentage-point wage decrease. Men have a slightly higher valuation than women, corresponding to a 10.6-percentage-point wage decrease compared to women's 7.9 percentage points.²⁴ These two estimates are not significantly different from each other at the 5% level.

[Figure 3 here]

In Figure 4 we compare the WTP for avoiding firms with sexual harassment between potential victims and bystanders. According to the survey question about the sex ratio of respondents' workplaces, 33% are defined as potential victims, with a high harassment risk, and 66% as bystanders with a low risk. Estimating equation (1) separately in each of these sub-samples reveals a striking difference. For potential victims, taking a job in a firm where sexual harassment has occurred is valued as a 17% lower wage. Bystanders are also reluctant to take these jobs, but the average valuation is much lower—equal to a 6% lower wage. The difference between these two estimates is statistically significant at the 5% level. To the immediate right of these estimates, we show that the results are similar when we define respondents as potential victims or bystanders based on their own, self-described harassment risk.²⁵

[Figure 4 here]

Next, we split the sample by the level of information that respondents have about incidents of sexual harassment (using the survey question described above). We define a person as uninformed if they have “no information” (36% of respondents), and informed if they know of “some cases” (49%) or “multiple cases” (15%) (3,364 respondents answered this question). Among people who have

²⁴ Recalculating these by using the estimate on the 5-percentage point wage decrease, rather than the average of the decrease and increases, gives smaller valuations of 3.2% for women and 4% for men.

²⁵ Respondents were asked to “*Imagine that you have received a job offer from a workplace where you heard that there had been sexual harassment. If you accepted the offer, what do you think the risk would be that you became the target of such harassment?*” (A low risk, A pretty low risk, Neither a high nor a low risk, A pretty high risk, and A high risk). People who answered “A low risk” were coded as bystanders (a total of 55% of the respondents) and the remaining 45% were coded as potential victims. The estimates show that self-defined potential victims value sexual harassment as a 12.5% wage decrease and bystanders as a 4.3% wage decrease. Recalculated for only negative wage-changes, they are 8% for potential victims and 2% for bystanders.

information, the hypothetical job choice in the experiment should be a more realistic representation of their actual job choice situation, should they decide to switch. Bias in the results from a lack of realism due to hypothetical bias should therefore be the smaller for these respondents. Interestingly, the subsample of informed respondents shows twice the size of the WTP gap between bystanders and potential victims compared to the full-sample analysis. An informed potential victim has a WTP of nearly 40% of the wage while an uninformed potential victim has a WTP of about 5%, and not statistically significant at the 5-percent level.

The rightmost section of Figure 4 shows gender-separated estimates for female (red dots) and male (blue dots) victims and bystanders across the three categories of sex segregation (female dominated, mixed, and male dominated). The results are less precise in these subcategories, but in all three, bystanders' distaste for sexual harassment is smaller than that of potential victims. We find the largest estimated distaste among male potential victims in women-dominated workplaces and the second largest for female potential victims in male-dominated workplaces.

Results from an analysis of the three different sexual harassment vignettes is shown in Figure W13. In the pooled, total sample of respondents, the gap between bystanders and victims is reproduced for all three types of sexual harassment. Further splitting the bystanders and potential victims by their sex shows a more complex pattern. Men in women-dominated workplaces are the most averse to sexist hostility—comments that men are not suitable for the job—and less averse to groping or intrusive, sexual conversation. Women in male-dominated workplaces exhibit the opposite pattern: sexist hostility is the least sensitive, and sexual advances are the most off-putting. These results are consistent with previous research showing that men are less likely than women to perceive sexualized behaviors as offensive (e.g. Quinn 2002). But the results for sexist hostility show that men are more sensitive than women to having their job suitability questioned when they chose counter-stereotypical jobs.

Robustness and validation checks. This section summarizes a longer description of our sensitivity analysis, presented in Appendix Section W1. Figure W14 shows that the results above are not an outcome of systematically different preferences for wages across women and men, or bystanders and

potential victims. Figure W15 shows five sensitive checks. First, we remove the sample weights. This shrinks the WTP gap between potential victims and bystanders, but it remains statistically significant at the 5% level. The size of the gap grows, instead, when we restrict the sample to people who show attentiveness in their survey responses. This indicates that inattentive respondents are not responsible for producing the baseline finding.²⁶ The right-hand side of the figure shows the sensitivity of the results to changing the estimation method. There is little variation in the results across logit, probit and mixed-logit specifications, demonstrating a lack of sensitivity to distributional assumptions.

6. Compensation for Sexual Harassment Risks

In the previous sections we have seen that the risk of sexual harassment varies systematically with gender across the sex segregated labor market. An important insight was that within the same workplace, one gender usually has a relatively high risk of harassment while the other has a low risk. We also saw that a high risk caused substantial disutility for the potential victims. In this section, we analyze if high-risk individuals, conditional on observables, receive compensation within the workplace compared to low-risk individuals.

We use two different methods to measure the risk of harassment. The first follows the standard in the literature on compensating pay-differentials by calculating the predicted probability of the disamenity based on observable characteristics of jobs (e.g. Hersch 1998; Lavetti, and Schmutte 2018; Lavetti 2020). The second method simply uses the data on self-reported sexual harassment in the Work Environment Survey as a measurement of risk, either (trivially) for the individual, or using the self-reports as a proxy for the risk level (high or low) in a workplace.

To calculate the predicted risk, we first regress the dummy variable for self-reported sexual harassment on the share of men in the workplace and fixed-effects for 3-digit occupations. This is done separately for women and men in the pooled cross-sectional data from the Work Environment

²⁶ We define attentive and inattentive respondents based on the data from strictly dominant job-choices, i.e. where one job in the job choice table had equal or better values than the other job for all traits. People who correctly chose the dominant job are defined as attentive respondents, and vice versa for inattentive respondents. For more details, see Web Appendix Section 1.

Survey. To avoid noise in the prediction we omit occupations with fewer than 50 respondents of each gender. Next, we use the estimated coefficients to predict the probability of harassment for the full Swedish labor force. This produces two variables for the individual's work environment: the risk of sexual harassment for women, and the risk for men.

To test if high-risk workers receive wage compensation relative to low-risk workers, we analyze the relationship between risk gaps and wage gaps at the workplace level. These two variables are calculated as follows. For each workplace with 5 or more employees, we calculate the female-male risk gap as the average risk for the women minus the average risk for the men, using the predicted values. We then calculate the female-male wage gap conditional on the occupation and age structure. In each workplace, we compute the average difference in the monthly log wages in full-time equivalents between women and men in each combination of 3-digit occupation and 5-year age bracket, and then take the average of these gaps.²⁷

Figure 5 shows the relationship between the risk and wage gaps by plotting binned averages for these workplace-level measurements. In correlation in the left graph shows a negative relationship. As women's risk of sexual harassment relative to men increases, their relative wage decreases. Strikingly, the only workplaces in which women are paid more than men (conditional on occupation and age) is where their risk of sexual harassment is lower than men's. This pattern directly contradicts the existence of economic compensation for the high-risk sex relative to the low-risk sex within workplaces.

In the right graph, we proceed to controlling for observables. The lack of evidence for compensating wages could depend on omitted variables at the individual level such as productivity or firm tenure, or on omitted variables at the workplace level, such as workplace size or industry sector. To control for the individual-level factors we add two sets of variables to the calculation of the gender wage gap in the workplace, fixed effects for the number of years of tenure in organization, and individual fixed-effects from an AKM model (details in the figure note). We add the workplace

²⁷ In practice, we do this by taking the average male and female residual from a wage regression with interacted fixed effect for workplace, 3-digit occupation and 5-year age bracket. By necessity, this calculation excludes any occupation-age combination, and/or workplace, with 100% men or women.

controls by residualizing the wage and harassment gaps on the controls before plotting the relationship. These controls are fixed effects for the 5-digit industry and linear and squared controls for workplace size. In the graph, the relationship between relative risks and relative wages becomes entirely flat, providing further evidence that there is no compensating pay for the relative risk of sexual harassment.

[Figure 5 here]

Next, we switch the risk measure to the self-reported sexual harassment in the Work Environment Survey.²⁸ We plot the distributions of workplace gender wage gaps, calculated as in Figure 4, in four sub-samples of data: by the sex of the respondent and whether he or she reported sexual harassment or not. Note that this analysis counts the workplace as many times as it has respondents, and may count it as having both a high and low risk if two respondents reported differently. A workplace can also be high risk for men and women at the same time if a man and women respondent both self-reported sexual harassment.²⁹

Figure 6 plots kernel density estimates of the female-male wage gaps, both with and without the additional individual and sector controls (left vs. right graphs). We compare the distributions of the wage gaps in the workplaces of the respondents who self-reported sexual harassment (solid lines) and the respondents who did not (dashed lines). For both women and men, these distributions largely overlap and, if anything, women's wage disadvantage is *larger* in workplaces where a female respondent self-reported sexual harassment. For men, the results also point in the opposite direction with respect to compensating pay. The overlap becomes even more clear when we calculate the gender wage gap while adding the individual-level control variables and industry controls (following the same procedure as in the right-hand side of Figure 5).

²⁸ This alternative measurement has both advantages and disadvantages. The key advantage is that it factors in any workplace prevention efforts. Thus, the high-risk firms that are able to prevent sexual harassment are also able to pay a lower relative wage to potential victims. This idea is at the heart of Rosen's (1986) seminal theory on sexual harassment. At the same time, using self-reported analysis at the individual level forces us to assume that the harassment of one individual is positively correlated with the probability of harassment of their same-sex co-workers.

²⁹ In the full survey data, 28.5% of the individual observations are in a workplace with more than one respondent, and 22.7% as in one with both a male and a female respondent.

As a sensitivity test, we re-run the analysis in Figures 5 and 6 in a sub-sample of “most-likely” data for compensating differentials (see Appendix Figures W16 and W17). Workplaces are included if they meet two criteria. First, that neither sex is below one third of the workforce, ensuring that the high-risk sex makes up a relatively large share. Second, that the degree of year-to-year wage flexibility is above average (we calculate wage flexibility in the administrative data, see figure note for details). As in the full-sample analysis, we find no indication of economic compensation for the risk of sexual harassment.

We test for compensating pay-differentials at two additional margins. Both compare wages within the workplace rather than across workplaces. The first margin is the wage gaps between women and men who have the same 3-digit occupations within workplaces. The second margin is the individual worker compared to their colleagues. Neither of these analyses, which we present in Web Appendix Section W2, provide evidence for compensating pay for the risk of sexual harassment.

Job dissatisfaction and job separations. It is possible that compensation for sexual harassment happens via amenities rather than wages. One way to approach this question is to compare measurements of total rewards from work, such as job satisfaction and job separations (Rosen 1986, see also Böckerman and Ilmakunnas 2009). The Work Environment Survey offers survey questions on job satisfaction and quit intentions, and we can use our employer-employee panel data to compute actual job separations.

We define three dummy variables for i) job dissatisfaction, ii) quit intentions, and iii) job separation, and regress each one on self-reported sexual harassment. The regression results are reported graphically in Figure 7 estimates. The top estimate (black dots) show the bivariate relationship. Below follow estimates for regression specifications that include an increasingly demanding set of control variables. The first adds fixed-effects for 3-digit occupation codes. The second adds dummy variables for categories of demographic traits: age, education level, and birth region. The third adds the share of men in the workplace as a continuous variable, and the fourth, with the lightest gray color, adds two dummy variables for self-reports of bullying and conflicts with either colleagues or managers, both in the last 12 months.

Starting with job dissatisfaction, this dummy takes the value 1 for people who were “dissatisfied” or “very dissatisfied” with their current work situation, and otherwise zero. The results show a much higher rate of job dissatisfaction among people who self-reported sexual harassment. The rate is 7 percentage points higher for female harassment targets, and 12 percentage points higher for male targets, which corresponds to a doubling of the baseline rates of 7% for women and 9% for men. These differences remain sizeable and statistically significant also after including the most extensive set of control variables.

We measure quit intentions with a dummy set to 1 for survey respondents who considered leaving their job for health reasons in the past year (a yes-or-no question). Figure 7 shows that both female and male harassment targets are about 20 percentage points more likely to have had quit intentions in the last year, against baseline rates of 19% for both sexes. Like the results on job dissatisfaction, these estimates are robust to adding the controls.

To analyze actual job separations, we obtain a dummy variable from the longitudinal employer–employee data where we can follow survey respondents in each year before and after they responded to the survey. Because of switching costs, sexual harassment may trigger job separations with a time lag, working its way via quit intentions and job search. In Figure 7 we define a job separation as having switched out of the surveyed workplace three years after the survey year. A switch out of a workplace can happen either by moving to another workplace (so that the other workplace becomes the person’s largest source of labor income) or by exiting the labor market (so that labor income is zero). We drop observations for people who are older than 60 to remove switches to zero labor income due to retirement.

Women and men who self-report sexual harassment are about 5 percentage points more likely to have separated from their workplace three years after they responded to the Work Environment Survey. For both sexes, this represents about a 15% increase from the baseline rate of 35%. The size of the estimate can be compared to the increased probability of job separation from a major life event, parenthood, which is 11 percentage points for women who have their first child (for men, this major life event does not increase the probability of job separation, authors’ own analysis). The results for

job separations are less robust to adding controls. Men's coefficient goes to zero and women's coefficient becomes statistically insignificant at the 5-percent level in the most extensive specification which includes bullying and conflicts.

[Figure 7 here]

Appendix Figures W21 and W22 replicate Figure 7 for each of the two survey questions on sexual harassment, showing similar negative consequences for both. In Web Appendix Figure W23, we extend the job separation analysis to an annual event study from four years before the survey year to eight years thereafter. Harassed women's relative probability to leave the workplace starts increasing after the survey year and grow over time after the survey. For men, there is an increase after the survey year, a peak in the third year, and then a disappearance of the relative difference as non-harassed men's separation rate catches up.

Do people who switch jobs after sexual harassment select more gender-segregated workplaces? For each survey respondent who is coded as separating from their job within 3 years of taking the survey, we calculate the difference in the share of men in their workplace before and after the switch. Figure 8 presents the results of this analysis, comparing the distributions and averages of changes in the share of male coworkers between switchers who reported harassment and those who did not.

[Figure 8 here]

Figure 8 shows that women switchers who self-report harassment leave for workplaces with a lower share of male coworkers by an average of -2.9 percentage points. For non-harassed women, the average job switch increases the share of male coworkers by +1.3 percentage points. Men who report harassment do not switch to workplaces with more, or less, men; nor do men who report zero harassment. These results are suggestive that sexual harassment of women, but not that of men, is associated with job separations that increase sex segregation in the labor market.

7. Sexual Harassment as Amenity Discrimination

Our analysis has shown that sexual harassment is an uncompensated workplace disamenity. This might be surprising to economists. In a competitive labor market, amenities and disamenities are

expected to be prized at their market values. We have argued, and shown empirically, why this is unlikely for sexual harassment. First, we showed that the victimization risks of sexual harassment are split along gender lines in the sex-segregated labor market. In gender-mixed and male-dominated workplaces, harassment is mainly directed at women, while it is mainly directed at men in female-dominated workplaces. Second, even though the exposed sex has a strong disutility for the harassment risk, they coexist in the workforces and recruitment pools with bystanders of the opposite sex, who do not share this preference. Because firms' incentives to invest in a safe work environment depends on aggregate demand, bystanders will depress the demand for prevention and hinder the emergence of compensating differentials.

As mentioned above, the lack compensation for sexual harassment is consistent with previous research in other academic disciplines. Whistle blowing on sexual harassment is usually met with trivialization or with social or professional punishment, rather than economic rewards. Our findings also align with another important strand of research centering on tolerant organizational cultures (Fitzgerald et al. 1997). A culture of tolerance is characterized by punishing and doubting the credibility of accusers, while protecting perpetrators. Our survey experiment offers novel way to quantify tolerant attitudes. We also show that tolerance may stem from dynamics of in-group/out-group hostility or altruism in the workplace.

Our results strongly suggest that gender discrimination in amenities is a barrier to the sex integration of the labor market. For both men and women, the incidence of (uncompensated) harassment in the most sex-segregated contexts provides a clear disincentive to taking such jobs. This incentive structure shapes gender equality in pay, status, and voice in society. In particular, we have shown that the sexual harassment of women is not concentrated in blue-collar, low-wage jobs; it is also commonplace in high-wage, academic occupations. The higher rate of amenity discrimination for women in those high-wage contexts stands in the way of equalizing pay, status, and voice. Occupational gender segregation is currently thought to explain between one-third to one-half of the gender pay gap (Goldin 2014b; Blau and Kahn 2017) and workplace segregation around 15% (Card et al. 2015).

Our argument about discrimination in amenities has implications for future research on compensating pay-differentials. We observe no gender difference in disutility from the existence of sexual harassment in a workplace. But we also find no evidence that having a higher harassment risk is associated with wage compensation. The sorting and pay inequalities that arise from this situation should therefore not be viewed as efficiency enhancing. It is not the case that women and men have different preferences for the same amenity and sort into firms that match their tastes. The sorting is instead reflective of an uncompensated disamenity that drives a wedge between men and women in the same occupation and/or workplace in terms of their returns to work. For women, who have a substantially higher victimization rate than men, this amenity discrimination is likely causing a systematic selection into less lucrative occupations and workplaces.

A parallel between wage discrimination and amenity discrimination can be drawn regarding the impact of market forces. We have argued that a low demand for prevention or compensating differentials for sexual harassment among bystanders is allowing mistreatment to thrive on the horizontally sex-segregated labor market. When the labor supply of bystanders is abundant, firms do not need to cater to workers from the victimized sex who make costly demands. But if the labor market tightens so that firms' recruitment depends more critically on the victimized sex, the incentives might change. As mentioned above, such relationships between pressures from the labor market and incentives for harassment prevention is a possible avenue of future research.

8. Conclusions

Legal frameworks around the world recognize sexual harassment as a form of workplace gender discrimination (EEOC 2016; Hoel and Vartia 2018). This paper has related sexual harassment more closely to economic theory and conceptualized it as gender discrimination in work amenities. In contrast to wage discrimination, where a worker of equal quality is paid less because of their sex, amenity discrimination gives a person of equal quality worse work conditions.

Our analysis shows that amenity discrimination in the form of sexual harassment is a larger problem for women than for men in the labor market, but that men are also affected in highly female-

dominated contexts. These results corroborate earlier research in other academic disciplines but using nationally representative data with continuous and objective measures of sex ratios for both occupations and workplaces. Future work on prevalence rates could extend this analysis to sex-imbalanced contexts that predate the labor market entry. For example, sexual harassment in educational environments could perhaps help explain the systematic dropout patterns of women from male-dominated tertiary education programs, as recently observed by Astorne-Figari and Speer (2019).

A second conclusion is that the risk of sexual harassment brings substantial disutility to the individual, but that risks directed at social out-groups in the workplace does not. In our survey experiments, respondents recoiled from jobs where a person of their own sex had been harassed, but much less so from jobs where the victim was of the opposite sex. This lack of disutility from opposite-sex victimization is likely important to understand employer incentives for prevention.

If bystander attitudes depress the demand for prevention, policies that change these attitudes could push employers toward making these investments. Future studies could explore the mechanisms that underpin bystander attitudes to guide policy design. For example, bystanders could be making different appraisals across types of harassment incidents. Our results on dislikes of sexist hostility offer a potentially interesting starting point. Sexist hostility, such as comments that women or men are not capable of performing a specific job, are often picked out by harassment victims as a more impactful behavior than other types of harassment (Mazzeo et al. 2001). In our results, men bystanders in male-dominated workplaces had a zero disutility on this phenomenon, while the dislike was strikingly high for male potential victims of these behaviors in women-dominated contexts. This indicates that the damage of some types of mistreatment, such as low-frequency comments about a group's supposed "bad fit" for a particular job, might be particularly hard to understand from an outside perspective and require innovative policy solutions.

A third and final conclusion is that gender amenity discrimination in the form of sexual harassment likely contributes to sex segregation of the labor market and misallocation of human capital. Sexual harassment is particularly severe for women and men who chose to work in

occupations and firms where they are a gender minority. Knowledge of this discrimination could pre-date labor market entry and affect occupational choices. Information revelation over time is also likely to shape wage-trajectories and hurt productivity and health in the presence of switching costs between workplaces and occupations. Future theoretical and empirical research could investigate these dynamics of information, sorting, and health impacts. This may also be worthwhile for amenity discrimination against labor market minorities along other sociodemographic traits.

Our findings have several implications for employers and policy makers. For employers, they show that the business case for harassment prevention includes an expanded recruitment pool, especially among gender minorities. They also show that harassment prevention within firms could minimize harmful turnover costs and work disengagement from job dissatisfaction or quit intentions. For policy makers, our results show that market forces alone cannot be expected to solve the problem of sexual harassment. Firms in sex-imbalanced contexts have a low demand to create safe environments for gender minorities. Outside intervention may be needed to shift the employers' incentive structure in the direction of prevention.

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Tables and Figures

Table 1: Vignettes for Sexual Harassment in the Hypothetical Job Choice Experiment.

Sex-composition in the respondent's workplace	Sexist Hostility	Sexual Hostility	Unwanted Sexual Attention
Mostly Women	Women in the work unit have expressed that men are not suitable for the job	A woman in the work unit makes male co-workers uncomfortable by stubbornly discussing her sex life.	A woman has groped a man in the work unit.
Mostly men OR Roughly the same number of men and women	Men in the work unit have expressed that women are not suitable for the job	A man in the work unit makes female co-workers uncomfortable by stubbornly discussing his sex life.	A man has groped a woman in the work unit.

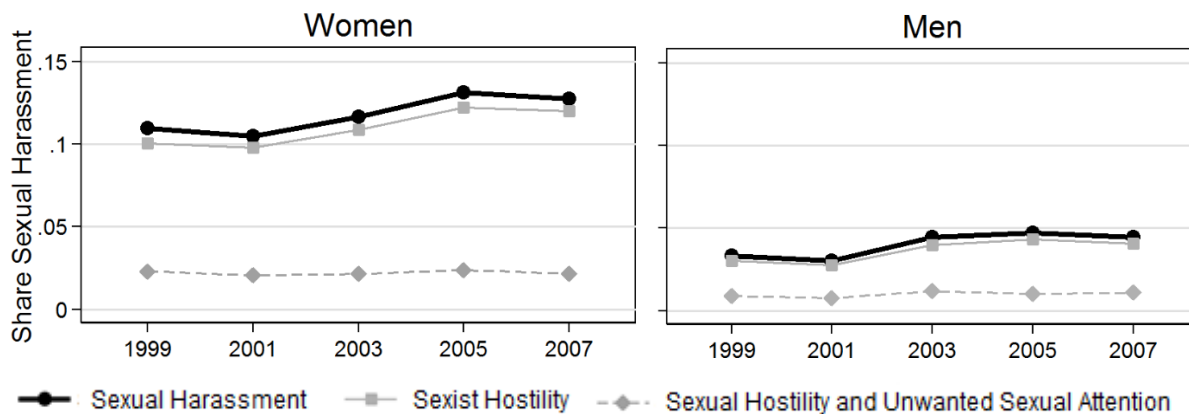


Figure 1: Incidence of Sexual Harassment for Women and Men.

Notes: The figure shows the proportion of women (left) and men (right) who self-report nonzero sexual harassment in the last 12 months from colleagues or managers in the Swedish Work Environment Survey. The gray solid line represents responses to the survey question about sexist hostility, and the gray dashed line represents the two survey questions sexual hostility and unwanted sexual attention. Details about these questions are shown in section 4.1. The black line shows the proportion of respondents who self-reported either kind of sexual harassment.

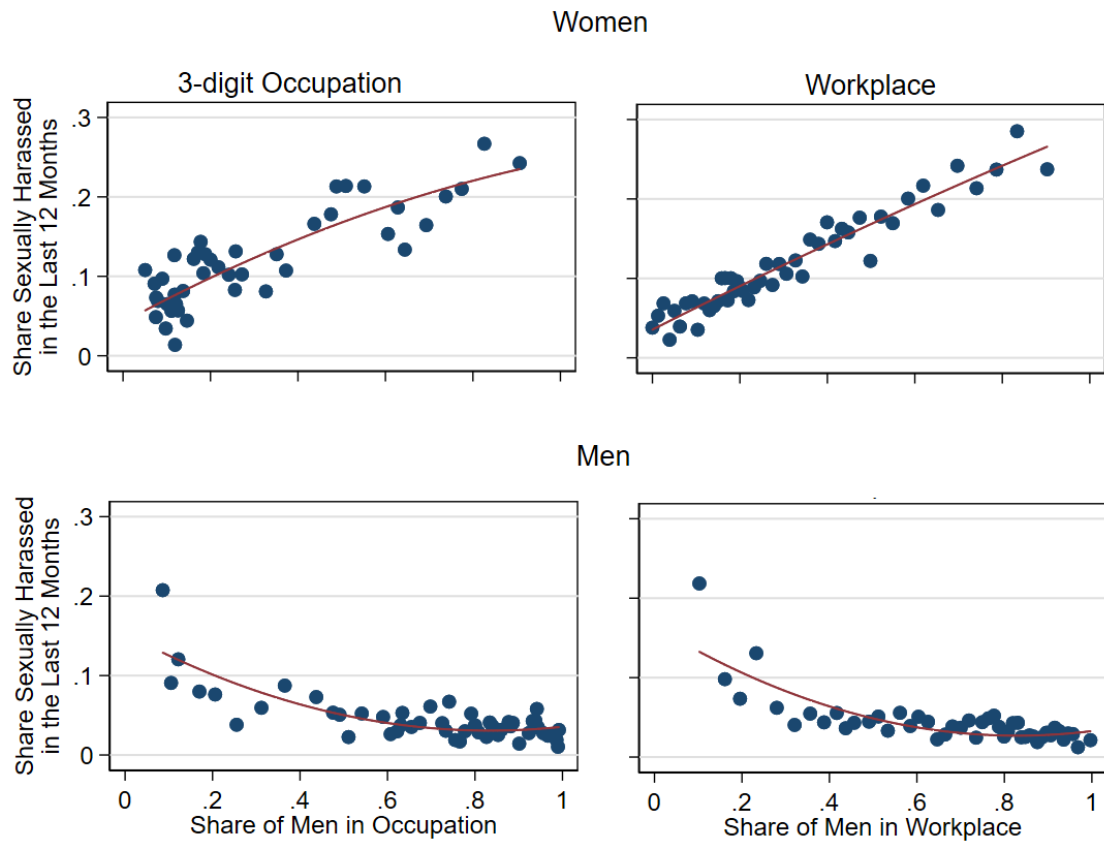


Figure 2: Sexual Harassment across Occupations and Workplaces.

Notes: The figure shows binned averages of a binary variable for self-reports of sexual harassment in the last 12 months from colleagues or managers. The data consists of five pooled cross-sections of the Work Environment Survey. The share of men in the occupation is computed at the 3-digit level and using administrative data for the full population and for each year of the survey. The workplace is defined as a unique combination of organization and plant ID codes. We define a survey respondent's workplace as people in the administrative data who had the same workplace as their largest source of labor income in the year. We then exclude the survey respondent when calculating the share of men in the workplace. Each graph shows averages of the outcome variable in 50 equally sized bins of the X-variables.

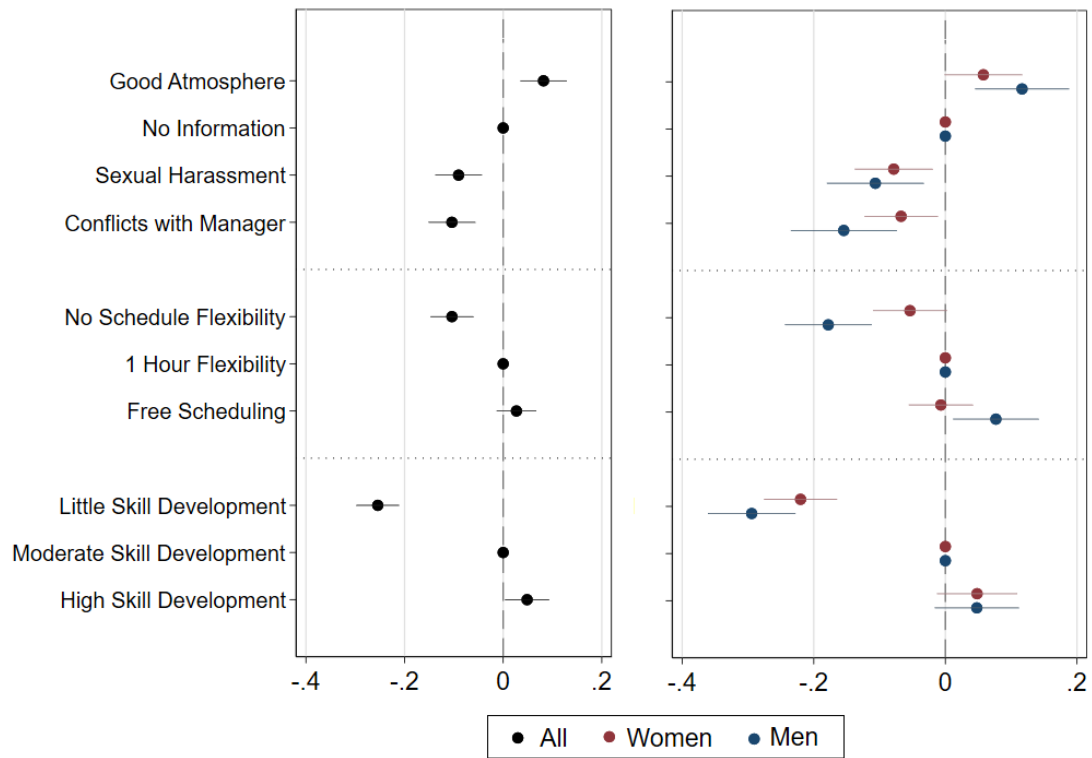


Figure 3: Estimates of Willingness-to-Pay from Job Choice Experiments.

Notes: 95% confidence intervals adjusted for clustering by respondent. Each respondent provided responses to 3 stated-preference experiments. WTP estimates are expressed as a percentage of respondents' wage. Results are weighted using population weights; unweighted counts in the stated-preference estimates are N=14,516 in the full sample, whereof 5,978 for women and N=8,538 for men.



Figure 4: Estimates of Willingness-to-Pay among Bystanders and Potential Victims.

Notes: 95% confidence intervals adjusted for clustering by respondent. Each respondent provided responses to 3 stated-preference experiments. Willingness-to-pay estimates from stated-preference experiments are expressed as a percent of respondents' wage. All results are weighted using population weights. Unweighted counts in the stated-preference estimates are N=10,050 for the pooled sample of bystanders, and N=4,808 for the pooled sample of potential victims. The sample split based on awareness is based on a question about the level of information that respondents have about incidents of sexual harassment in "specific firms in their industry". We define a person as not aware if they have "no information" (36% of respondents), and aware if they know of "some cases" (49%) or "multiple cases" (15%).

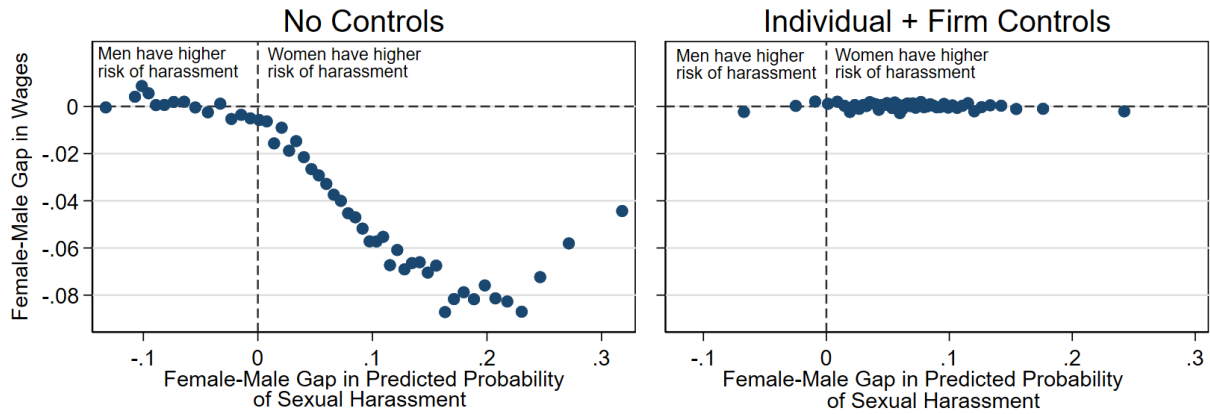


Figure 5: Gender Gaps in Wages and Sexual Harassment Risks in the Workplace.

Notes: The figure plots the binned averages of workplace difference between the monthly log wages of women compared to men, and in full-time equivalents. The horizontal axes show the workplace difference in the average of the predicted risks of sexual harassment for women minus the risk for men in the workplace. The sample is divided into 50 equally sized bins. The risk of sexual harassment is predicted based on 3-digit occupation and the share of men in the workplace; and the wage gap is calculated conditional on the occupational and age structure of the workplace. In the right-hand side graph, individual controls are fixed effects from an AKM model, estimated in the full labor force, and fixed effects for the number of years of workplace tenure. Firm controls are fixed effects for 5-digit industries and linear and squared variables for firm size. The data comes from the working-age population in the Swedish Salary Statistics in 1999, 2001, 2003, 2005 and 2007 (see Section 4 for more details).

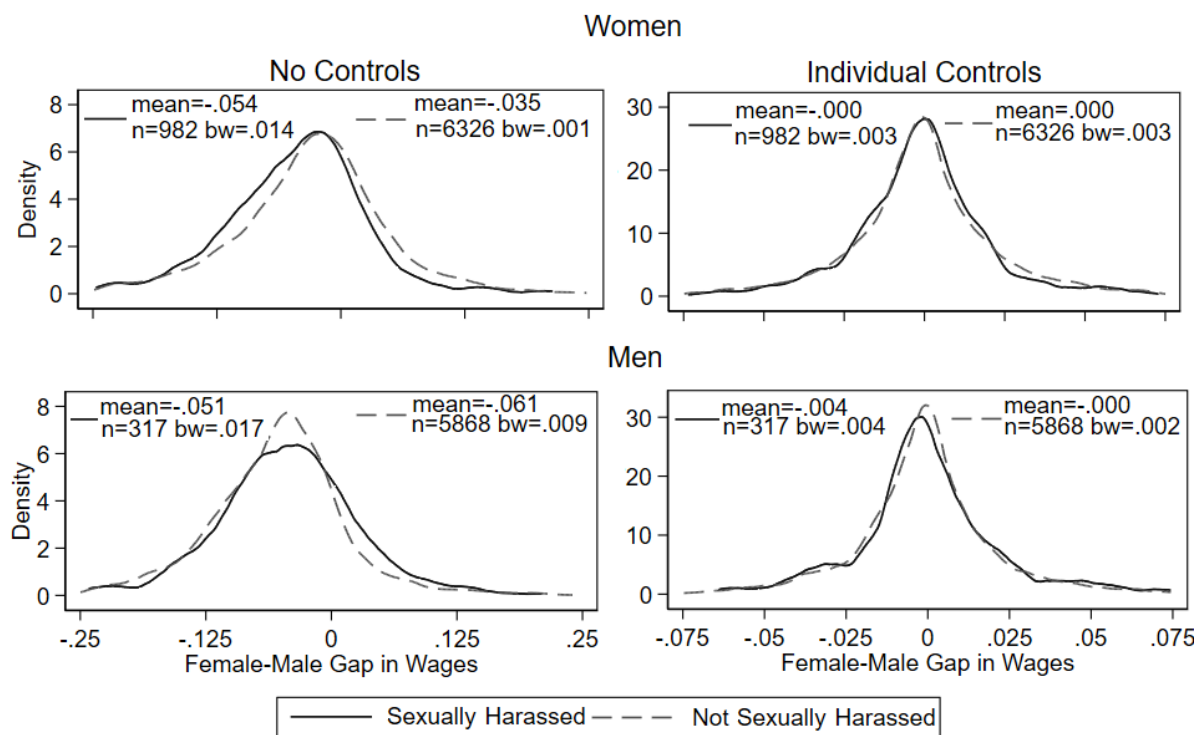


Figure 6: Distributions of Wage Gaps in Workplaces with or Without Sexual Harassment.

Notes: The figure plots the distribution firm gender wage gaps in the workplaces of respondents in the Work Environment Survey. The wage-gap is calculated as the average difference in the log of the monthly wage between men and women with the same 3-digit occupation and 5-year age group. Estimates are calculated using the Epanechnikov kernel and bandwidths and sample sizes are given in the figure legend. Solid lines show the distribution of wage gaps for survey respondents who self-reported sexual harassment, and dashed lines for those who did not. Data is from five pooled cross-sections of the Swedish Work Environment Survey, and peers are matched in the same year from administrative data from the working-age population in the Swedish Salary Statistics (see section 3 for more details).

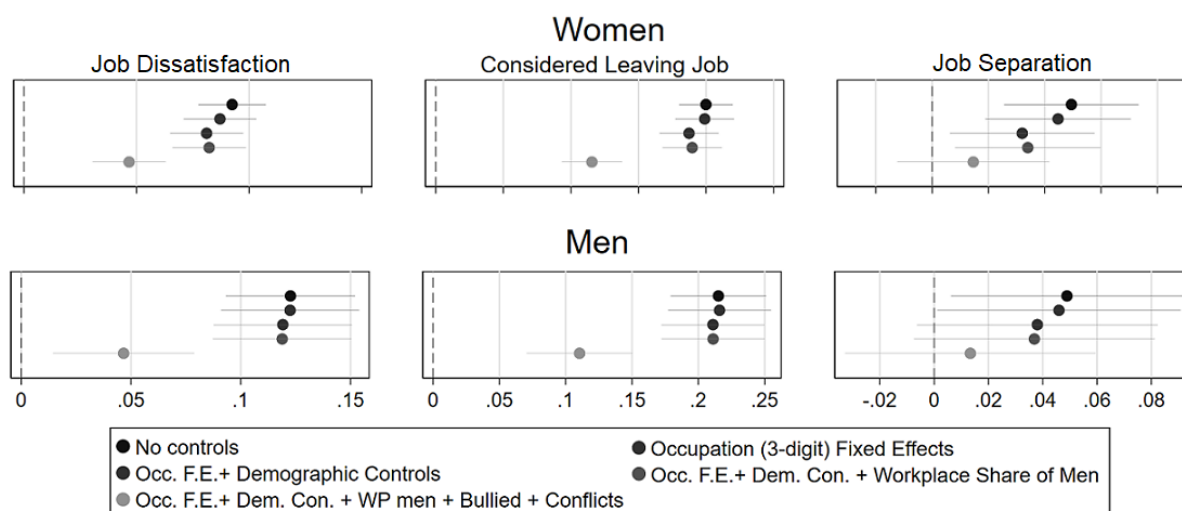


Figure 7: Differences in Job Dissatisfaction, Quit Intentions and Turnover between People with and without Self-Reported Sexual Harassment.

Notes: The figure shows estimates from OLS regressions with three outcome variables, listed in the figure titles, on a binary indicator for self-reported sexual harassment in the last 12 months from colleagues or managers. Job Dissatisfaction is a dummy variable for “agreeing somewhat” or “agreeing completely” with the statement of being unsatisfied with one’s job. Considered Leaving Job is a dummy for having considered leaving one’s job in the last 12 months due to health reasons. Job Separation takes the value 1 if the person is no longer in the same workplace three years after answering the survey. Persons older than 60 and who were not employed in the workplace the year before taking the survey are excluded from the regression for Job Separation. The data is five pooled cross-sections of the Work Environment Survey (1999, 2001, 2003, 2005, and 2007), with control variables linked to the respondent via ID codes for the individual, firm, and plant. Demographic control variables are dummies for age brackets: i) under 24 or younger, ii) 25 to 39, and iii) 40 or older; dummies for education level: i) no tertiary education, ii) tertiary education; and birth regions: i) Sweden, ii) Europe (excluding Sweden), and iii) outside of Europe. Bullying and Conflicts are dummy variables for self-reporting either of these in the last 12 years in the Work Environment Survey.

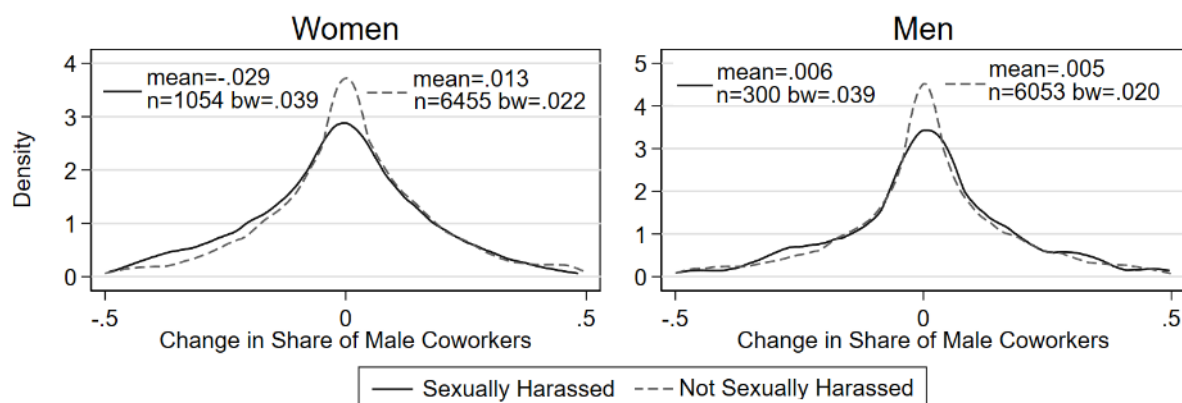


Figure 8: Pre-Post Changes in the Workplace Share of Men in Job Switches.

Notes: The figure plots the distribution of changes in the share of male co-workers for people who switch jobs within three years of answering the Work Environment Survey. In each graph, the solid line is the distribution of changes for respondents who self-reported sexual harassment, and the dashed lines is the distribution for respondents who did not self-report harassment. The note to figure 7 gives the definition of job switches. Estimates are calculated using the Epanechnikov kernel, and bandwidths and sample sizes are shown in the figure legend. The data is five pooled cross-sections of the Work Environment Survey, linked with administrative data via mandatory ID codes. The share of male co-workers is computed among people who have the same organizational and plant ID codes as the survey respondent.

Web Appendix Tables and Figures

Sexual Harassment and Gender Inequality in the Labor Market

Olle Folke and Johanna Rickne

Table W1: List of Behaviors in Categories of Sexual Harassment.

Sexist hostility (insulting, degrading, or contemptuous attitudes about women)

- Treated you differently because of your sex?
- Displayed, used, or distributed sexist or sexually suggestive materials?
- Made offensive sexist remarks?
- Put you down or was condescending to you because of your sex?

Sexual hostility (sexual and obviously hostile behaviors)

- Repeatedly told sexual stories or jokes that were offensive to you?
- Whistled, called, or hooted at you in a sexual way?*
- Made unwelcome attempts to draw you into a discussion of sexual matters?
- Made crude and offensive sexual remarks, either publicly or to you privately?*
- Made offensive remarks about your appearance, body or sexual activities?
- Made gestures or used body language of a sexual nature which embarrassed or offended you?
- Exposed themselves physically in a way that embarrassed you or made you feel uncomfortable?*

Unwanted sexual attention

- Made attempts to establish a romantic sexual relationship with you despite your efforts to discourage it?
- Stared, leered, or ogled you in a way that made you feel uncomfortable?*
- Continued to ask you for dates, drinks, dinner, etc., even though you said “No”?
- Touched you in a way that made you feel uncomfortable?
- Made unwanted attempts to stroke, fondle, or kiss you?
- Attempted to have sex with you without your consent or against your will, but was unsuccessful?*
- Had sex with you without your consent or against your will?*

Sexual coercion (unwanted sexual attention is combined with various job-related pressures)

- Made you feel like you were being bribed with some sort of reward or special treatment to engage in sexual behavior?
- Made you feel threatened with some sort of retaliation for not being sexually cooperative?
- Treated you badly for refusing to have sex?
- Implied faster promotions or better treatment if you were sexually cooperative?
- Made you afraid you would be treated poorly if you didn’t cooperate sexually?*

Source: Behaviors are drawn from the Sexual Experiences Questionnaire, see Louise F. Fitzgerald, Vicki J. Magley, Fritz Drasgow, and Craig R. Waldo, “Measuring Sexual Harassment in the Military: The Sexual Experiences Questionnaire (SEQ–DoD),” *Military Psychology* 11 (3) (1999): 243–263. The behaviors marked with (*) are not included in the revised and abbreviated, 2002 version (Stark et al. 2002).

Table W2: Details for the Swedish Work Environment Survey.

Year	Sample	Respondents in the telephone survey	Respondents in the written survey (response rate %)	Non-missing values on sexual harassment	(3) + Workplace with 5 employees or more	(4) + Non-missing wage
	(1)	(2)	(3)	(4)	(5)	(6)
1999	14,642	12,546	9,792 (67%)	9,523	8,566	4,887
2001	14,809	12,911	9,635 (65%)	9,334	8,402	4,861
2003	14,779	12,372	9,232 (62%)	8,965	8,042	4,676
2005	14,787	13,541	9,696 (58%)	9,596	8,587	5,124
2007	14,523	10,680	7,728 (62%)	7,623	6,869	4,061
Total	73,540	62,050	46,083 (63%)	45,041	40,466	23,609

Notes: The table shows the number of responses for each wave of the Swedish Work Environment Survey. It also shows the number of non-missing responses for the questions on sexual harassment and the reduction in sample size from limiting the sample to workplaces with 5 or more employees (column 3) and after removing observations with missing data for the monthly wage (column 4).

Table W3: Summary Statistics for the Employed Population and Survey Samples.

	Employed Adult Population			Work Environment Survey Sample			Survey Experiment Sample		
	All (1)	Women (2)	Men (3)	All (4)	Women (5)	Men (6)	All (7)	Women (8)	Men (9)
Tertiary Education	0.357	0.401	0.316	0.356	0.386	0.322	0.782	0.837	0.743
Age									
16—30	0.194	0.194	0.194	0.162	0.166	0.160	0.042	0.04	0.46
30—39	0.234	0.231	0.236	0.239	0.229	0.250	0.205	0.183	0.236
40—49	0.244	0.248	0.240	0.2659	0.268	0.248	0.277	0.282	0.269
50—59	0.224	0.229	0.220	0.262	0.263	0.261	0.289	0.287	0.292
60+	0.104	0.110	0.098	0.077	0.073	0.082	0.168	0.182	0.147
Woman	0.479			0.535			0.412		
Monthly Income									
—15 999	0.249	0.310	0.192	0.218	0.290	0.135	0.033	0.044	0.024
16 000—29 999	0.507	0.542	0.475	0.585	0.605	0.563	0.193	0.228	0.169
30 000 +	0.244	0.148	0.333	0.197	0.105	0.302	0.774	0.729	0.807
Region of Birth									
Sweden	0.878	0.877	0.879	0.923	0.921	0.927	0.928	-	-
Europe, excl. Sw.	0.074	0.077	0.071	0.057	0.060	0.053	0.037	-	-
Non-Europe	0.050	0.047	0.047	0.020	0.019	0.020	0.025	-	-

Notes: This table compares demographic traits in three datasets. Columns 1—3 use data for all employed Swedish citizens in the 16—65 age range, pooling yearly cross sections for 1999, 2001, 2003, 2005 and 2007. Columns 4—6 uses pooled cross-sectional datasets for the same five years of the Work Environment Survey. This sample is restricted to people who responded to two survey questions on sexual harassment and who are employed in workplaces with at least 5 employees. Columns 7—9 use data from an internet sample of employed Swedish citizens in 2019.

Table W4: Summary Statistics by Sex and Self-reported Sexual Harassment.

Sexual Harassment:		Women		Men	
		No	Yes	No	Yes
Age Bracket	18—34	0.31	0.43	0.34	0.43
	35—50	0.38	0.37	0.37	0.34
	51—65	0.31	0.20	0.29	0.23
Birth Region	Sweden	0.92	0.92	0.93	0.88
	EU28, excl. Sweden	0.06	0.06	0.05	0.06
	Outside EU28	0.02	0.02	0.02	0.06
Education Level	High School or Less	0.64	0.52	0.70	0.62
	At Least Some College	0.36	0.48	0.30	0.38
New Child in Survey Year		0.019	0.015	0.038	0.039
Self-stated Productivity		8.96	8.91	8.98	8.44
Unease of Going to Work		0.40	0.63	0.43	0.71
Bullying in the Last 12 Months		0.06	0.28	0.08	0.45
Conflict with Colleagues in the Last 12 Months		0.30	0.57	0.36	0.70
Conflict with Supervisor in the Last 12 Months		0.22	0.51	0.34	0.65
		N=21,209	N=2,801	N=18,828	N=848

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 which is linked to survey respondents via a mandatory personal ID code. (a) self-stated productivity is measured on a scale from 1—10 on 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?*”. (b) Unease of going to work is a dummy variable for feeling unease “a couple of days per month” or more often.

Table W5: Top 5 Highest Rates of Sexual Harassment in 3-Digit Occupations.

<i>Men</i>		<i>Women</i>	
3-Digit Occupation	Rate	3-Digit Occupation	Rate
A. All Sexual Harassment			
Pre-primary education teaching associate professionals	0.200	Motor-vehicle drivers	0.404
Nursing associate professionals	0.169	Architects, engineers and related professionals	0.350
Personal care and related workers	0.166	Writers and creative or performing artists	0.311
Psychologists, social work and related professionals	0.141	College, university and higher education teaching professionals	0.277
Doorkeepers, newspaper and package deliverers and related workers	0.134	Physical and engineering science technicians	0.244
B. Sexist Hostility			
Pre-primary education teaching associate professionals	0.200	Motor-vehicle drivers	0.351
Personal care and related workers	0.165	Architects, engineers and related professionals	0.333
Nursing associate professionals	0.153	Writers and creative or performing artists	0.304
Psychologists, social work and related professionals	0.141	College, university and higher education teaching professionals	0.266
Client information clerks	0.115	Physical and engineering science technicians	0.235
C. Sexual Hostility and Unwanted Sexual Attention			
Personal care and related workers	0.073	Library and filing clerks	0.098
Chemical-processing-plant operators	0.068	Motor-vehicle drivers	0.088
Nursing associate professionals	0.051	College, university and higher education teaching professionals	0.069
Doorkeepers, newspaper and package deliverers and related workers	0.045	Architects, engineers and related professionals	0.068
Chemical-products machine operators	0.364	Manufacturing labourers	0.062

Notes: The table shows rates of self-reported sexual harassment in the last 12 months from colleagues or managers in 3-digit occupations. The top panel (A) shows the 5 occupations for men and women with the highest rates of harassment for the combined harassment variable (see Section 3 for details). The bottom panels (B and C) show rates for the two separate survey questions that together make up the combined variable. The data is five pooled cross-sections of the Swedish Work Environment Survey (1999, 2001, 2003, 2005, and 2007), restricted to people who answered both questions about sexual harassment and who are employed in workplaces with 5 or more people.

Table W6: Sex Ratios and Sexual Harassment against Women.

	(1)	(2)	(3)	(4)
A. Women				
Share of men in 3-digit occupation	0.22*** (0.03)		0.11*** (0.01)	0.09*** (0.01)
Share of men in workplace		0.25*** (0.01)	0.20*** (0.01)	0.21*** (0.01)
Observations	18,256	19,962	17,044	17,019
B. Men				
Share of men in 3-digit occupation	-0.08*** (0.01)		-0.06*** (0.01)	-0.05*** (0.01)
Share of men in workplace		-0.07*** (0.01)	-0.04*** (0.01)	-0.05*** (0.01)
Observations	14,474	17,483	13,722	13,691
Demographic controls				x
Supervisor dummy				x

Notes: The table shows coefficients from regressions of a dummy variable for sexual harassment in the last 12 months on two continuous variables for the sex-ratio of 3-digit occupations, and workplaces. The data is pooled cross-sections of biannual cross-sectional survey data in 1999—2007. Standard errors are shown in parenthesis and are clustered at the 3-digit occupation level in regressions that include this variable. Demographic control variables are dummies for age brackets: i) under 24 or younger, ii) 25 to 39, and iii) 40 or older; and dummies for birth regions as i) Sweden, ii) Europe (excluding Sweden), and iii) outside of Europe. Respondents are defined as supervisors if they reported that at least some part of their job involved “leading or delegating work for other employees”. *** p<0.01, ** p<0.05, * p<0.1.

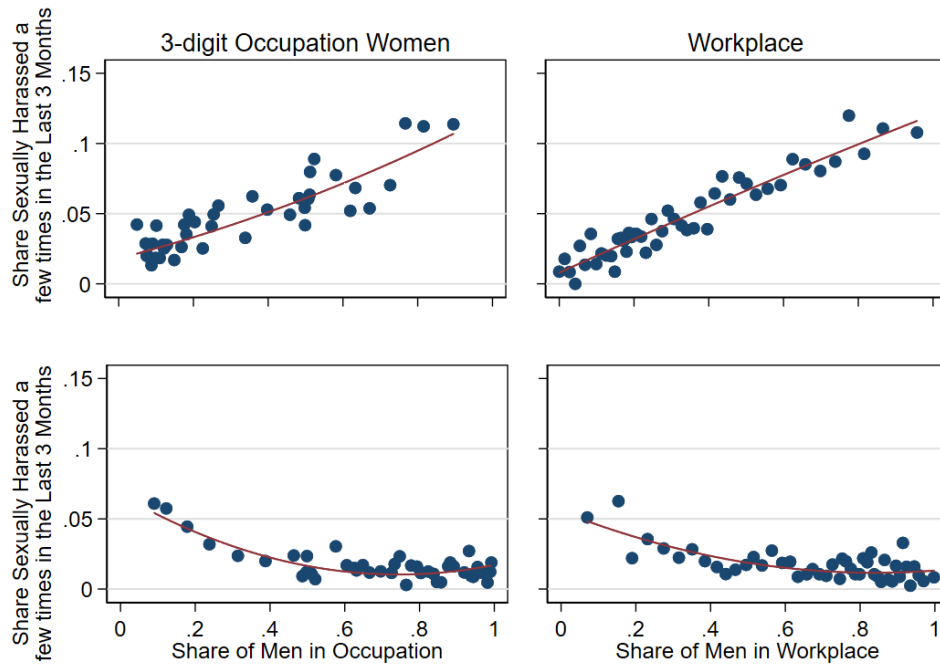
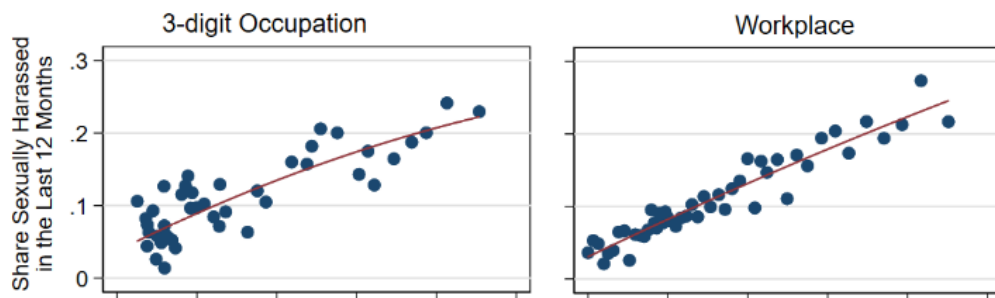


Figure W1: Higher-Frequency Sexual Harassment across Occupations and Workplaces

Notes: The figure replicates Figure 2 but measures sexual harassment as a dummy for self-reported victimization at least “a couple of in the last three months”. Further details in the note to Figure 2.

Women



Men

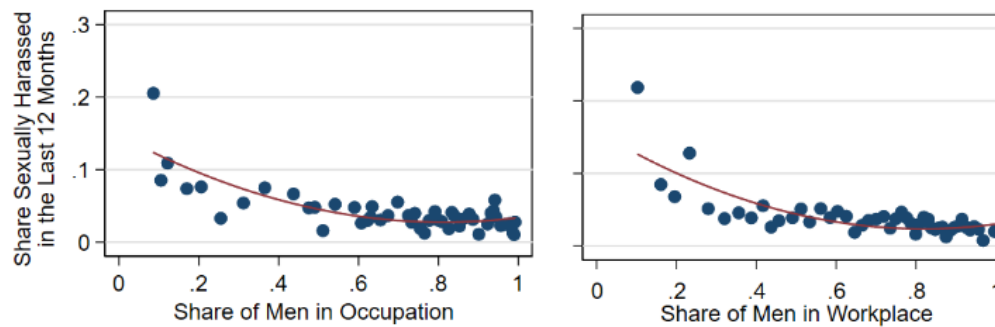


Figure W2: Sexist Hostility across Occupation and Workplaces.

Notes: The figure replicates Figure 2 but measures sexual harassment as a dummy variable for sexist hostility, only. Further details in the note to Figure 2.

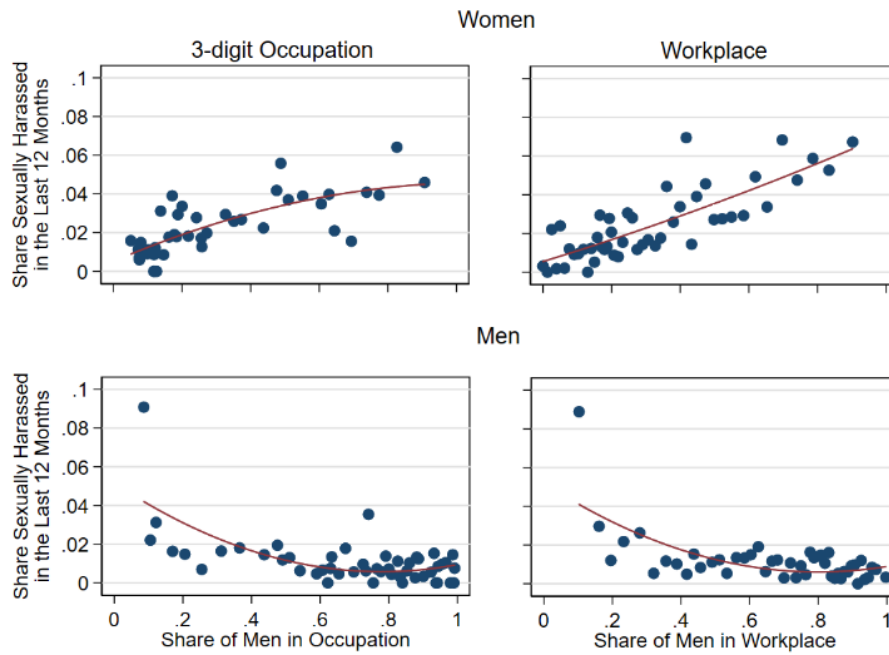


Figure W3: Sexual Hostility and Unwanted Sexual Attention across Occupations and Workplaces.

Notes: The figure replicates Figure 2 but measuring sexual harassment as a combined dummy variable for sexual hostility and unwanted sexual attention, only. Further details in the note to Figure 2.



Figure W4: Sexual Harassment across Occupations and Workplaces, Sample Split by the Sex of the Respondent's Closest Supervisor.

Notes: The figure replicates Figure 2 but splits the sample by respondents who say, in the Work Environment Survey, that they have a man or a woman as their closest supervisor.



Figure W5: Sexual Harassment across Occupations and Workplaces, Sample Split by being an Employee or Supervisor.

Notes: The figure replicates Figure 2 but splits the sample by respondents who say, in the Work Environment Survey, that at least some part of their job involved “leading or delegating work for other employees”.

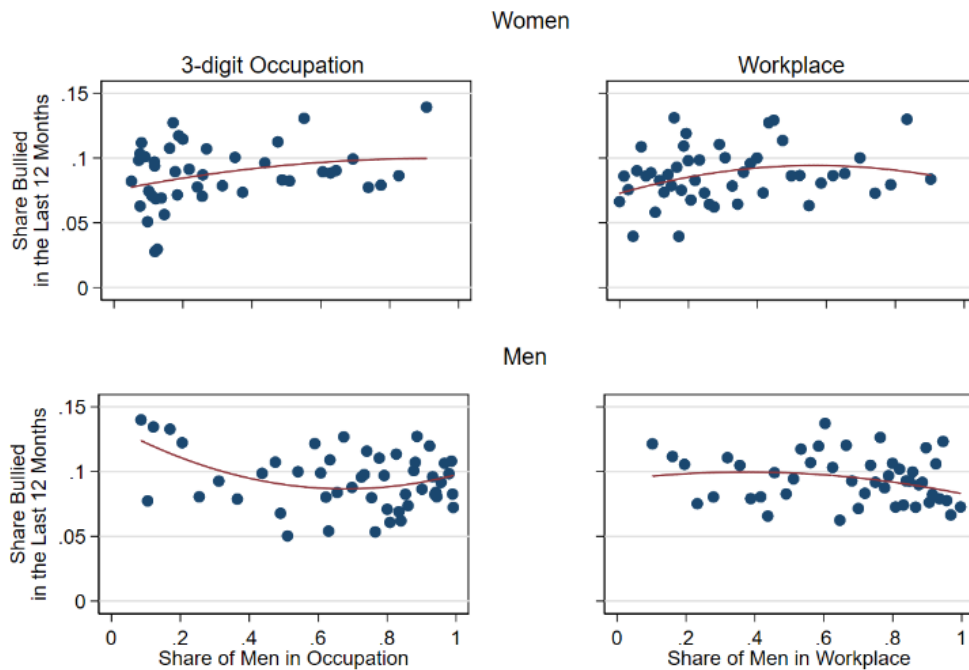


Figure W6: Bullying across Occupations and Workplaces.

Notes: The figure replicates Figure 2 but for binned averages for a dummy variable of self-reported bullying in the last 12 months. Further details in the note to Figure 2.

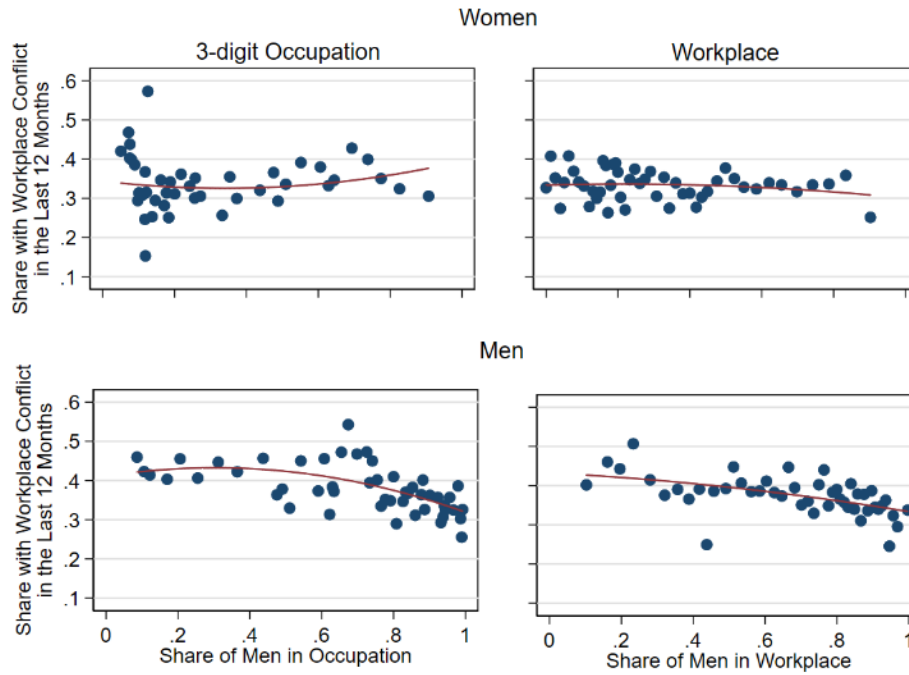


Figure W7: Workplace Conflicts across Occupations and Workplaces.

Notes: The figure replicates Figure 2 but for binned averages for a dummy variable of self-reported conflicts with either colleagues or managers in the last 12 months. Further details in the note to Figure 2.

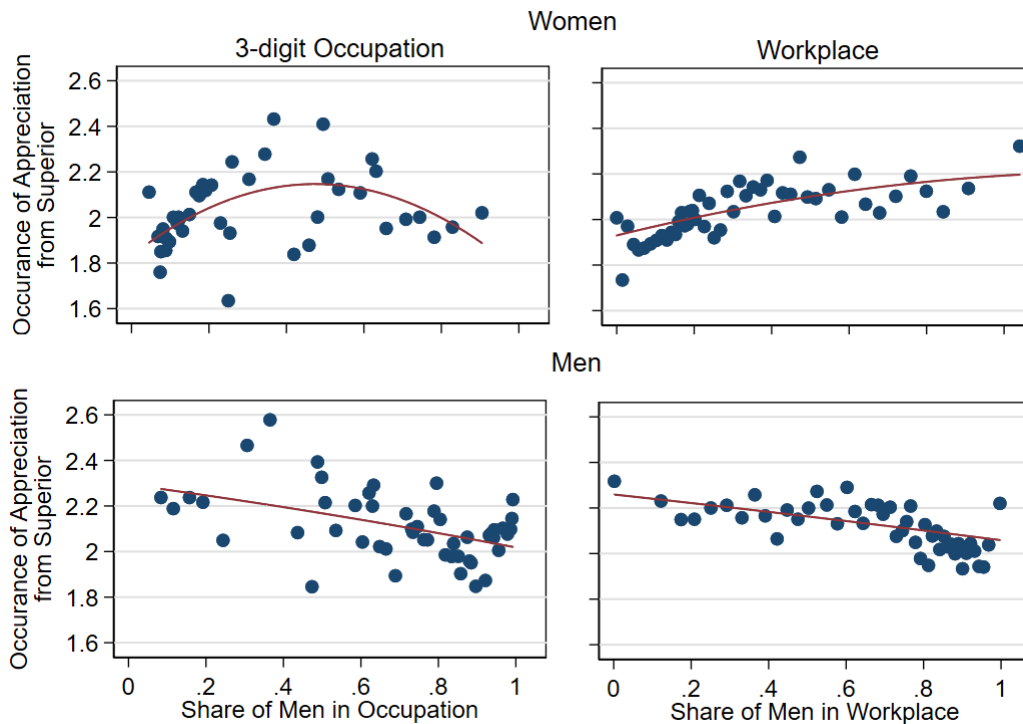


Figure W8: Appreciation from Supervisor across Occupations and Workplaces.

Notes: The figure replicates Figure 2 but for binned averages for a dummy variable of self-reported appreciation from one's manager. For more details about the data source, see Figure note 2.

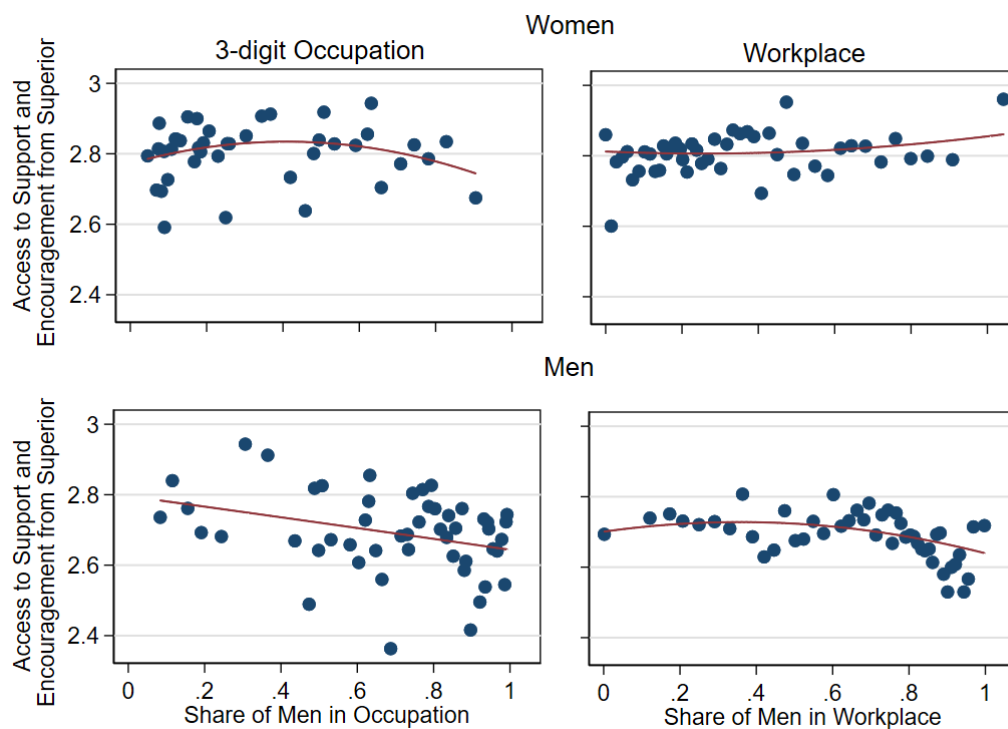


Figure W9: Encouragement and Support from Manager across Occupations and Workplaces.

Notes: The figure replicates Figure 2 but for binned averages for a dummy variable of self-reported encouragement and support from one's manager. For more details about the data source, see Figure note 2.

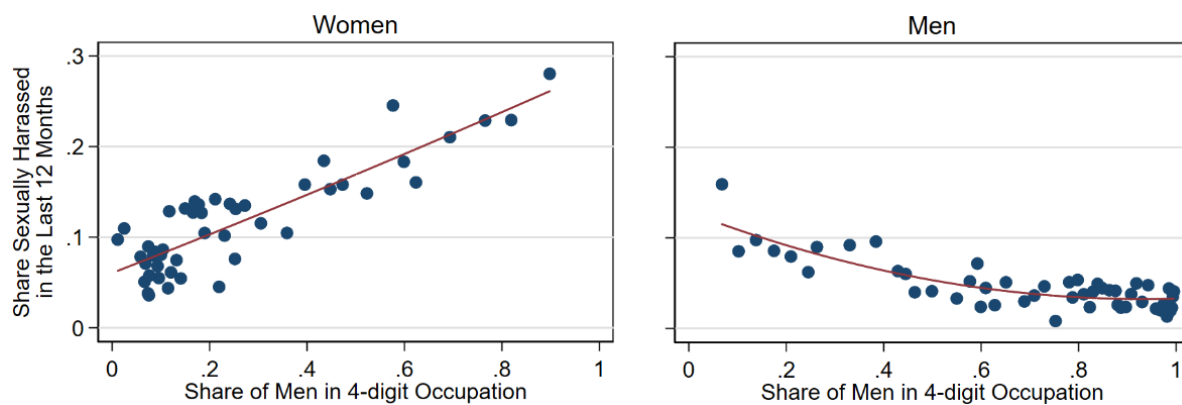


Figure W10: Sexual Harassment across 4-Digit Occupations.

Notes: The figure replicates the correlations between sexual harassment and the occupation share of men, but using 4-digit occupation codes instead of 3-digit occupation codes.

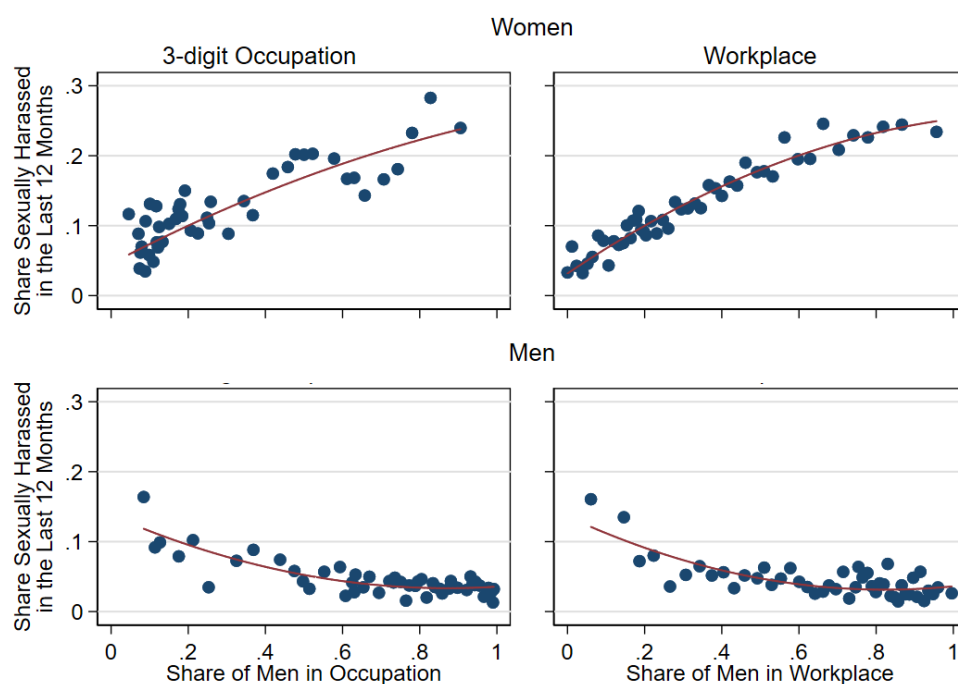


Figure W11: Sexual Harassment across Occupations and Workplaces: Alt. Weighting Scheme.

Notes: The figure replicates Figure 2 but uses a more extensive weighting scheme. In addition to the weighting variables in the main analysis, dummy variables for sex at birth, age (<30, 30—39, 40—49, 50—60, and 60+), education level (less than tertiary education, at least some tertiary education), we add dummies for monthly income (—15 999, 16 000—29 999, 30 000+) and birth region (Sweden, Europe excluding Sweden, Non-Europe).

Egenskaper	Jobb A	Jobb B
Arbetsuppgifterna	Inte så utvecklande	Utvecklande
Löneläget	Ungefär samma som du tjänar nu	5 % mer än du tjänar nu
Arbetsmiljön	En man har tafsat på en kvinna på enheten	Anställda verkar nöjda med miljön
Inflytande över arbetstiden	Inget inflytande	1h flex i början eller slutet av dagen

Figure W12: Screenshot of Hypothetical Job Choice Table in the Original, Swedish Survey.

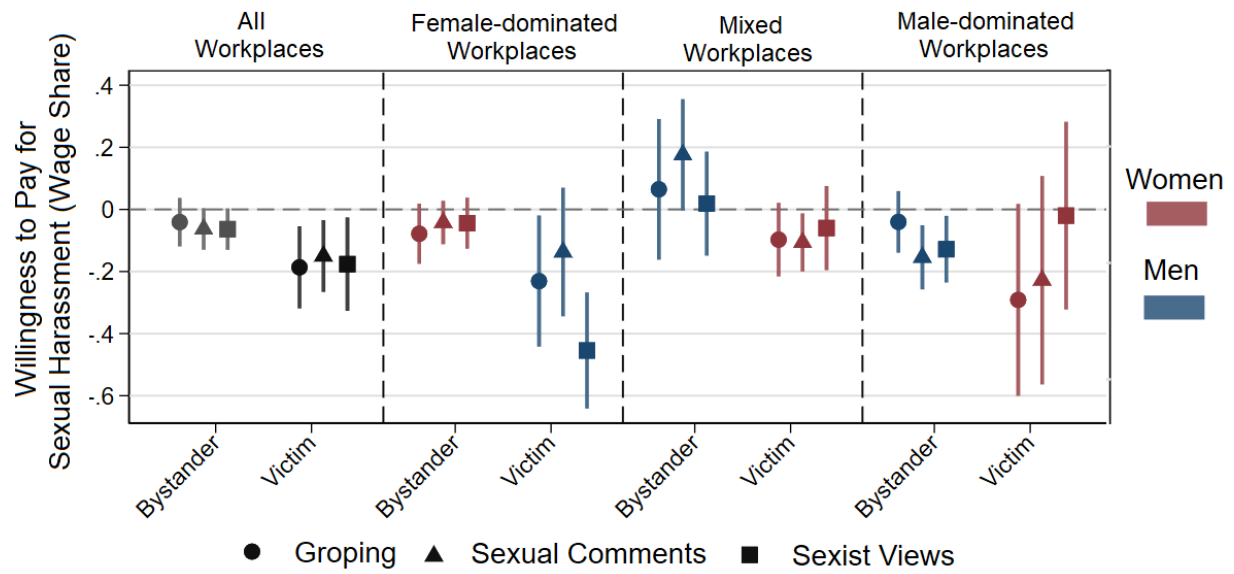


Figure W13: Willingness-to-Pay Estimates for Types of Sexual Harassment.

Notes: 95% confidence intervals adjusted for clustering by respondent. For more details, see the notes to Figure 4.

Section W1. Sensitivity Analysis for the Survey Experiment

Our WTP statistic is the ratio of preferences for wages and sexual harassment. This opens for the concern that differences in the statistic across groups come from different preferences for wages rather than different preferences for sexual harassment. Figures W14 show the AMCEs for sexual harassment and wages for the groups of men, women, bystanders and potential victims. The similar estimates for (both) wage preferences and sexual harassment preferences means that the baseline finding of similarly sized WTPs are based on similarities in both these quantities.

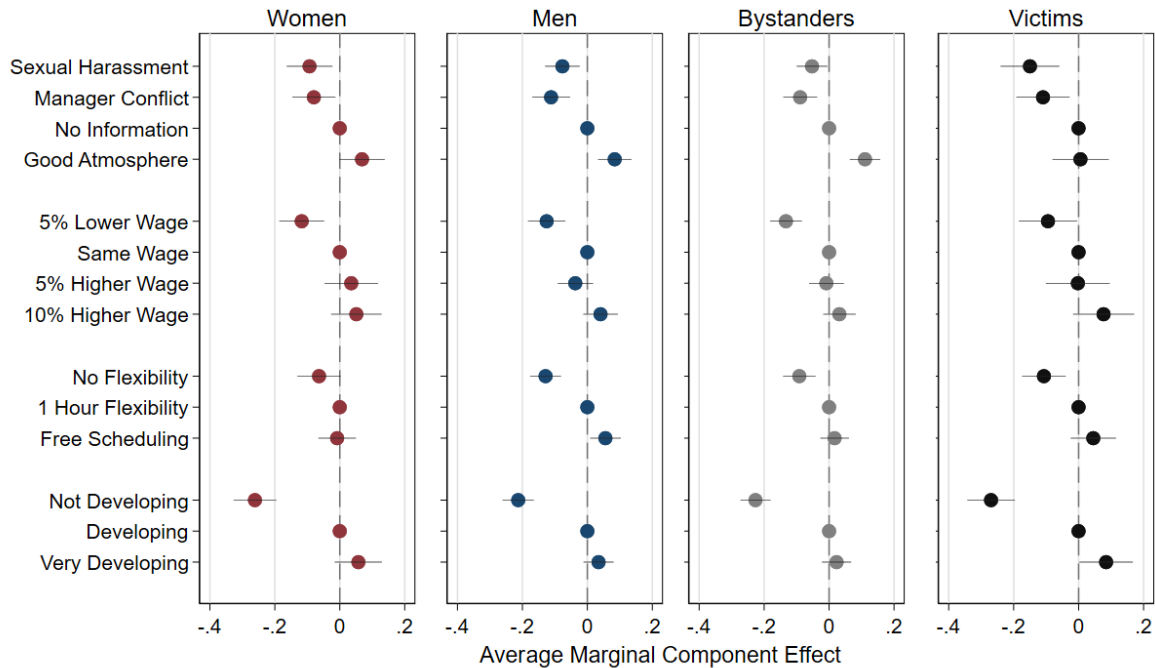


Figure W14: Full Set of Estimated AMCEs from the Stated-Preference Experiment.

Notes: The figure shows point estimates from regression equation (1) for the women (maroon), men (blue), bystanders (gray), and potential victims (black).

Figure W15 shows several variations of the bystander-victim comparison. First, we remove the weighting scheme for demographic characteristics, which leads to a drop in the size of the gap between the two groups. A closer look at the weighting scheme shows that the education variable is the most important for the shift in the estimates. Interestingly, dropping the weights produces the opposite impact on the estimates for the self-defined groups of potential victims of bystanders, i.e. the self-assessed harassment risk of taking a job in a firm where harassment has happened. In that sample, the difference between bystanders and potential victims becomes larger, not smaller (results not shown).

A common concern in survey experiments is that respondents who fail to read the questions closely will gravitate toward characteristics similar than their current job instead of carefully considering their options. We approximate the proportion of attentive respondents with people who correctly picked the “correct” job in cases where one option was strictly better than the other. These job choices, which were dropped in the analysis above, account for 40% of the raw data. In that data 90% pick the correct job. We re-estimate the WTPs among people who made a correct choice in at least one table with a dominant option, who make up roughly 53% of the full sample. The gap in WTPs between bystanders and victims is larger in the attentive sample than in the full sample, which

shows that the main result is not spuriously created by inattentive survey respondents. Using the “inattentive” respondents instead—i.e. people who saw a dominant option but picked the wrong job—also shows a gap between bystanders and potential victims, which suggests that including these respondents is not skewing the analysis. Removing the inattentive respondents from the main sample also yields highly similar WTP results as those reported in Figure 4.

Finally, we show the sensitivity to using alternative estimation methods. There is little variation in the results across using a logit, probit or mixlogit specification, demonstrating a lack of sensitivity to distributional assumption. The mixlogit analysis is calculated using the *mixlogitwtp* program that automatically calculates the WTP using a mixlogit specification.

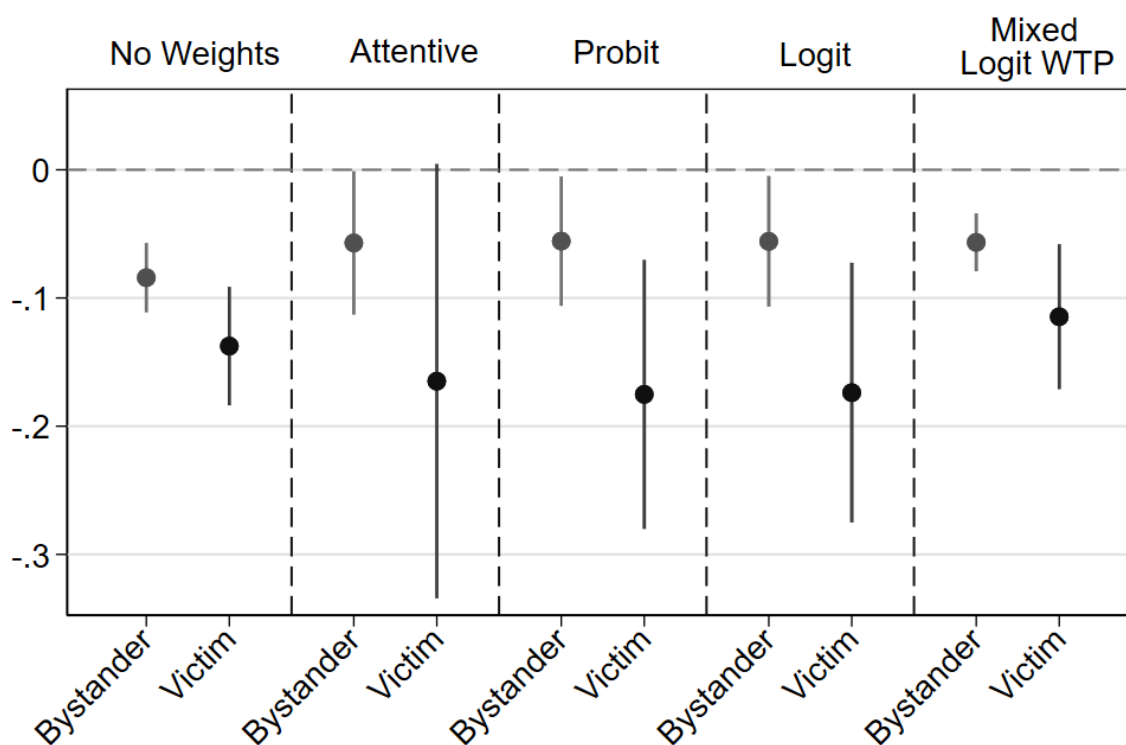


Figure W15: Sensitivity Checks for Willingness-to-Pay Estimates.

Notes: 95% confidence intervals adjusted for clustering by respondents. The text in Web Appendix Section W1 gives details for the definitions of attentive and informed respondents as well as estimation details.

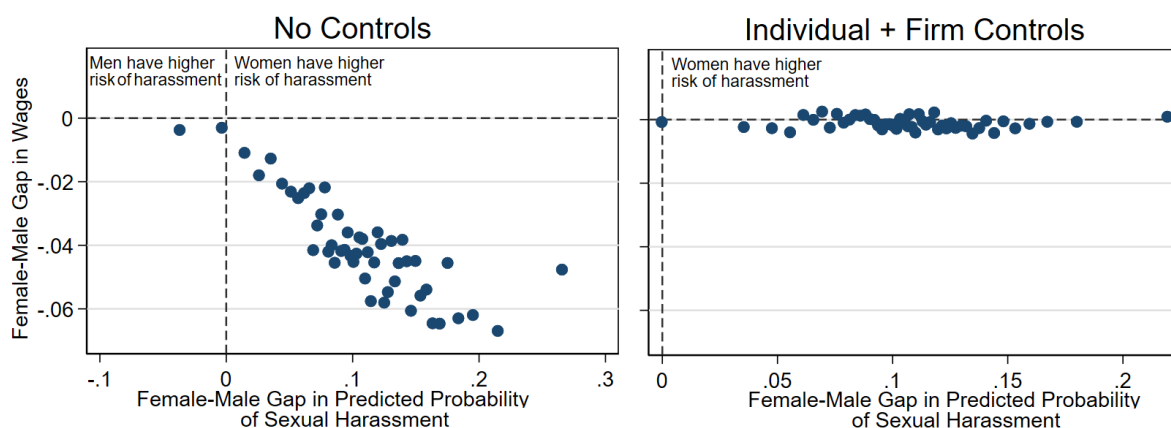


Figure W16: Gender Gaps in Wages and Sexual Harassment Risks in the Workplace, Sub-sample of Most Likely Subset of Workplaces.

Notes: The figure replicates the analysis in Figure 5 in a sub-sample of firms. Firms are included if they meet two criteria: the neither sex is below one third of the workforce, and if they have an above-median flexibility in wages from year to year. To calculate wage changes, we compare wages in each year of the pooled cross section to the wage structure in the same firm in the previous year. For people who remain employed since that previous year, we compute their wage change to the survey year. We then take the standard deviation of these changes at the workplace level. Values of the standard deviations above the median are defined as high wage flexibility.

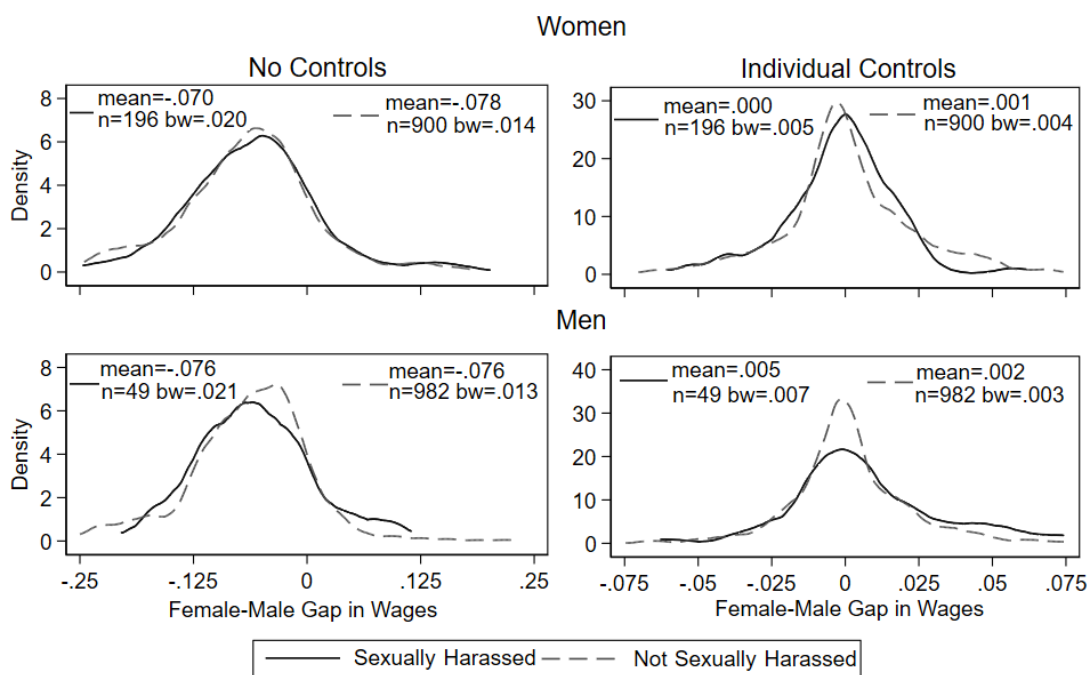


Figure W17: Distributions of Wage Gaps in Workplaces with or Without Sexual Harassment, Sub-sample of Most Likely Subset of Workplaces.

Notes: The figure replicates the analysis in Figure 5 in a sub-sample of firms. Sample restrictions are described in the notes to Figure W16.

Section W2. Two Alternative Levels of Compensating Pay

In this appendix section, we analyze two additional levels of compensating differentials. The first analysis replicates the one for the workplace-level, but compute the gaps between women and men in the risk of harassment and in wages within 3-digit occupations at the same workplace. Figure W18 replicates Figure 5 at this level of disaggregation, and Figure 19 replicates Figure 6.

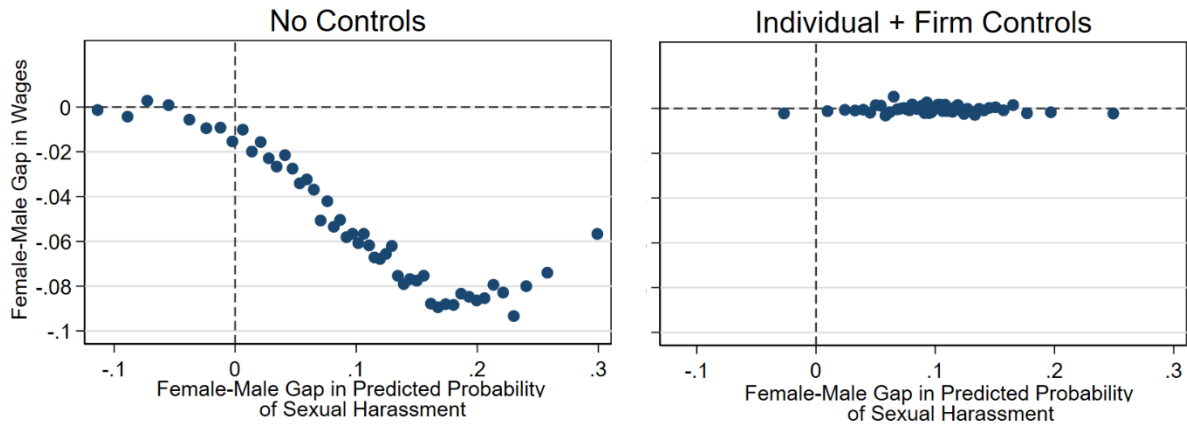


Figure W18: Gender Gaps in Wages and Sexual Harassment Risks at the Workplace-Occupation-level.

Notes: The figure replicates figure 5 at the level of the 3-digit occupation within the workplace.

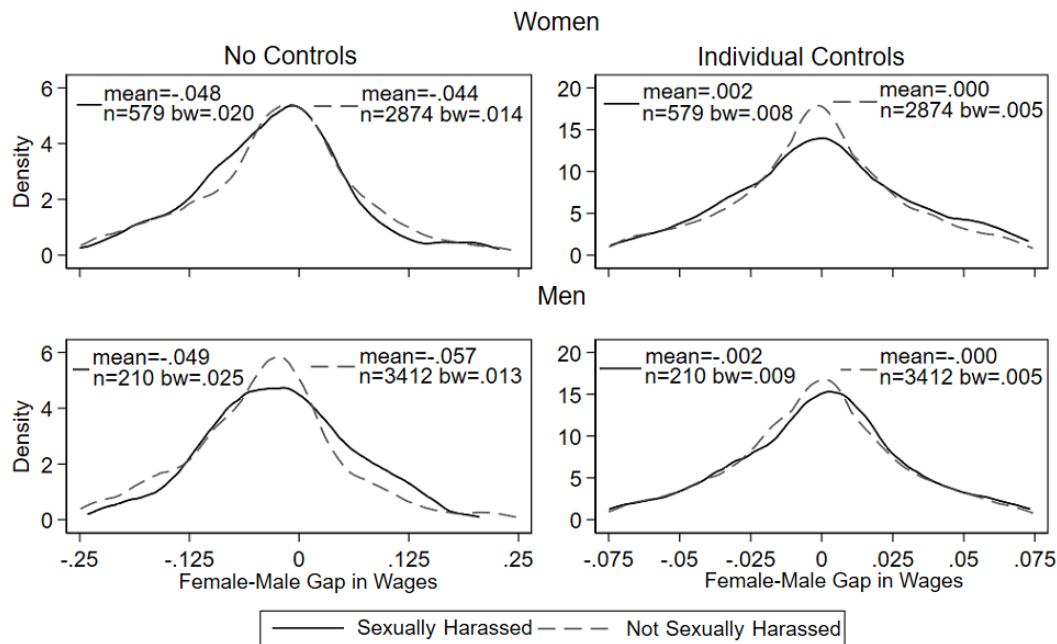


Figure W19: Distributions of Wage Gaps in Workplace-Occupation Cells with or Without Sexual Harassment.

Notes: The figure replicates figure 6 at the level of the 3-digit occupation within the workplace.

Next, we examine if there is a compensating differential at the individual level. If there is a compensating differential at this level it would imply that the individual victim of sexual harassment compensated relative to its colleagues. We examine this in Figure W20, which shows the distribution of survey respondents wage relative to colleagues in the same workplace, 3-digit occupation and 5-year age bracket. On left hand side of the figure we compare women who report harassment to those who do not and on the right-hand side we make the same comparison for men.

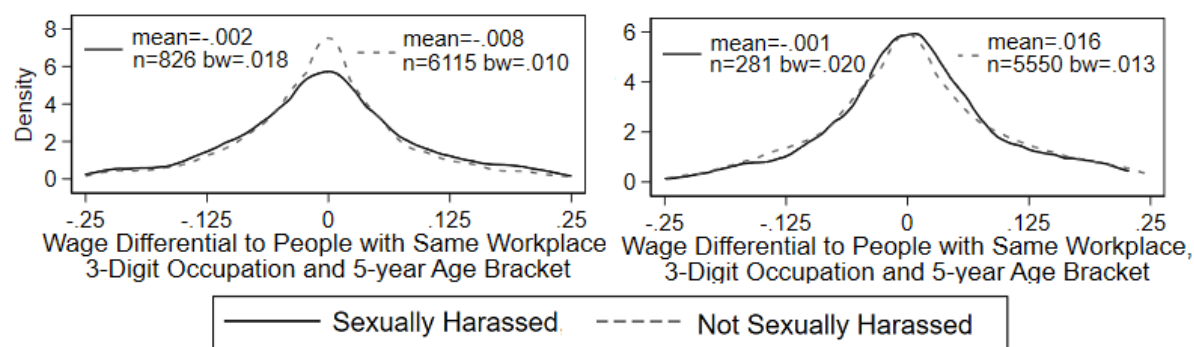


Figure W20: Distributions of Differences in Log-wages for Survey Respondents Compared to their Colleagues.

Notes: The figure plots the distribution of monthly wage differences in log points between respondents of the Work Environment Survey and two comparison groups, as detailed in the legend. In each graph, the solid line is the distribution of peer-comparisons for respondents with self-reported sexual harassment, and the dashed lines are respondents who reported to not have been harassed. Estimates are calculated using the Epanechnikov kernel and bandwidths and sample sizes are given in the figure legend. Data is from five pooled cross-sections of the Swedish Work Environment Survey, and peers are matched in the same year from administrative data from the working-age population in the Swedish Salary Statistics (see section 4 for more details).

Figure W20 shows that the distributions of the relative wage for women who report harassment is largely overlapping with the distribution for women who do not. The difference in the average of the relative wage is also negligible in size at 0.6%, and a simple regression analysis shows that it is statistically insignificant at the 5-percent level. Men who self-report harassment have a 1.6% lower wage compared to their colleagues, relative to men who do not report harassment.

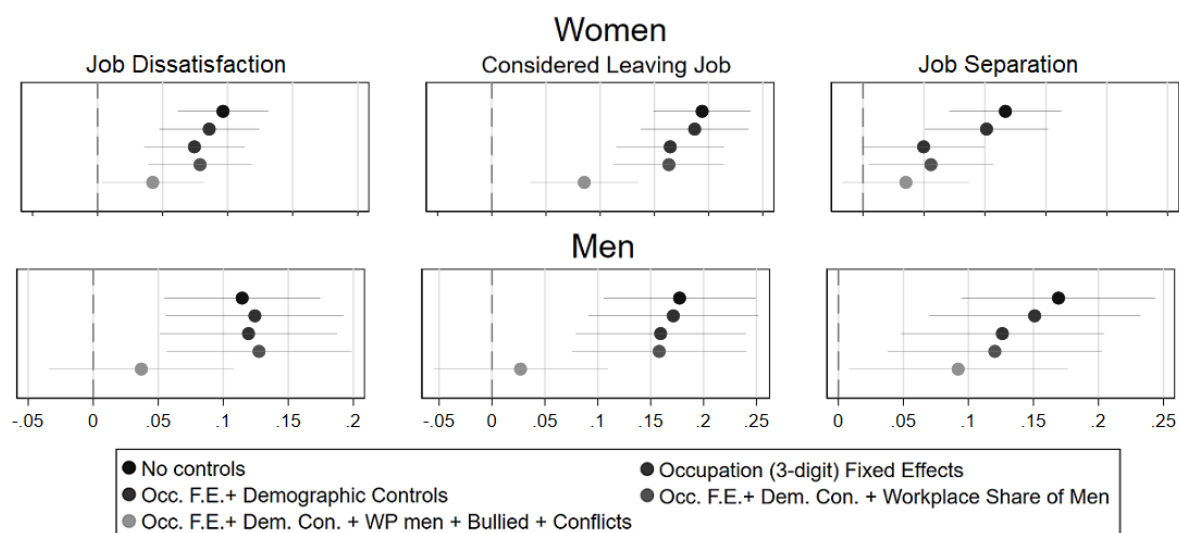


Figure W21: Differences in Job Satisfaction, Quit Intentions and Turnover between People with and without Self-Reported Sexual Hostility and/or Unwanted Sexual Attention.

Notes: The figure shows point estimates from regressions of three dummy variables on a dummy variable for having experienced Sexual Hostility and/or Unwanted Sexual Attention in the last 12 months from colleagues or managers. See the notes Figure 7 for estimation details.

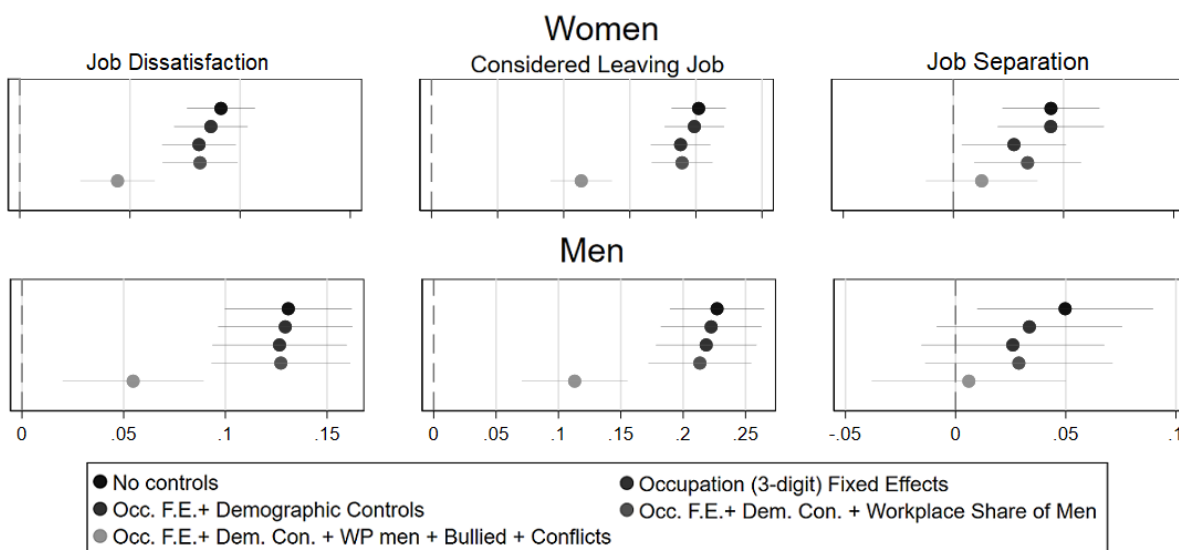


Figure W22: Differences in Job Satisfaction, Quit Intentions and Turnover between People with and without Self-Reported Sexist Hostility.

Notes: The figure shows point estimates from regressions of three dummy variables on a dummy variable for having experienced sexist hostility in the last 12 months from colleagues or managers. See the notes Figure 7 for estimation details

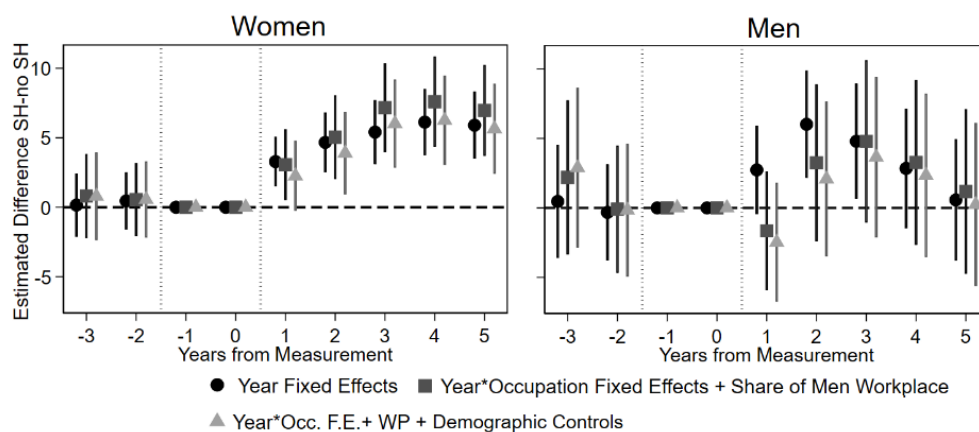


Figure W23: Dynamic Analysis of Job Separations.

Notes: The figure shows differences-in-differences estimates for job separations among people who self-reported sexual harassment in the Work Environment Survey compared to those who did not. A job-separation is defined as having a different workplace as the largest source of total annual labor income than in the survey year. The sample is restricted to people whose largest source of labor income in the survey year is the same as in the previous year. The X-axis denotes the number of years passed since the survey. An estimate of 5 means that people who self-reported sexual harassment in the survey were 5 percentage points less likely to remain in the workplace compared to people who did not self-report sexual harassment. The data is five pooled cross sections of the Work Environment Survey, matched to annual labor market data from the Longitudinal LISA administrative data via mandatory personal ID codes. Control variables are measured in administrative data and linked via these ID codes.