Safer Streets, Re-Segregated Workplaces*

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Abstract

This paper studies whether protecting women from sexual harassment brings them into the labor force. In 2014, Egypt criminalized sexual harassment both in public space and in the workplace. Using a triple difference, we compare women with men, before and after the law, across governorates with different levels of pre-law harassment. We find that women in governorates where harassment was more prevalent entered the labor force. A one standard deviation increase in pre-law harassment raises female participation by 9 percent. Women also become much more likely to work in a governorate with more harassment than their own, and the distance between the governorate where they live and the governorate where they work increases by four kilometers. However, they move into occupations that involve less interaction with others, that is, less work with a group or team, less communication with supervisors, peers and subordinates, and less contact with others. This pattern is consistent with the law raising the cost for employers of mixing men and women at work. Using the firm survey, we find that establishments reduce their female employment share in industries where men and women work together.

JEL classification: J16, J22, J71, K38, O15.

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1 Introduction

Few development objectives have absorbed as much effort, for as little movement, as raising women's participation in the labor market. About one in two adult women participates in the labor force, against three in four men (International Labour Organization, 2026). Across low- and middle-income countries governments have spent decades on the problem: conditional cash transfers, childcare provision, microfinance, quotas, vocational training. While economists have focused on the role of social norms (Bursztyn, González, and Yanagizawa-Drott, 2020; Jayachandran, 2021) and on bargaining within the household (Field, Pande, Rigol, Schaner, and Troyer Moore, 2021), little is known about the role of safety in shaping women's labor supply.

Across the world, women face a constraint that men largely do not face. Sexual harassment is pervasive both in public space and in the workplace. More than half of women report having experienced street harassment in their lifetime (Amaral, Borker, Fiala, Kumar, Prakash, and Sviatschi, 2025), and about half are harassed at some point in their working lives (Folke and Rickne, 2022). To avoid it, women take costly precautions, such as keeping away from entire areas of their cities (Amaral et al., 2025), giving up college quality for safer routes (Borker, forthcoming) or staying out of work (Beaman, Jayachandran, and Sharma, 2026). Governments have responded, mostly within the past three decades, and 153 of 190 economies now have legislation against sexual harassment (World Bank, 2024). But does criminalizing sexual harassment bring women into the labor force? The answer is not obvious, as the law can affect women's work through three channels. First, safer streets could draw women into work outside the home (the *safer-commuting channel*). Second, a safer workplace makes it less costly for women to work with men (the *safer-interaction channel*). Third, legislation of this kind also reaches inside the workplace, and it exposes supervisors and firms to criminal and reputational risk, which raises the cost of employing women in jobs where women and men work together (the *employer-cost channel*). The last two channels have opposite effects on the jobs women hold.

To shed light on this question, we study how women's labor-force participation and the jobs they hold responded to the national criminalization of sexual harassment in Egypt in 2014. In June 2014, Egypt made sexual harassment a crime, in the street and in the workplace, under Articles 306-bis A and 306-bis B of the Penal Code, punishable by prison and a fine. Egypt is a particularly relevant setting for this question, since female participation is among the lowest in the world, with 18.5% of women in the labor force against 71% of men (International Labour Organization, 2026). In addition, harassment is widespread and varies a lot across the country. Among women aged 10–29, 45% report having experienced it, and prevalence ranges from 10% to 76% across governorates.

To exploit the local variation in how strongly the law bound, we combine three main sources of data. Pre-law harassment comes from the 2009 Survey of Young People in Egypt (SYPE), fielded five years before the law. Its harassment module allows us to compute the share of women reporting harassment in each of Egypt's 27 governorates. We link this measure to two sources of microdata. Information on women's employment comes from Egypt's harmonized Labor Force Survey, a quarterly survey covering 2006–2022 with a total of 2.5 million observations. We use these data to study participation, occupation and workplace location for adults aged 25–65. Information on establishments comes from the firm survey of 2013 and 2017–18, which records employment by sex and industry. To our knowledge, the survey has not previously been used to study women's employment.

To identify the effect of the law, we leverage the variation in pre-law harassment across governorates, over time and between sexes, using a triple difference. The first source of variation is geographic: since the law should have had more to change where harassment was more common, we compare high- and low-harassment governorates. The second source of variation is the timing of the law, which took effect in June 2014, so we compare quarters before and after. Together, these two sources of variation account for any fixed difference between governorates and for any shock common to the whole country. The third source of variation is the sex of the respondent, since the law protects women while men live in the same governorates and work in the same local labor markets. Hence, comparing women to men accounts for any governorate-specific shock that affected both sexes in the same way. In other words, we identify the effect of the law from the change specific to women in high-harassment governorates after June 2014.

We find that women in high-harassment governorates entered the labor force after the law. A one standard deviation increase in pre-law harassment raises women's participation by 9 percent. Women also change where they work. The harassment share of the governorate where a woman works (as opposed to the governorate where she lives) increases, and she becomes almost twice as likely to work in a governorate with more harassment than her own. In addition, the distance between the governorate where she lives and the governorate where she works increases by 4.3 kilometers per standard deviation of pre-law harassment, relative to a pre-law mean of 1.4 kilometers for women. The event studies show no trend before the law, and we find no comparable change for men.

A key question remains, namely which jobs these women moved into. If the law worked only through the safer-interaction channel, working among men would have become less costly, and women should move toward the jobs they had been avoiding, namely male-dominated jobs that involve working with other people. However, if the

law also raised the cost for employers of having women work with men, we should observe the opposite, namely women moving out of jobs that involve working with other people. To distinguish these two channels, we examine the interaction content of women's occupations using the Occupational Information Network (O*NET), and we focus on three tasks, namely working with a group or team, communicating with supervisors, peers and subordinates, and contact with others. We find that women in high-harassment governorates move into occupations that are less intensive in all three tasks. This is consistent with the employer-cost channel, through which firms face a higher cost of employing women together with men. For this shift to come from women themselves, the law would have to have made women more reluctant to work among men than before, at the same time as it made such work safer.

We complement our analysis by studying the response of employers, using a novel firm survey that provides information on the employment of women and men across occupations within an establishment. We compare the female employment share of establishments in high- and low-harassment governorates, before and after the law, across industries with different levels of gender mixing, that is, industries in which men and women work together to a different extent. We measure gender mixing in two ways. The first is industry *mixedness*, which is highest when an industry is evenly split between the sexes. The second is occupational *integration*, which measures how similarly men and women are distributed across occupations within an industry. Under the employer-cost channel, the female employment share should decline the most in high-harassment governorates and in the most gender-mixed industries. Consistent with this channel, we find that, per standard deviation of mixedness, a one standard deviation increase in pre-law harassment lowers the female employment share of establishments by about 6 percent of its 2013 mean. The decline is of similar size when we use integration.

Finally, we dedicate part of our analysis to documenting the enforcement and the application of the law in sexual harassment cases. We first collect all the harassment headlines published by *Youm7*, one of Egypt's largest daily newspapers, and show that the share of these headlines about arrests, prosecutions and trials more than doubled after the law. In addition, we find that this coverage increased in public space as well as at work: between the five years before the law (2009–2013) and the four years after it (2015–2018), the average yearly coverage of harassment rose 2.3-fold in public space and 3.6-fold at work. We then use the geocoded reports of HarassMap, an online crowdsourcing platform on which women report harassment incidents, and find that reports declined the most in high-harassment governorates after the law. Per standard deviation of pre-law harassment, reports are 29 percent lower in 2016 and 27 percent lower in 2017, and they remain 25 percent lower in 2020. Although these estimates are imprecise and measure reporting rather than incidents, they suggest that the law

reduced harassment the most where it was most common.

Taken together, these results suggest that an effective law that criminalizes sexual harassment in public places and in the workplace affects women's work in two ways. On the one hand, by making streets safer, the law raises female participation and changes where women work, since it becomes safer for women to work outside the home and to travel further to work. This is consistent with the safer-commuting channel. On the other hand, instead of the greater gender mixing at work, we observe the opposite. Establishments reduce their female employment share the most in gender-mixed industries and in industries that involve more contact with others.

Related literature. This paper contributes to the literature on female labor supply in developing countries, where the rise in education has not translated into higher participation (Jayachandran, 2021). This literature has identified social norms (Bursztyn et al., 2020; Grosjean and Khattar, 2019), bargaining within the household (Field et al., 2021) and distortions faced by female entrepreneurs (Chiplunkar and Goldberg, 2024) as binding constraints. Safety has been suggested as an additional constraint, and there is now substantial evidence that women pay a cost to avoid harassment. They give up college quality for safer routes (Borker, forthcoming), work less where the threat of assault is higher (Chakraborty, Mukherjee, Rachapalli, and Saha, 2018; Siddique, 2022), and apply for more jobs when offered a safe commute (Garlick, Field, and Vyborny, 2025). So far, however, the response of labor supply has been either inferred from willingness to pay or estimated for local interventions, such as a transport service, a patrolled hotspot or a women-only carriage. We provide the first causal estimate of how women's labor supply responds when a state criminalizes harassment.

Our paper also relates to a large literature on gender segregation in the labor market and its causes. The sorting of men and women across firms and occupations accounts for a substantial share of the gender pay gap (Blau and Kahn, 2017; Card, Cardoso, and Kline, 2016), and it has been linked to preferences, human capital and employer discrimination (Goldin, 2014a). Recent work shows that the risk of harassment pushes workplace gender minorities out of gender-mixed firms (Folke and Rickne, 2022), that in Finland, the share of women employed declines in firms where a male employee is violent toward a female colleague (Adams-Prassl, Huttunen, Nix, and Zhang, 2024), and that workplace harassment is underreported (Dahl and Knepper, 2026). We add to this literature by studying a change in the law. Since the same law affects both sides of the market, it allows us to distinguish the response of women from the response of their employers.

In addition, our findings inform the literature on who bears the cost of laws designed to protect workers. Mandated benefits are absorbed by the wages of the workers they cover (Summers, 1989; Gruber, 1994), and employment protection for disabled workers

reduced their employment (Acemoglu and Angrist, 2001), as anti-harassment reforms did for women in US startups (Chen, Ma, and Zhang, 2026). Closest to our paper, Bhalotra, Chatterjee, Mahajan, Walia, and Wang (2025) study India's 2013 law requiring workplaces with ten or more workers to set up a complaints committee, and find that covered establishments reduced their female employment share while women moved to smaller, uncovered establishments. Our establishment-level results point in the same direction. Two features of the Egyptian setting allow us to go further. First, India's law covers only the workplace, whereas Egypt's also covers harassment in public space. Because the law makes women's commute to work safer, it lets us ask whether safer streets bring women into the labor force. Second, because no employer is exempt from Egypt's law, women cannot adjust by moving to uncovered establishments. Hence, the adjustment takes place in the type of jobs that women hold, and we show that they move into occupations that involve less interaction with others.

Roadmap. The paper is structured as follows. Section 2 describes female labor supply and harassment in Egypt and the 2014 law. Section 3 presents a conceptual framework. Section 4 introduces the data and the harassment measure. Section 5 documents the enforcement and the application of the law. Section 6 presents the empirical design. Section 7 presents the individual-level results and Section 8 the establishment-level results. Section 9 concludes.

2 Setting

2.1 Female labor-force participation and sexual harassment in Egypt

In Egypt, the gap in labor-force participation between women and men has not closed as women's education attainment improved. This rules out education as the binding constraint, and it has made the Egyptian case a long-standing puzzle.

In this context, safety in public space is an important candidate to explain why women stay out of the labor force, and it is also the one that the literature has examined the least. Sexual harassment in Egypt is very common: in the International Men and Gender Equality Survey (IMAGES), which surveyed men and women aged 18–59 in Egypt, Lebanon, Morocco and Palestine, almost two-thirds of Egyptian men report having ever sexually harassed a woman or girl, the highest share of the four countries. Almost 60% of Egyptian women report having ever been harassed, and more than a quarter in the preceding three months alone (El Feki, Heilman, and Barker, 2017). These prevalence rates also understate how frequent harassment is, since among Egyptian women who report street harassment, three-quarters experience it at least once a week (Hesham, Dupont-Kieffer, Ramadan, and Nazier, 2025). At this frequency, harassment

represents a permanent cost on the activities that paid work outside the home requires, such as walking in the street and using public transport. In addition, harassment is perceived as a cost of *working* in particular, both by women and by the men who often decide whether they can work. In a nationally representative survey of social norms conducted for the National Council for Women, 70% of Egyptians agree that women's work exposes them to harassment (76% of men and 64% of women) (Zeitoun, El Assiouty, Rahman, Osman, and Girgis, 2023). Consistent with this, Hesham et al. (2025) document that Egyptian women who report harassment in public space are less likely to be in the labor force, and that those who work are disproportionately found in occupations they describe as safer.

2.2 The 2014 anti-sexual-harassment law

On June 5, 2014, President Adly Mansour signed Decree-Law 50 of 2014, which replaced Article 306-bis A of the Penal Code, added Article 306-bis B and made sexual harassment a distinct offense. Article 306-bis A covers verbal, gestural and physical harassment committed in *any* "public space, a private one, or where one is present with permission". This covers the street, public transport and workplace premises. The offense is punishable by at least six months in prison, a fine of 3,000 to 5,000 pounds, or both. Article 306-bis B defines sexual harassment as the same acts committed to obtain a sexual benefit from the victim, punishable by at least a year in prison, and it raises the penalty to two to five years in prison when the offender holds *occupational, familial or educational authority* over the victim.

Before the law, harassment was not a distinct offense, but some of its forms could already be prosecuted. The earlier version of Article 306-bis A punished anyone who offended the modesty of another person by word, deed or gesture in a public road or a frequented place, and more serious acts could be prosecuted as indecent assault. Compared to this provision, the 2014 law extended the offense to private places, including the workplace, raised the fines and introduced heavier penalties for offenders with authority over the victim.

3 Conceptual Framework

There are three channels through which the law can affect women's work. The first operates in public space and affects whether and where women work. The other two operate in the workplace, and they have opposite implications for the jobs women hold.

First, the law makes the street and public transport safer for women, which lowers the cost of getting to work (the *safer-commuting channel*). By raising the expected penalty

for harassment, the law should deter it in the places that Article 306-bis A covers (Becker, 1968; Polinsky and Shavell, 2000). These include the street and public transport, which women have to use to reach a job outside the home. As discussed in Section 2, harassment in these places is frequent in Egypt, and most Egyptians see it as a risk of women's work. There is also evidence that this risk limits women's work, since women in Lahore apply for more jobs when they are offered a safe commute (Garlick et al., 2025). In Cairo, women are less mobile than men and perceive public transit as unsafe, and they increase their travel more than men when ride-hailing becomes cheaper (Christensen, Nino, and Osman, forthcoming). In addition, the men who often decide whether women can work see the same risk, so a safer commute can change their decision as well as hers. Through this channel, women should become more willing to work outside the home, so that female participation rises. Women should also become more willing to work farther from home, including in a governorate with more harassment than the one where they live. Since the law has more to change where harassment was common, both effects should be largest in the governorates with the highest pre-law harassment.

Second, the law makes the workplace itself safer, which lowers the cost, for women, of working with men (the *safer-interaction channel*). The 2014 law extended the offense to private places, including workplace premises, and it introduced heavier penalties for offenders with authority over the victim. Inside the workplace, the risk of harassment that a woman faces depends on how much she interacts with men. Most perpetrators of harassment are of the opposite sex of their victim, and supervisors initiate a sizable share of workplace harassment, so the risk grows with the number of men a woman works with and the number of men above her (Folke and Rickne, 2022). Workers treat this risk as a disamenity, for which they require higher wages (Basu, 2003; Hersch, 2011), and it pushes women who are a minority in their workplace to leave (Folke and Rickne, 2022). A substantial share of the harassment that women face at work also comes from clients, customers and patients, in particular in occupations where most workers are women (Folke and Rickne, 2025; Chikhale, Duncombe, and Larsen, 2025). Jobs that involve teamwork, communication with colleagues and contact with clients and the public are therefore the jobs in which harassment was the largest risk for women before the law, and in which the law reduces this risk the most. Through this channel, women should become more willing than before the law to take jobs with more interaction with others and to work in gender-mixed industries, both among women who already worked and among those who enter the labor force. However, some women may avoid these jobs for reasons that the law does not change, such as social norms against working with men or the objections of their families, and for them a safer workplace leaves the choice of job unchanged. Hence, through the safer-interaction channel, the interaction content of women's jobs and the female employment share of

establishments in gender-mixed industries should either increase or remain unchanged.

At the same time, however, the law raises the cost of harassment for employers (the *employer-cost channel*). Since Article 306-bis B sets the heaviest penalties for harassment by someone with occupational authority over the victim, owners, supervisors and managers face the greatest legal risk. Moreover, any case of harassment within the firm can now become a criminal case involving the firm's own staff. Such a case disrupts production, for instance through investigations, the absence or departure of the employees involved and conflict within teams, and it exposes the firm to reputational risk. For the employer, any team in which women work with men therefore becomes a source of legal risk and of costly disruptions to production. In addition, since the law only amended the Penal Code, it gave employers no procedure, such as a complaints committee, through which they could manage this risk, and limiting the contact between women and men at work is one way to reduce it. Evidence from other settings shows that employers and male colleagues respond in this way. In India, establishments covered by a law requiring a complaints committee for sexual harassment reduced their female employment share (Bhalotra et al., 2025), in the United States, startups hired fewer women after states weakened nondisclosure agreements in sexual-harassment cases (Chen et al., 2026), and after the #MeToo movement raised the perceived cost of an allegation, senior male economists started fewer new projects with junior female colleagues (Gertsberg, 2026).[1] Through this channel, employers should become less willing to employ women in jobs where men and women work together, either by hiring fewer women for these jobs or by moving the women they employ to jobs with less interaction with men.

Therefore, the effect of the law on women's work is ambiguous. The safer-commuting channel should raise female participation, while the employer-cost channel can lower the number of women that employers hire. The effect of the law on the composition of women's jobs is also ambiguous. If the safer-interaction channel dominates, women should move *into* jobs that involve interaction with others and into gender-mixed industries, unless other reasons keep them out of these jobs. On the other hand, if the employer-cost channel dominates, women should move *out of* these jobs and industries. A preference against such jobs, or a social norm against working with men, that predates the law can explain why women do not move into these jobs. However, it cannot explain a move out of them, since it does not change when the law is passed. For women's own choices to explain such a move, the law would have to have made women *more* reluctant to work with men than before, at a time when it made such work safer. Hence, only the employer-cost channel can explain

[1] More generally, laws that raise the cost of employing a protected group can reduce the employment of that group. For example, the Americans with Disabilities Act reduced the employment of disabled workers (Acemoglu and Angrist, 2001).

a decline in the interaction content of women's jobs. In addition, the employer-cost channel predicts that a decline in the female employment share should be concentrated in male-led establishments, whose owners and managers face the heaviest penalties under Article 306-bis B, while the safer-interaction channel makes no such prediction.

A change in who works could also lower the average interaction content of women's jobs, without any response from employers. If the women whom the safer-commuting channel draws into the labor force take jobs with less interaction than the women who already worked, the average interaction content of employed women declines, and we might mistakenly attribute this decline to employers. However, the two explanations have different implications for establishments. The entry of new women into the labor force can only raise the female employment share of establishments, whereas the employer-cost channel predicts that it declines in gender-mixed industries. Hence, the female employment share of establishments allows us to distinguish a response of employers from a change in who works.

To distinguish these channels, we therefore examine (1) female labor-force participation and (2) where women work, which respond to the safer-commuting channel, (3) the interaction content of women's occupations, for which the safer-interaction and employer-cost channels predict opposite changes, and (4) the female employment share of establishments in gender-mixed industries and separately for male-led and female-led establishments.

4 Data

In this section, we describe the three main sources of data. We use the Labor Force Survey to measure the outcomes of women, the firm survey to measure the outcomes of establishments, and a youth survey fielded five years before the law to measure the treatment. We complement them with occupational scores from O*NET.

4.1 Individual-level data

Labor Force Survey. Data on women's work come from the harmonized Labor Force Survey (OAMDI, 2016–2022), a nationally representative quarterly repeated cross-section conducted by the Central Agency for Public Mobilization and Statistics (CAPMAS), Egypt's statistical agency. Our sample includes adults aged 25 to 65 in the 68 quarters from 2006q1 to 2022q4. The final sample consists of **2,507,489 observations**, of which 1,294,522 are on women, and the mean age is 42.

The survey records labor-force status, employment, occupation, industry, sector, weekly hours and monthly wages. We define labor-force participation as being em-

ployed or unemployed and searching for a job. In some rounds, the survey also includes a module on the workplace, which records the governorate where the respondent works. This information allows us to study whether a woman works and also where she works.

Occupational attributes. The Labor Force Survey reports the occupation a woman holds but not what this occupation involves. We measure the task content of a job with data from the Occupational Information Network (O*NET). O*NET surveys workers and occupational experts about the content of each occupation. Its scores are widely used to measure the content of jobs (see, for instance, Goldin, 2014b). Since they describe jobs in the U.S., we assume that occupations rank similarly by task content in Egypt. We merge the O*NET scores to each ISCO-88 4-digit code using the SOC-to-ISCO crosswalk, which covers 356 occupations. Occupations in 2020–2022 are recorded in ISCO-08, and we map them to ISCO-88 using the survey's own ISCO-08 to ISCO-88 crosswalk. We use three scores: *work with a work group or team*, which records how important it is to work with others in a group or team; *communicating with supervisors, peers and subordinates*, which measures the importance of internal communication in the job; and *contact with others*, which measures how much the job requires contact with other people. The three scores are standardized across the 356 occupations and enter continuously, so that coefficients are expressed in standard deviations of the attribute. In the rest of the paper, we refer to these three scores as *teamwork*, *internal communication* and *contact with others*, and to the three together as the *interaction content* of an occupation.

4.2 Establishment-level data

Firm survey. Data on establishments come from the 2013 and 2017–18 rounds of the Economic Census of CAPMAS (OAMDI, 2023, 2022). Although it is called a census, it is a sample survey of establishments, and we refer to it as the firm survey. By design, CAPMAS surveys large establishments in full and draws a sample of small ones, and the Economic Research Forum (ERF) releases half of the surveyed establishments to researchers, with sampling weights. The sample consists of **232,636 observations**, 62,306 in the first wave and 170,330 in the second. Once weighted, they represent 2.4 and 3.7 million establishments, respectively, which are the totals published by CAPMAS. For each establishment, we use employment by sex, governorate, industry, and the sex of the owners and managers.

Gender mixing within industries. Using the survey, we build two measures of *gender mixing*, that is, of how much an industry puts men and women in contact at work. Both measures are fixed at their pre-law (2013) values and standardized with establishment weights.

The first measure is industry *mixedness*, $1 - |0.5 - f_j|/0.5$, where f_j is the female

employment share of two-digit industry j. It equals one when an industry is evenly split between the sexes, which is its theoretical maximum ($f_j = 0.5$), and zero when only one sex works in the industry. In the 2013 survey, mixedness varies widely across industries, from zero in fishing and gambling, which employ no women, to 0.36 in retail trade, 0.87 in education and 0.97 in social work. The establishment-weighted mean is 0.29 (standard deviation 0.18).

The second measure is *occupational integration*, defined as one minus the Duncan dissimilarity index between the occupational distributions of men and women across the eight occupation categories of the survey (owners, managers, specialists, supervisors, clerks, operatives, sales, other), where $D_j = \frac{1}{2}\sum_{o=1}^{8}|F_{jo}/F_j - M_{jo}/M_j|$, F_{jo} and M_{jo} are the numbers of women and men in occupation o of industry j, and F_j and M_j are the numbers of women and men in the industry. An integration equal to 1 means that men and women are distributed identically across occupations, that is, they hold the same kinds of jobs and therefore work together. An integration equal to 0 means that occupations are completely segregated by sex within the industry. Integration is undefined in the two industries that employ only one sex, fishing and gambling, which therefore do not enter their section's average. For instance, in construction, the few women hold office jobs while men work on site (integration of 0.29). In education, both men and women are teachers (0.86), which is among the most integrated industries.

4.3 Sexual harassment

Survey of Young People in Egypt. The treatment comes from SYPE 2009 (Population Council, 2009), a nationally representative survey of Egyptians aged 10 to 29 conducted by the Population Council. The survey was fielded five years before the law, on a population independent of the Labor Force Survey. The harassment module was administered to female respondents only, who were asked: *"Have you ever experienced any sexual harassment?"* Among the 8,087 women who answered, 45.1% said yes. We compute the share of women answering yes in each of Egypt's 27 governorates and standardize it across governorates, so that coefficients are expressed per standard deviation of harassment. Prevalence varies a lot across governorates, from 9.9% to 75.6%, which represents a difference of more than sixty-five percentage points within a single national labor market. Figure 1 maps these shares. In the median governorate, 40 percent of women report having experienced harassment, and in ten governorates more than half do.

5 The Law and Sexual Harassment

In this section, we document the enforcement and the application of the law in sexual harassment cases. We first use the harassment coverage of *Youm7* to show that the law durably raised the salience of harassment, that the share of this coverage about arrests, prosecutions and trials more than doubled, and that coverage increased for harassment in public space as well as at work. We then use the HarassMap reports to show that reported harassment declined the most in the governorates where it was most common before the law.

5.1 Enforcement and the content of the law in the news

To document how the law was received, we use the harassment coverage of *Youm7*, one of Egypt's largest daily newspapers. This source measures public attention rather than incidents. We collect all the harassment coverage of the newspaper and keep the 7,479 articles published between 2008 and 2026 whose headline contains the Arabic root for harassment, *taharrush*. We classify each headline by whether it mentions enforcement and by the setting it refers to. Enforcement and public space are classified with Arabic keyword lists. The workplace setting is coded by hand, since workplace words such as worker or employee often describe the offender's job when the harassment takes place in the street. Figure 2 shows an example of an article, in the original Arabic and in translation. Since its headline mentions both the setting and the sanction, this article would be classified as both workplace and enforcement coverage. Appendix B.3 gives the keyword lists, the coding rule and the retrieval procedure.

First, harassment coverage rose more than two and a half times after the law and remained high. As Figure 3 shows, coverage was low until 2012, between 109 and 334 articles a year, and rose to 544 in 2013. It then jumped to 1,648 articles in 2014, mostly in the quarter when the law was signed (740 articles in June alone), and remained at a higher level of 592–753 articles a year until 2018. This suggests that the law durably raised the salience of harassment as a public issue, beyond a single news cycle.

Second, the share of this coverage about arrests and prosecutions increased permanently. We classify an article as *enforcement-themed* if its headline mentions an arrest, a prosecution, a detention, a verdict, a conviction, the Public Prosecution, a court or the police, and we compute the share of harassment articles that are enforcement-themed. This share was 7–17% before the law and about 30% after it. In absolute numbers, enforcement articles rose from 9 to 85 a year before the law (a mean of 39) to 181–225 a year after it (line in Figure 3).

Third, coverage increased for harassment at work as well as in public space. Although the amendment is popularly known as the "anti-street-harassment law," it

extended the offense to private places, including the workplace, and raised the penalty when the offender has authority over the victim (Section 2.2). Comparing the five years before the law (2009–2013) with the four years after it (2015–2018), the average annual number of articles rose 2.3-fold for harassment in public space and 3.6-fold for harassment at work (Figure 4).

5.2 HarassMap and the law

Did the law reduce harassment? To answer this question, we use data on sexual harassment events from HarassMap, an Egyptian crowdsourcing platform launched in Cairo in 2010, on which women can report harassment incidents online or by text message. Figure 5 shows the public map where reports are displayed, and Figure 6 shows the individual records behind it, which include the category of harassment, a free-text location, a description and a date. We scrape the archive of individual geocoded reports, which runs from 2010 to 2020. Of these four fields, we only use the date and the location. Appendix B.2 describes the retrieval, the geocoding and the construction of the panel.

If the law reduced harassment, reporting should have declined the most where harassment was the most prevalent before the law. To test this, we build a balanced governorate-year panel with the number of reports filed in each governorate and year over 2010–2020, and estimate the following event study:

$$R_{gt} = \sum_{k \neq 2013} \lambda_k \left(\text{Intensity}_g \times \mathbb{1}\{t = k\} \right) + \alpha_g + \tau_t + \varepsilon_{gt}, \tag{5.1}$$

where R_{gt} is the log of one plus the number of reports in governorate g and year t, and Intensity_g is the standardized pre-law harassment share of the governorate, measured in 2009. The governorate fixed effects α_g absorb any persistent difference in the use of the platform across governorates, and the year fixed effects τ_t absorb any change common to all governorates, such as the decline in the overall activity of the platform. Each coefficient λ_k therefore measures how reports changed between 2013, a year before the law, and year k in a governorate with one standard deviation more pre-law harassment. Standard errors are clustered at the governorate level.

Figure 7 shows that before the law, the coefficients are close to zero, and none of them is statistically different from zero. After the law, reporting declines more where pre-law harassment was higher. Per standard deviation of pre-law harassment, reports decline by 29 percent in 2016 ($p = 0.06$) and by 27 percent in 2017 ($p = 0.08$), and they are still 25 percent lower in 2020 ($p = 0.10$). These results suggest that reporting declined the most where the law had the most to change, which is the starting point of

our analysis.

6 Empirical Strategy

6.1 Triple difference

Our empirical strategy is a triple difference that exploits three sources of variation. The first is pre-law harassment, measured by the share of women reporting sexual harassment in each governorate five years before the law. The second source of variation is the timing of the law. The law entered into force in June 2014, so all quarters from 2014q3 onwards are post-treatment and all earlier quarters are pre-treatment.[2] The third source of variation is the sex of the respondent. The law changed the expected cost of harassment for women and, through the employer's enforcement responsibility, the expected cost for firms of employing women. It did not change these costs for men, who live in the same governorates and face the same local labor markets and the same national shocks. Therefore, men serve as a comparison group within each governorate. The triple-difference specification is

$$\begin{aligned} Y_{igt} &= \beta \left(\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i\right) + \text{(lower-order interactions)} \\ &\quad + \left(\text{Female}_i \times \delta_t\right) + \alpha_g + X'_{igt}\gamma + \varepsilon_{igt}, \end{aligned} \tag{6.1}$$

where i denotes an individual, g her governorate of residence and t the year-quarter of the survey. The outcome Y_{igt} is either an indicator for participating in the labor force or, conditional on working, a characteristic of the job held, namely the harassment share of the governorate where the job is located, whether this governorate has more harassment than her own, the distance to it, and the interaction content of the occupation (teamwork, internal communication and contact with others). Intensity_g is the continuous measure of pre-law harassment at the governorate level, standardized across the 27 governorates so that β is expressed per standard deviation. Post_t is a dummy taking the value one from 2014q3 onwards, and Female_i indicates a woman. We substitute the governorate fixed effect α_g for the main effect of harassment (Intensity_g) and the female$\times$year-quarter fixed effect δ_t for the main effects of sex and of time and their interaction. These fixed effects account for any time-invariant difference between governorates and for any shock common to all women in a given quarter. X_{igt} is the vector of individual controls, which includes age, age squared, ever-married status, urban residence and household size. We use the same controls for every outcome and refer to them as the individual controls in the results section. Our preferred specification includes these

[2] The law was signed on June 5, 2014, in the middle of the second quarter, so 2014q3 is the first quarter subject to the new law.

controls and corresponds to column (2) of Tables 1 to 3, and the estimates we quote in the text come from this column unless stated otherwise. Standard errors are clustered on the 27 governorates, the level at which treatment varies (Abadie, Athey, Imbens, and Wooldridge, 2023).

In addition, the comparison with men removes any governorate-level shock that coincides with the law and affects women and men in the same way. What remains is the change specific to women within a governorate after the law, which is the change the law is expected to produce.

The coefficient of interest is β, which measures the differential change for women relative to men, in high- relative to low-harassment governorates, after the law. The change common to every governorate, namely the national effect of the law, is absorbed by the female$\times$year-quarter fixed effects. Hence, β measures how much more the law affected women where harassment was more prevalent.

6.2 Identifying assumption

Though the triple-difference specification accounts for many potential identification concerns, β will still be biased if women in high-harassment governorates would have experienced differential non-linear trends relative to men in those governorates even without the law. To assess whether trends are parallel before the law, we estimate a specification that replaces Post_t with a full set of leads and lags. This event-study analysis allows us to compare the treated and comparison groups at multiple points in time before and after the law. We estimate this specification separately by sex.[3] The coefficients β_q capture the differential effect of a standard deviation of pre-law harassment in each quarter, relative to the last quarter before the law took effect (2014q2). Figures 8 to 11 use specification (1) of Tables 1 and 3, without the individual controls, and Figures A.1 to A.4 show the coefficients with the controls. Estimating the event study separately for women and men allows us to use the male series as a placebo. Men in the same governorates are exposed to the same local shocks that the design could otherwise attribute to the law, so their coefficients should be close to zero throughout.[4] Figures A.1 to A.4 and A.6 report event-study results.

District-level variation. A more granular version of the design is available for

[3]The specification is

$$Y_{igt} = \sum_{q \neq q_0} \beta_q \left(\text{Intensity}_g \times \mathbb{1}\{t = q\} \right) + \alpha_g + \delta_t + X'_{igt}\gamma + \varepsilon_{igt}, \tag{6.2}$$

where q indexes year-quarters and $q_0 =$ 2014q2 is the omitted reference quarter, the last one before the law took effect. Governorate and year-quarter fixed effects are as in equation (6.1).

[4]In the Appendix, we also estimate a pooled version of the event study on women and men together, in which each quarter is interacted with $\text{Intensity}_g \times \text{Female}_i$.

part of the sample. SYPE 2009 provides a harassment share at the district level for 222 of Egypt's 365 districts, and the Labor Force Survey identifies the district of residence in 2006–2010, 2012–2015 and 2017–2018. On this subsample, we re-estimate equation (6.1) with the district harassment share, district fixed effects and a full set of governorate $\times$ year-quarter fixed effects, which absorb any shock common to women and men in a governorate and quarter, and standard errors are clustered at the district level. The district design measures harassment at a finer level than the governorate, and it increases the number of clusters from 27 governorates to 219 districts.

With only 27 clusters, inference requires particular attention, since cluster-robust t-tests over-reject when there are few clusters (Cameron, Gelbach, and Miller, 2008; MacKinnon, Nielsen, and Webb, 2023). Therefore, for every main coefficient, we also report a wild cluster bootstrap with Webb weights. Table A.1 reports the results.[5]

7 Individual-Level Results

In this section, we present the main results of the paper. We estimate equation (6.1) on the Labor Force Survey, first on the extensive margin, namely whether a woman participates in the labor force, and then, conditional on working, on the characteristics of the job she holds.

The characteristics of the job allow us to distinguish the safer-interaction and employer-cost channels described in Section 3. Under the safer-interaction channel, women should have *more* contact with men, or no less than before, since the law makes this contact safer. Under the employer-cost channel, women should have *less* contact with men, since employers stop hiring women in jobs where men and women interact. Therefore, a decline in this contact can only come from the employer-cost channel.

7.1 Labor-force participation

Triple difference. Table 1 reports the first set of results. We find that a one standard deviation increase in pre-law harassment raises women's labor-force participation by 0.023 ($p = 0.03$), which represents an increase of *9 percent* relative to the pre-period mean of 0.26. This result does not depend on the specification.

Each column of Table 1 addresses a different concern. Column (1) only includes the

[5] A final concern is that two-way fixed-effects estimates can be biased when treatment effects are heterogeneous, see Borusyak, Jaravel, and Spiess (2024) and de Chaisemartin, D'Haultfœuille, and Vazquez-Bare (2024). The staggered-adoption version of this critique does not apply here, since the law has a single national date and there is no variation in timing that could be weighted badly. The relevant case is a continuous treatment with a common date (Callaway, Goodman-Bacon, and Sant'Anna, 2024). In Section 7.4, we show that a binary version of the treatment gives event-study coefficients similar to those of the continuous treatment.

fixed effects required by the design. Governorate fixed effects absorb any characteristic of a governorate that does not change over time, and female $\times$ year-quarter fixed effects absorb any shock that affects all women in the country at the same time, such as the national decline in participation, the macroeconomic situation or a nationwide change in attitudes. Column (2) adds the individual controls, and the estimate is very similar (0.0224 in column (1) and 0.0230 in column (2)). Column (3) includes governorate-specific linear trends, which account for slow divergence between governorates due, for instance, to urbanization, long-run industrial decline or a gradual change in local labor demand, and the estimate is 0.0232. Column (4) replaces the governorate fixed effects with governorate $\times$ year-quarter fixed effects, which absorb any shock common to women and men in a governorate and quarter. In this specification, identification only comes from the gap between women and men in the same governorate and quarter, and the estimate is 0.0234.

Event study. Figure 8 shows the coefficients from equation (6.2), estimated separately for women and men, where Post_t is replaced with a full set of leads and lags and the reference period is 2014q2, the last quarter before the law. There are several takeaways from this figure. First, women's coefficients are close to zero during the eight years before the law, between -0.027 and $+0.021$, and show no trend. Second, they start increasing right after 2014q3, reach about 0.034 around 2020 and remain positive until 2022. Finally, the coefficients for men, estimated on the same governorates and quarters, remain within two percentage points of zero during the whole period.

The male series is important to interpret the female series. Men are exposed to the same local shocks that the design could otherwise attribute to the law, since they live in the same governorates, face the same labor markets and experienced the same macroeconomic disruption. However, men were not the intended beneficiaries of the law, and the law did not make their employment more costly for firms. The fact that men's coefficients remain close to zero while women's coefficients increase, and that neither series shows a trend before 2014, supports the parallel-trends assumption required by the triple difference.

7.2 Workplace location

To better understand how women entered the labor force, we use a module on the location of the job, available for a subsample. In this module, the Labor Force Survey records the governorate where a woman's main job is located, which may differ from the governorate where she lives.[6] The module is only available in some rounds (2010,

[6]The survey records the governorate where a person works and no finer location. Hence, our outcomes measure work across governorates, and they do not capture changes in commuting within a governorate. Most women who work in another governorate work in an adjacent one, so that these outcomes may

2011, 2014 and 2016–2022) and is absent in 2012, 2013 and 2015. Hence, 2010 and 2011 are the pre-law period and 2016–2022 the post-law period, while 2014 includes quarters before and after the June 2014 law. Since the module only covers two pre-law years, we only report the static triple difference for these outcomes, and these results should be read as suggestive.

Triple difference. Table 2 reports the results, which show that relative to men and to low-harassment governorates, the harassment share of the governorate where a woman works increases by 0.041 standard deviations ($p < 0.001$; Panel A). Women also become 1.8 percentage points more likely to work in a governorate with more harassment than the one they live in, relative to a pre-law mean of 2.1 percent ($p < 0.01$; Panel B). Both estimates are slightly larger with governorate $\times$ year-quarter fixed effects (0.044 and 0.019).

In Panel C, we express this result in kilometers. We compute the great-circle distance between the centroid of the governorate where a woman lives and the centroid of the governorate where her main job is located, where centroids are the mean of the vertices of each governorate's polygon. This distance increases by *4.3 kilometers* per standard deviation of harassment ($p = 0.015$). Before the law, the mean distance was 1.4 kilometers for women and 9.7 for men. In other words, before the law, women worked about a seventh as far from their governorate of residence as men did, and the law closes part of this gap. The estimate is robust to all specifications, including governorate $\times$ year-quarter fixed effects (3.7 km). The three outcomes are estimated on the same sample of 461,524 observations, so that the three panels are directly comparable.

Taken together, these results suggest that harassment had limited where women could work, which is in line with previous work showing that women give up opportunities to avoid harassment during their commute (Borker, forthcoming; Kondylis, Legovini, Vyborny, Zwager, and Andrade, 2025; Garlick et al., 2025), and that women give up more of their wage than men to have a shorter commute (Le Barbanchon, Rathelot, and Roulet, 2021), and that mothers who cut their commute after childbirth end up in thinner local labor markets with fewer and lower-quality employers (Bütikofer, Karadakic, and Willén, 2025).

7.3 Interaction content of women's jobs

The law can affect the interaction content of women's jobs through the safer-interaction and employer-cost channels described in Section 3. Under the safer-interaction channel, a credible sanction reduces the risk a woman faces when she works closely with co-

reflect residents of border areas. Overall, our results are likely to be a lower bound on the changes in commuting after the law.

workers. Hence, she should become *more* likely to hold a job that involves interaction. Under the employer-cost channel, the same sanction exposes her employer to what happens between her and her colleagues, so employers should avoid placing women in these jobs. Therefore, the safer-interaction channel predicts a positive or zero coefficient on the triple interaction, while only the employer-cost channel predicts a negative one.

We measure the interaction content of a job with the three O*NET items described in Section 4, namely teamwork, internal communication and contact with others. The three items are mapped to ISCO-88 four-digit occupations and standardized across occupations, so that coefficients are expressed in standard deviations of the attribute.

We find that the three measures decline, which is consistent with the employer-cost channel dominating. In governorates where harassment was more prevalent before the law, women end up in jobs that involve less work and communication with other people.

Triple difference. Table 3 reports the results for employed women, and shows that a one standard deviation increase in pre-law harassment reduces the teamwork content of the occupation by 0.031 standard deviations ($p < 0.01$) and its internal-communication content by 0.034 standard deviations ($p < 0.01$). Column (1) includes governorate fixed effects, which absorb any time-invariant characteristic of a governorate, and female $\times$ year-quarter fixed effects, which absorb any shock affecting all women in the country at the same time. Column (2) adds the individual controls, and both estimates barely change, from -0.0341 to -0.0310 for teamwork and from -0.0369 to -0.0344 for internal communication. Column (3) includes governorate-specific linear trends to account for slow divergence between governorates, and the estimates are -0.0298 and -0.0321. Column (4) replaces the governorate fixed effects with governorate $\times$ year-quarter fixed effects, so that identification only comes from the gap between women and men in the same governorate and quarter, and the estimates are -0.0288 and -0.0312.

The third measure, contact with others, also declines, and the effect is larger (-0.059 in column (1) and -0.046 in the most demanding specification). It is statistically significant at the five percent level in all four columns. Taken together, these results do not depend on the controls or on the fixed effects included. Across the four columns, the teamwork estimate ranges from -0.034 to -0.029 and the internal-communication estimate from -0.037 to -0.031, and both remain statistically significant.

Event study. Figures 9, 10 and 11 show the coefficients from equation (6.2), estimated separately for women and men, with 2014q2 as the reference period. Before the law, women's coefficients show no trend for any of the three outcomes. After the law, they decline, with an average of -0.08 for teamwork, -0.04 for internal communication and -0.10 for contact with others over 2019–2022. In contrast, men's coefficients, estimated

on the same governorates and quarters, remain close to zero, with an average of 0.01 to 0.02 before the law and about 0.01 over 2019–2022.

Labor force participation and interaction content of jobs respond at different times. Participation increases right after the law, whereas the interaction content of women's jobs changes little in the first years after the law and declines towards the end of the decade. This delay is consistent with the employer-cost channel. As employers realize the cost of employing women in mixed teams over time, they start to change their workforce. In contrast, if jobs with little interaction were the entry-level jobs in the job ladder, the interaction content should decline as soon as new women enter the labor force, and recover as they move up the ladder.

7.4 Robustness

In this subsection, we perform a series of robustness checks on the individual-level estimates, each addressing a different concern.

Identification: finer fixed effects. Columns (3) and (4) of Tables 1, 2 and 3 include the two most demanding sets of fixed effects that the design allows, namely governorate-specific linear trends and governorate $\times$ year-quarter fixed effects. The latter absorb any shock common to women and men in a governorate and quarter, so that only the difference between women and men in the same governorate and quarter is used, and the estimates do not change much. Figures A.1 to A.4 show the same result for the event studies. They plot the event-study coefficients under each of the four specifications, in separate panels, for participation and for the three measures of interaction content. For each outcome, the four series are almost identical, which implies that the change after the law is not driven by the controls or by the fixed effects.

Identification: district-level variation. We also estimate the district-level version of the design described in Section 6.2, where harassment is measured for 222 districts and the last column adds a full set of governorate $\times$ year-quarter fixed effects (Tables A.2 and A.3).

The results for participation and for the three workplace-location outcomes are similar at the district level. The estimate for participation is $+0.0189$ with district fixed effects and $+0.0190$ when governorate $\times$ year-quarter fixed effects are added. Both are statistically significant at the one percent level and estimated with more precision than in the governorate-level specification, since the number of clusters increases from 27 to 219. The estimates are $+0.0374$ for the harassment share of the governorate of work, $+0.0186$ for working in a higher-harassment governorate and $+4.46$ km for the distance to the governorate of work, all statistically significant at the one percent level. Hence, for these four outcomes, the results are similar whether harassment is measured at the

governorate or at the district level.[7]

Excluding the Arab Spring. Egypt's 2011 revolution and its aftermath are included in the sample period, and the disruption was not uniform across governorates. When we exclude 2011–2013, we compare a 2006–2010 pre-period to the 2014–2022 post-period. All the estimates remain similar on this sample, which is a fifth smaller (Table A.4). The estimates are 0.0219 for participation, 0.0407 for the harassment share of the governorate of work, 0.0160 for working in a higher-harassment governorate and 3.86 kilometers for the distance to the governorate of work. The three interaction measures are *larger* than in the full sample, at -0.0338, -0.0399 and -0.0567, the last one being significant at the five percent level.

Placebo reform dates. We estimate placebo reforms dated 2010q1, 2011q1, 2012q1 and 2013q1 on the pre-law period only. We find no effect on participation or on the three interaction measures at any date, and the smallest p-value across these sixteen combinations is 0.16.

Leave-one-governorate-out. With 27 clusters and a continuous treatment, a single governorate with high leverage could in principle drive the estimate. In Figure A.5, we re-estimate each outcome 27 times, omitting one governorate at a time. The seven outcomes keep the same sign and remain significant at the ten percent level in *all* 27 samples. The range of the estimates is also narrow, namely $[0.015, 0.026]$ for participation, $[0.036, 0.045]$ for the harassment share of the governorate of work, $[-0.038, -0.027]$ for teamwork, $[-0.041, -0.031]$ for internal communication and $[-0.064, -0.036]$ for contact with others.

Wild cluster bootstrap. Since cluster-robust t-tests can over-reject when there are few clusters (Cameron et al., 2008; MacKinnon et al., 2023), Table A.1 reports, for every outcome, the p-values from a wild cluster bootstrap with Webb six-point weights and 9,999 replications, next to the asymptotic p-values. All outcomes remain statistically significant, with bootstrap p-values ranging from below 0.001 to 0.065.

Sampling weights. In Tables A.5 and A.6, we re-estimate all outcomes under the four specifications using the survey's sampling weights. In specification (2), the weighted estimate for participation is 0.0238, against 0.0230 unweighted. Similarly, the weighted estimates are 0.0524 against 0.0415 for the harassment share of the governorate of work, 0.0239 against 0.0178 for working in a higher-harassment governorate, 5.36 km against 4.30 km for the distance to the governorate of work, -0.0310 for teamwork (identical to the unweighted estimate), -0.0307 against -0.0344 for internal communication, and -0.0480 against -0.0507 for contact with others. All seven estimates remain statistically significant in every specification.

[7] We do not report district-level results for the interaction content of jobs, since the district identifier is not available after 2018, whereas the change in job content appears mostly after that year (Figures 9 to 11).

Two definitions of the treatment. One may worry that the functional form of the treatment drives the results. Figure A.6 plots the event-study coefficients under two definitions of the treatment. The first is the continuous standardized SYPE 2009 harassment share that we use throughout the paper. The second compares governorates above and below the 70th percentile of this share, so that identification comes from a comparison between two groups. When the continuous coefficients are rescaled to a high-versus-low contrast, the two series follow each other closely before the law for all four outcomes. Hence, the results are qualitatively similar under the two definitions of the treatment.

8 Establishment-Level Results

In this section, we provide establishment-level results and test whether the law increased the cost for employers of hiring women in gender-mixed settings, as predicted by the employer-cost channel.

8.1 Specification

The second part of our empirical strategy examines whether establishments in the same governorates changed whom they employ, in particular where the law made contact between men and women at work more costly. Since the firm survey does not have an establishment identifier linking the two waves, we cannot use men as a comparison group within the same unit of observation. Instead, sex enters through the gender mixing of the establishment's industry. We estimate

$$\begin{aligned} Y_{ejgs} &= \theta \left(\text{Intensity}_g \times \text{Post}_s \times Z_{k(j)} \right) + (\text{lower-order interactions}) \\ &\quad + \alpha_g + \alpha_j + \alpha_s + \nu_{ejgs}, \end{aligned} \tag{8.1}$$

where e denotes an establishment, j its two-digit industry, g its governorate and s the census wave. The unit of observation is an establishment in a given wave, and the sample includes 232,636 observations. The outcome Y_{ejgs} is the female employment share of the establishment. $Z_{k(j)}$ is a characteristic of the industry, measured either by industry mixedness or occupational integration, which are defined in Section 4.2 (Table 4), or by one of the three O*NET measures of interaction content (Table 5). Post_s indicates the 2017–18 wave. Governorate, industry and wave fixed effects absorb, respectively, any persistent difference between governorates, the level of female employment specific to each industry, and any shock common to a census wave. As before, standard errors are clustered at the governorate level.

We report all establishment-level results for three sets of fixed effects, which correspond to the columns of each table. Column (1) includes census-wave and governorate fixed effects only. Column (2) adds two-digit industry fixed effects, so that we compare establishments in the same industry. Column (3) replaces them with a full set of industry $\times$ wave fixed effects, so that identification comes from variation across governorates within an industry and wave. Since the outcome is always the female employment share of the establishment, the three columns allow us to test whether the results depend on how we account for industry composition. The coefficient of interest is θ, and under the employer-cost channel, θ should be negative. That is, an employer facing a higher expected cost of contact between men and women should employ fewer women in industries where they would work with men.

8.2 Gender mixing and the female employment share

Table 4 reports the results for the two measures of gender mixing defined in Section 4.2, namely industry mixedness and occupational integration. Per standard deviation of mixedness, a one standard deviation increase in pre-law harassment reduces the female employment share of establishments by 0.0073 ($p < 0.01$), which represents about 6 percent of its 2013 mean. Per standard deviation of integration, the female employment share declines by 0.0070 ($p < 0.01$). Hence, with both measures, establishments in high-harassment governorates reduce their female employment share in industries that are more mixed and where men and women hold the same kinds of jobs. These results suggest that gender mixing at work is the relevant margin.

8.3 Interaction content and the female employment share

In Table 5, we use the three O*NET measures from the individual-level analysis (Table 3) and interact harassment and Post with the average score of each industry. We compute these averages over the employed workers of the Labor Force Survey in 2009–2013, before the law, so that they do not reflect the change in women's jobs after the law documented in Section 7. The three coefficients are negative, which implies that establishments in industries where occupations involve more work with other people reduce their female employment share where pre-law harassment was higher.

For teamwork, the estimate is -0.0069 with fixed effects for the census wave and the governorate, -0.0049 when we add two-digit industry fixed effects and -0.0051 with industry $\times$ wave fixed effects, all significant at the one percent level. For internal communication, the estimates are of similar size, -0.0062, -0.0056 and -0.0050 in the same three specifications, all significant at the five percent level. For contact with others, the estimates are -0.0060, -0.0060 and -0.0066, also significant at the one percent level.

Relative to a pre-law (2013) female employment share of 13 percent, these estimates represent a decline of 3.7 to 5.3 percent of the female employment share per standard deviation of the measure.

8.4 The sex of owners and managers

The main threat to the employer-cost interpretation is that the decline in the female employment share reflects women's own choices. As discussed in Section 3, a preference against gender-mixed work that existed before the law cannot explain the decline, and the law would have to have made women *more* reluctant to work with men than before, in the governorates where it made such work safer. Moreover, to explain the gradient we document below, women would have to have become more reluctant to work in establishments led by men in particular, although men in positions of authority are the ones the law punishes the most. The sex of the employer allows us to distinguish the two explanations. If the decline is driven by employers, it should be concentrated in establishments owned or managed by men. That is because owners and managers decide whom to hire, and Article 306-bis B increases the penalty when the offender holds a position of hierarchical authority over the victim, so the law puts male owners and managers at the highest legal risk. In contrast, a decline driven only by women's reluctance to work with men does not predict such a gradient. Therefore, we split the triple difference by the sex of the establishment's owners and, separately, by the sex of its managers, using the census head-counts of each. We call an establishment *male-led* if none of its owners (or, in the second split, none of its managers) is a woman, and *female-led* otherwise. To remove the mechanical correlation between the sex of the employer and the female employment share, the outcome is the female share of employees, excluding owners and managers.

Table 6 reports the results for owners and managers, for the three specifications. In establishments where no owner is a woman, the female share of employees declines relative to female-led establishments by 0.022 per standard deviation of pre-law harassment with census-wave and governorate fixed effects only, and by 0.030 when we include two-digit industry or industry $\times$ wave fixed effects. In establishments where no manager is a woman, it declines by 0.065 in the first specification, and by 0.051 and 0.060 in the other two.

The results for owners and managers go in the same direction and have a similar magnitude, between 2.2 and 6.5 percentage points of the female share of employees per standard deviation. The sign is the same in all six estimates, although they are imprecisely estimated.

Finally, we check that the results on gender mixing and interaction content are

not driven by shocks common to all establishments of a governorate. In Appendix Table A.7, we replace the governorate and census-wave fixed effects with a full set of governorate $\times$ wave fixed effects, which absorb any economic, political or administrative shock common to all establishments of a governorate in a census wave. The estimates remain essentially unchanged, at -0.0074 for mixedness and -0.0071 for integration.

9 Conclusion

In June 2014, Egypt criminalized sexual harassment, and the law affected women differently across the country, since it had more to change where harassment was more common before it was passed. Harassment reports declined the most in these governorates after the law, and national news coverage of arrests and prosecutions increased, which suggests that the law was effectively enforced. Comparing women to men across governorates with different levels of pre-law harassment, we find that the law successfully brought women into the labor force, but it also changed where they work. Relative to men, women become more likely to work in a governorate with higher pre-law harassment than their own, and the distance between the governorates where they live and work increases.

The law also changed the type of jobs women hold. In governorates with higher pre-law harassment, women moved into occupations that involve less teamwork, less internal communication and less contact with others, relative to men. Using the firm survey, we show that employers also responded. Where pre-law harassment was higher, the law lowered the female employment share of establishments in industries where men and women work together, relative to other industries. Our results indicate that this decline is larger in male-led establishments relative to female-led establishments, which suggests that employers are part of the response.

Our findings suggest that while anti-harassment policy increased women's willingness to work, it may have inadvertently reduced establishments' demand for female labor in gender-mixed industries. Since our results indicate that this decline is smaller in establishments with a woman among their owners or managers, policies against harassment may be more effective at increasing female labor-force participation when combined with measures that ease women's entry into entrepreneurship and, especially, the expansion of female-led firms. This is in line with Chiplunkar and Goldberg (2024), who show that female entrepreneurs are more likely to hire women and that addressing barriers to their growth can increase the demand for female labor. Whether this combination raises women's employment and improves their access to gender-mixed jobs is a question for future research.

Tables

Table 1: Triple Difference: Labor-Force Participation

Dependent variable:	Labor-force participation			
	(1)	(2)	(3)	(4)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	0.0224**	0.0230**	0.0232**	0.0234**
	(0.0100)	(0.0098)	(0.0100)	(0.0100)
	[0.033]	[0.028]	[0.029]	[0.028]
Wild cluster bootstrap p	{0.018}	{0.015}	{0.017}	{0.014}
N: Observations	2,506,902	2,506,897	2,506,897	2,506,897
Individual controls	N	Y	Y	Y
Female × year-quarter FE	Y	Y	Y	Y
Governorate FE	Y	Y	Y	Y†
Governorate linear trends	N	N	Y	N
Governorate × year-quarter FE	N	N	N	Y

Note. Table 1 reports the estimated coefficients for equation (6.1). Results are the triple-difference estimates of the effect of the law on women's labor-force participation under four specifications. Column (1) does not include individual controls. Column (2) adds the individual controls used throughout the paper, namely age, age^2, an ever-married indicator, an urban indicator and household size. Column (3) adds governorate-specific linear trends. Column (4) replaces the governorate fixed effects with a full set of governorate × year-quarter fixed effects, which absorb any shock common to women and men in a governorate and quarter. In the lower panel, a dagger indicates a set of fixed effects that is absorbed by a more saturated set of fixed effects in that column. The row *wild cluster bootstrap p* reports the p-value from a wild cluster bootstrap. All specifications include female × year-quarter fixed effects. Standard errors clustered at the governorate level (27 governorates) are in parentheses, and p-values are in brackets. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Triple Difference: Workplace Location

Panel A. Dependent variable:	Harassment share of the governorate of work			
	(1)	(2)	(3)	(4)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	0.0415***	0.0415***	0.0436***	0.0437***
	(0.0090)	(0.0091)	(0.0087)	(0.0086)
	[0.000]	[0.000]	[0.000]	[0.000]
Wild cluster bootstrap p	{0.000}	{0.000}	{0.001}	{0.001}
N: Observations	461,524	461,524	461,524	461,524
Panel B. Dependent variable:	Works in a higher-harassment governorate			
	(5)	(6)	(7)	(8)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	0.0178***	0.0178***	0.0191***	0.0188***
	(0.0060)	(0.0060)	(0.0056)	(0.0056)
	[0.007]	[0.007]	[0.002]	[0.002]
Wild cluster bootstrap p	{0.018}	{0.018}	{0.010}	{0.008}
N: Observations	461,524	461,524	461,524	461,524
Panel C. Dependent variable:	Distance to the governorate of work (km)			
	(9)	(10)	(11)	(12)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	4.3209**	4.3010**	3.7503**	3.6557**
	(1.6233)	(1.6461)	(1.5469)	(1.5372)
	[0.013]	[0.015]	[0.023]	[0.025]
Wild cluster bootstrap p	{0.004}	{0.005}	{0.013}	{0.019}
N: Observations	461,524	461,524	461,524	461,524
Individual controls	N	Y	Y	Y
Female × year-quarter FE	Y	Y	Y	Y
Governorate FE	Y	Y	Y	Y†
Governorate linear trends	N	N	Y	N
Governorate × year-quarter FE	N	N	N	Y

Note. Table 2 reports the triple-difference estimates of equation (6.1) for three measures of where a woman works. The sample consists of employed individuals who report the governorate of their workplace. In Panel A, the dependent variable is the harassment share of the governorate where the main job is located, standardized with the same 27-governorate moments as the treatment. In Panel B, the dependent variable is an indicator for working in a governorate with more harassment than the governorate of residence. In Panel C, the dependent variable is the great-circle distance in kilometers between the governorates of residence and work, which is zero for individuals who work in the governorate where they live. The workplace module is available in 2010, 2011, 2014 and 2016–2022, so the pre-law period is 2010–2011. Each dependent variable is estimated under the same four specifications. The first column does not include individual controls, and the second adds the individual controls used throughout the paper, namely age, age^2, an ever-married indicator, an urban indicator and household size. The third column adds governorate-specific linear trends, and the fourth replaces the governorate fixed effects with governorate × year-quarter fixed effects. In the lower panel, a dagger indicates a set of fixed effects that is absorbed by a more saturated set of fixed effects in that column. The row *wild cluster bootstrap* p reports the p-value from a wild cluster bootstrap. All specifications include female × year-quarter fixed effects. Standard errors clustered at the governorate level (27 governorates) are in parentheses, and p-values are in brackets. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Triple Difference: Interaction Content of Women's Occupations

Dependent variable:	Teamwork			
	(1)	(2)	(3)	(4)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	-0.0341***	-0.0310***	-0.0298**	-0.0288**
	(0.0112)	(0.0106)	(0.0112)	(0.0114)
	[0.005]	[0.007]	[0.013]	[0.018]
Wild cluster bootstrap p	{0.001}	{0.002}	{0.003}	{0.003}
N: Observations	1,043,289	1,043,289	1,043,289	1,043,289
Dependent variable:	Internal communication			
	(5)	(6)	(7)	(8)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	-0.0369***	-0.0344***	-0.0321***	-0.0312***
	(0.0108)	(0.0107)	(0.0109)	(0.0108)
	[0.002]	[0.003]	[0.007]	[0.008]
Wild cluster bootstrap p	{0.002}	{0.003}	{0.005}	{0.005}
N: Observations	1,043,289	1,043,289	1,043,289	1,043,289
Dependent variable:	Contact with others			
	(9)	(10)	(11)	(12)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	-0.0593**	-0.0507**	-0.0461**	-0.0459**
	(0.0233)	(0.0233)	(0.0221)	(0.0220)
	[0.017]	[0.039]	[0.047]	[0.047]
Wild cluster bootstrap p	{0.034}	{0.065}	{0.071}	{0.065}
N: Observations	1,043,289	1,043,289	1,043,289	1,043,289
Individual controls	N	Y	Y	Y
Female × year-quarter FE	Y	Y	Y	Y
Governorate FE	Y	Y	Y	Y†
Governorate linear trends	N	N	Y	N
Governorate × year-quarter FE	N	N	N	Y

Note. Table 3 reports the triple-difference estimates of equation (6.1) for the interaction content of a woman's occupation. The sample consists of employed individuals. We construct three occupation-level measures using O*NET, namely teamwork, internal communication and contact with others, which are described in Section 4 and standardized across occupations. Each coefficient is expressed in standard deviations of the measure per standard deviation of pre-law governorate harassment. The dependent variable is teamwork in columns (1)–(4), internal communication in columns (5)–(8) and contact with others in columns (9)–(12). Each outcome is estimated under the same four specifications. The first does not include individual controls, the second adds the individual controls used throughout the paper (age, age^2, an ever-married indicator, an urban indicator and household size), the third adds governorate-specific linear trends, and the fourth replaces the governorate fixed effects with a full set of governorate × year-quarter fixed effects. In the lower panel, a dagger indicates a set of fixed effects that is absorbed by a more saturated set of fixed effects in that column. The row *wild cluster bootstrap p* reports the p-value from a wild cluster bootstrap. All specifications include female × year-quarter fixed effects. Standard errors clustered at the governorate level (27 governorates) are in parentheses, and p-values are in brackets. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Triple Difference: Establishment Female Employment Share by Industry Gender Mixing

Dependent variable:	Female employment share		
	(1)	(2)	(3)
$\text{Intensity}_g \times \text{Post}_s \times \text{Mixedness}_{k(j)}$	-0.0073***	-0.0072***	-0.0068***
	(0.0017)	(0.0017)	(0.0013)
N: Observations	232,636	232,636	232,635
	(4)	(5)	(6)
$\text{Intensity}_g \times \text{Post}_s \times \text{Integration}_{k(j)}$	-0.0070***	-0.0093***	-0.0100***
	(0.0024)	(0.0023)	(0.0023)
N: Observations	232,636	232,636	232,635
Census wave FE	Y	Y	Y
Governorate FE	Y	Y	Y
Industry FE (2-digit)	N	Y	Y†
Industry $\times$ wave FE	N	N	Y

Note. Table 4 reports the estimated coefficients for equation (8.1). Results are the establishment-level triple-difference estimates on the female employment share of establishments, where harassment and Post are interacted with two measures of gender mixing, that is, of how much an industry puts men and women in contact. *Industry mixedness* is $1 - |0.5 - f_j|/0.5$, which equals one when an industry is evenly split between the sexes. *Occupational integration* is one minus the Duncan dissimilarity index between the occupational distributions of men and women across the eight occupation categories of the census. Both measures are fixed at their pre-law (2013) values, aggregated to the 18 one-digit sections and standardized with establishment weights. Columns (1)–(3) interact the treatment with mixedness, and columns (4)–(6) with occupational integration. Each measure is estimated under the same three specifications. The first includes census-wave and governorate fixed effects, the second adds two-digit industry fixed effects, and the third replaces them with industry $\times$ wave fixed effects. In the lower panel, a dagger indicates a set of fixed effects that is absorbed by a more saturated set of fixed effects in that column. Estimates are weighted with sampling weights, and standard errors clustered at the governorate level (27 governorates) are in parentheses. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Triple Difference: Establishment Female Employment Share by Industry Interaction Content

Dependent variable:	Female employment share		
	(1)	(2)	(3)
$\text{Intensity}_g \times \text{Post}_s \times \text{Teamwork}_{k(j)}$	-0.0069***	-0.0049***	-0.0051***
	(0.0019)	(0.0016)	(0.0014)
N: Observations	232,636	232,636	232,635
	(4)	(5)	(6)
$\text{Intensity}_g \times \text{Post}_s \times \text{Communication}_{k(j)}$	-0.0062**	-0.0056***	-0.0050***
	(0.0030)	(0.0016)	(0.0012)
N: Observations	232,636	232,636	232,635
	(7)	(8)	(9)
$\text{Intensity}_g \times \text{Post}_s \times \text{Contact}_{k(j)}$	-0.0060***	-0.0060***	-0.0066***
	(0.0020)	(0.0021)	(0.0021)
N: Observations	232,636	232,636	232,635
Census wave FE	Y	Y	Y
Governorate FE	Y	Y	Y
Industry FE (2-digit)	N	Y	Y†
Industry $\times$ wave FE	N	N	Y

Note. Table 5 reports the estimated coefficients for equation (8.1). Results are the establishment-level triple-difference estimates on the female employment share of establishments, where harassment and Post are interacted with three O*NET work-context measures of the interaction content of the industry. These are the same three measures as in the individual-level analysis of Table 3, namely teamwork in columns (1)–(3), internal communication in columns (4)–(6), and contact with others in columns (7)–(9). Each measure is the employment-weighted average of the scores of the occupations within the industry, over all employed Labor Force Survey respondents of 2009–2013, before the law, aggregated to the 18 one-digit sections and standardized with establishment weights. Each measure is estimated under the same three specifications. The first includes census-wave and governorate fixed effects, the second adds two-digit industry fixed effects, and the third replaces them with industry $\times$ wave fixed effects. In the lower panel, a dagger indicates a set of fixed effects that is absorbed by a more saturated set of fixed effects in that column. Estimates are weighted with sampling weights, and standard errors clustered at the governorate level (27 governorates) are in parentheses. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6: Triple Difference: Female Share of Employees by the Sex of Owners and Managers

Dependent variable:	Female share of employees		
	(1)	(2)	(3)
Owners: male-led (no female owner) vs. female-led			
$\text{Intensity}_g \times \text{Post}_s \times \text{MaleLed}_e$	-0.0224	-0.0297*	-0.0297*
	(0.0195)	(0.0160)	(0.0165)
N: Observations	159,072	159,072	159,069
	(4)	(5)	(6)
Managers: male-led (no female manager) vs. female-led			
$\text{Intensity}_g \times \text{Post}_s \times \text{MaleLed}_e$	-0.0652**	-0.0513	-0.0603
	(0.0255)	(0.0327)	(0.0356)
N: Observations	34,832	34,832	34,830
Census wave FE	Y	Y	Y
Governorate FE	Y	Y	Y
Industry FE (2-digit)	N	Y	Y†
Industry × wave FE	N	N	Y

Note. Table 6 reports the coefficient on the triple interaction of equation (8.1), $\text{Intensity}_g \times \text{Post}_s \times \text{MaleLed}_e$. MaleLed_e is an indicator for establishments with no female owner (columns 1–3) or no female manager (columns 4–6), based on the census head-counts of female and male owners and of female and male managers. An establishment is considered female-led if at least one of its owners (managers) is a woman. Hence, the coefficient measures the differential response of male-led establishments relative to female-led establishments, which are the omitted category. The dependent variable is the female share of employees, excluding owners and managers. Each split is estimated under the same three specifications as in Tables 4 and 5. Columns (1) and (4) include census-wave and governorate fixed effects, columns (2) and (5) add two-digit industry fixed effects, and columns (3) and (6) replace them with industry × wave fixed effects. In the lower panel, a dagger indicates a set of fixed effects that is absorbed by a more saturated set of fixed effects in that column. Estimates are weighted with sampling weights, and standard errors clustered at the governorate level (27 governorates) are in parentheses. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figures

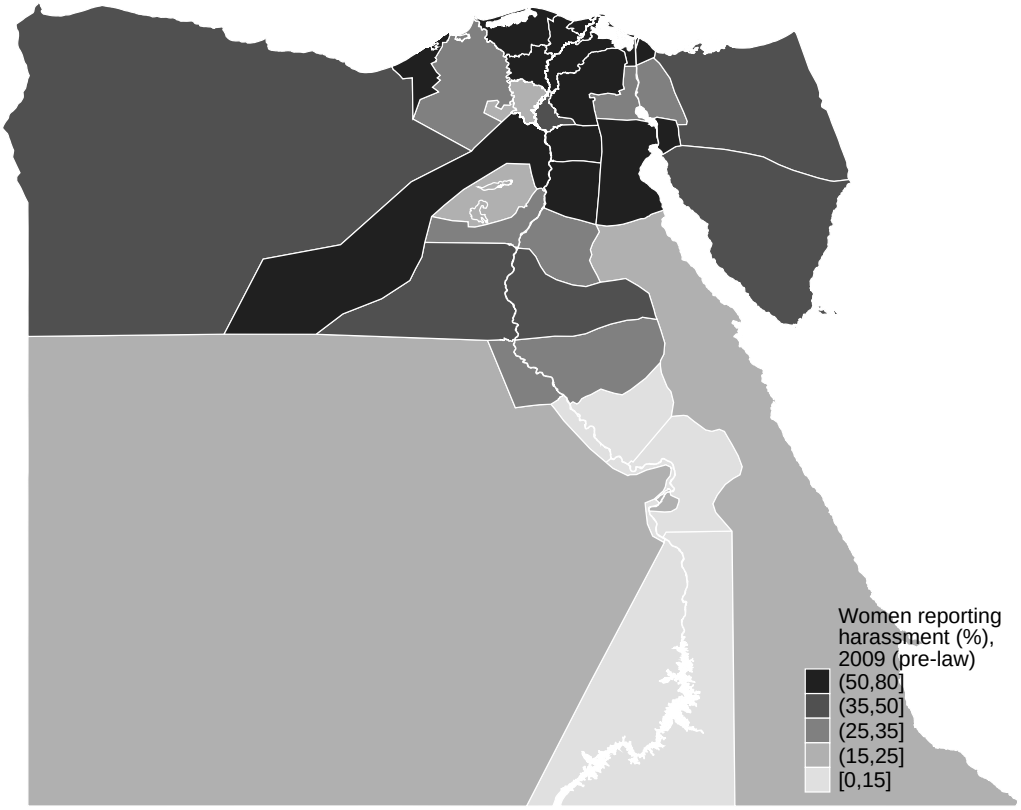

Figure 1: Sexual-Harassment Prevalence across Egypt's Governorates, 2009 (Pre-Law)

Note. Figure 1 maps Intensity$_g$, that is, the share of female respondents who report having ever experienced sexual harassment, in each of Egypt's 27 governorates. The share is computed from the 2009 Survey of Young People in Egypt, where the harassment module was only administered to women. The shading uses five gray bands (up to 15, 15–25, 25–35, 35–50 and above 50 percent), and darker colors indicate higher prevalence. The standardized version of this measure, computed across the 27 governorates, is the treatment Intensity$_g$ used throughout the paper. Boundaries are administrative level 1.



(a) Original (Arabic)



(b) English translation

Figure 2: An Illustrative *Youm7* Article

Note. Figure 2 shows the *Youm7* article discussed in the text, "Know your rights: seven years in prison is the penalty for harassment at work" (January 6, 2024), in the original Arabic (panel a) and in English translation (panel b). The classification is applied to the headline. Since the headline mentions both the setting (*at work*) and the sanction (*seven years in prison*), the article would be classified as both workplace and enforcement coverage in the series of Section 5.1, which cover 2009–2018. The publication date is obtained from the article identifier.

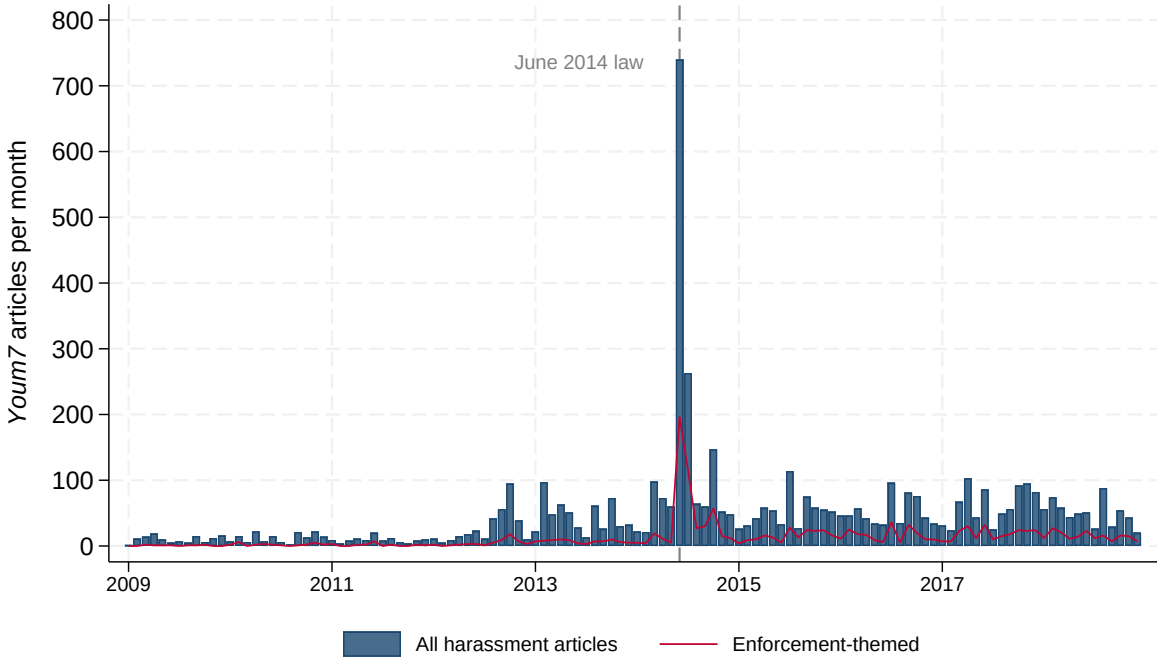

Figure 3: Adoption and Enforcement of the 2014 Law: Harassment News Coverage

Note. Figure 3 plots the monthly number of harassment articles published by *Youm7* between 2009 and 2018. The bars report all the articles published in the month whose headline mentions harassment, and the line reports the articles whose headline mentions enforcement (arrest, prosecution, detention, court, verdict, public prosecution, police). The dashed vertical line indicates the June 2014 law.

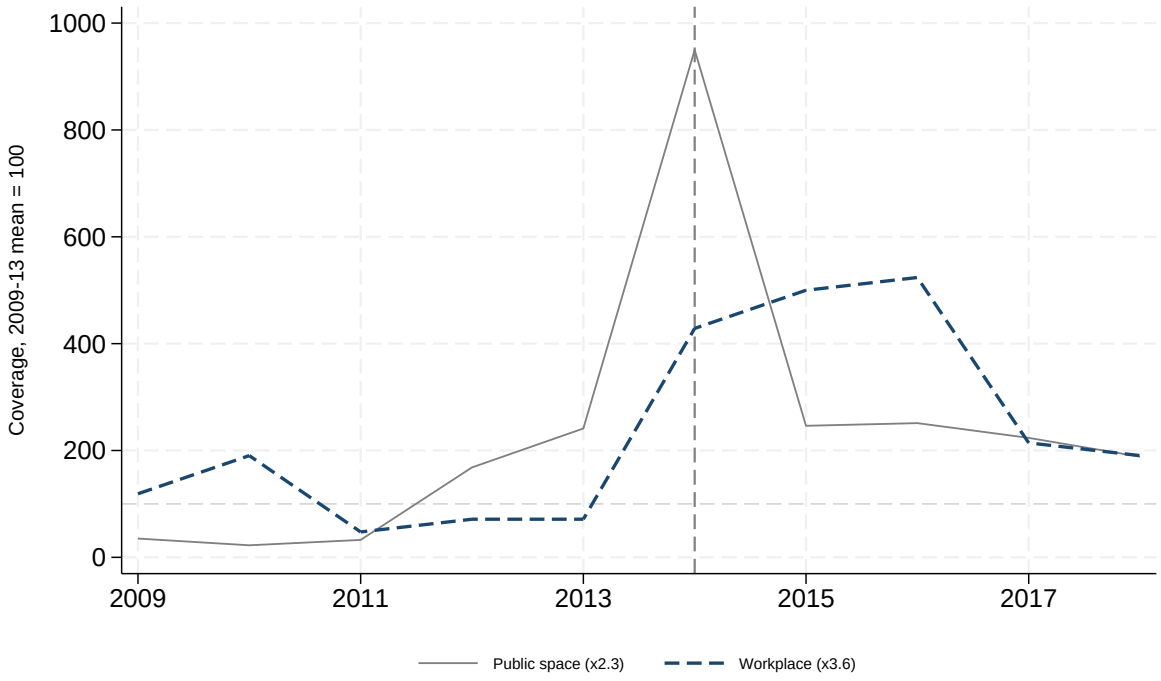

Figure 4: Harassment News Coverage by Setting: Public Space and Workplace

Note. Figure 4 plots the annual coverage of harassment in *Youm7* between 2009 and 2018, by setting. Headlines about street and public space (including transport) are classified using an Arabic keyword list. Workplace headlines are coded by hand and include only stories about Egypt in which the harassment happens at the victim's work or between people linked by work (Appendix B.3). The y-axis reports an index of coverage, where the pre-law (2009–2013) mean of each setting equals 100, which corresponds to 39.8 headlines a year for public space and 4.2 for the workplace. The thin horizontal line indicates 100. The gray solid line is public space and the navy long-dashed line is the workplace. In the legend, the multiplier next to each setting is its average post-law (2015–2018) coverage divided by its pre-law average. The dashed vertical line indicates 2014, the year of the law.



Figure 5: The HarassMap Public Map

Note. Figure 5 shows the public map where HarassMap displays the reports it receives. Circles group individual reports by location, and the number inside each circle is the number of reports in the cluster. The legend distinguishes incidents that were reported from incidents in which a bystander is recorded as having intervened. Our measure is built from the individual report records shown in Figure 6. We take the date and the coordinates of each report, assign it to one of the 27 governorates, and count the number of reports per governorate-year over 2010–2020. The screenshot shows the platform as retrieved in 2026.

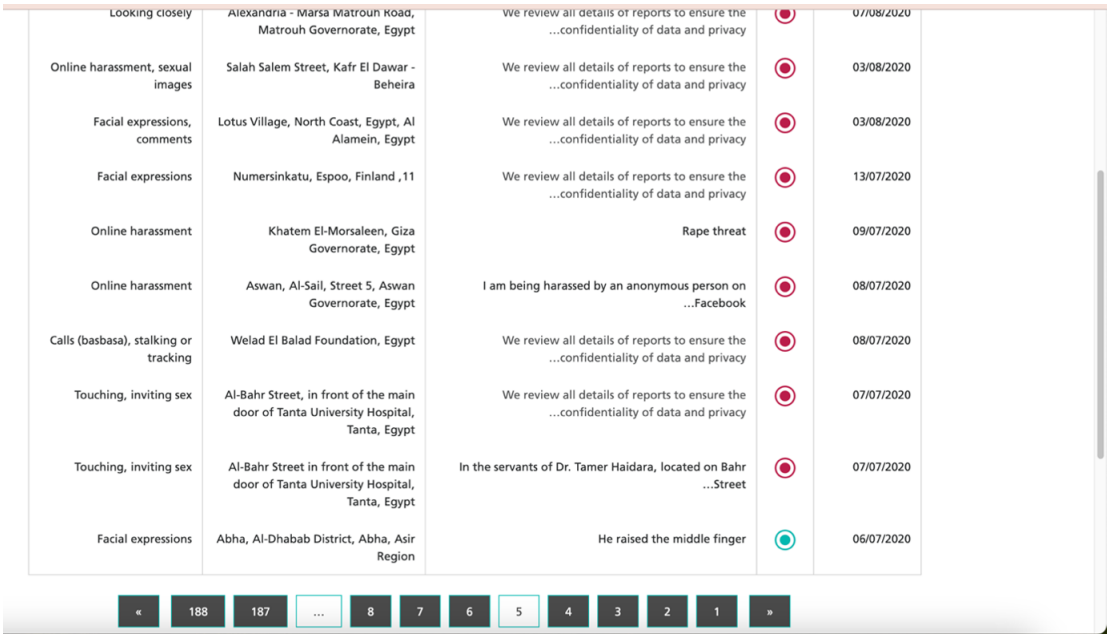

Figure 6: Individual HarassMap Reports

Note. Figure 6 shows the individual report records used to build the HarassMap panel. Each row corresponds to one report and has four fields, namely the category of harassment (for example "touching, inviting sex", "online harassment", "facial expressions"), a free-text location, a description (which is often withheld and replaced by a confidentiality notice) and the date. The archive contains 188 such pages. We only use the date and the location, and we assign each report to one of Egypt's 27 governorates using its coordinates. The screenshot shows the platform as retrieved in 2026.

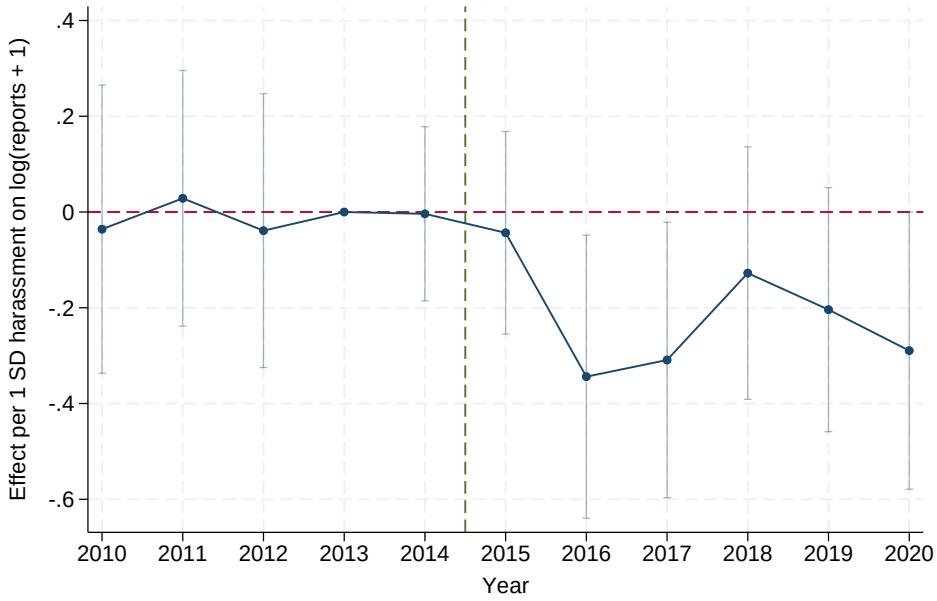

Figure 7: Event Study: HarassMap Reports and Pre-Law Harassment

Note. Figure 7 reports the coefficients λ_k of equation (5.1), estimated on a balanced governorate-year panel with 2013 as the reference year. Intensity$_g$ is the standardized SYPE 2009 governorate harassment share, so each coefficient is the differential change in log reports per standard deviation of pre-law harassment. The panel includes all 27 governorates over 2010–2020, and governorate-year cells with no reports are set to zero. All estimates include governorate and year fixed effects. The capped vertical bars show 90% confidence intervals calculated using standard errors clustered at the governorate level and a t distribution with 26 degrees of freedom.

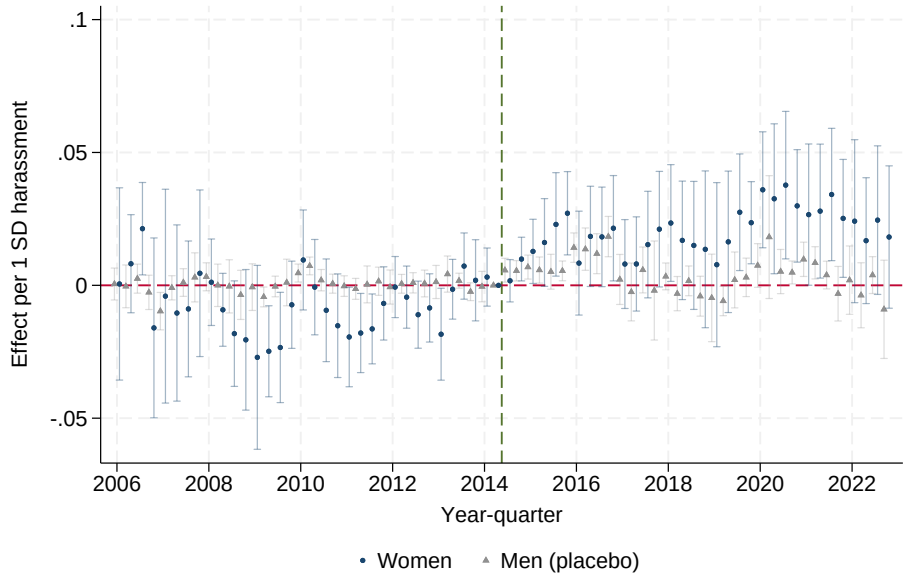

Figure 8: Event Study: Labor-Force Participation

Note. Figure 8 reports the coefficients from equation (6.2) for labor-force participation, estimated separately for women and men with specification (1) of Table 1, that is, without individual controls and with governorate fixed effects. The y-axis reports the coefficient on Intensity$_g$ interacted with each quarter, expressed per standard deviation of pre-law governorate harassment. The x-axis reports the calendar year-quarter. Navy circles are the estimates for women and gray triangles the estimates for men, which serve as a placebo, and the two series are shifted horizontally by a fifth of a quarter. The capped vertical bars show 90% confidence intervals calculated using standard errors clustered at the governorate level (27 governorates). The dashed horizontal line indicates zero, the dashed vertical line indicates the June 2014 law, and the reference period is 2014q2, the quarter before the law. The sample consists of adults aged 25–65.

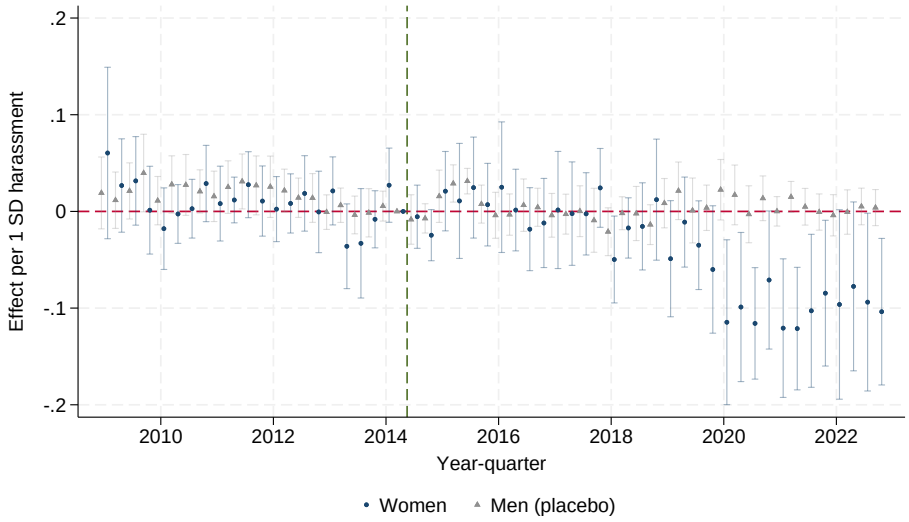

Figure 9: Event Study: Teamwork

Note. Figure 9 reports the coefficients from equation (6.2) for teamwork, standardized across occupations. The coefficients are estimated separately for women and men with specification (1) of Table 3, that is, without individual controls and with governorate fixed effects, on employed individuals. The y-axis reports the coefficient on Intensity$_g$ interacted with each quarter, in standard deviations of the attribute per standard deviation of pre-law governorate harassment. The x-axis reports the calendar year-quarter. Navy circles are the estimates for women and gray triangles the estimates for men, which serve as a placebo, and the two series are shifted horizontally by a fifth of a quarter. The capped vertical bars show 90% confidence intervals calculated using standard errors clustered at the governorate level (27 governorates). The dashed horizontal line indicates zero, the dashed vertical line indicates the June 2014 law, and the reference period is 2014q2. The series starts in 2009.

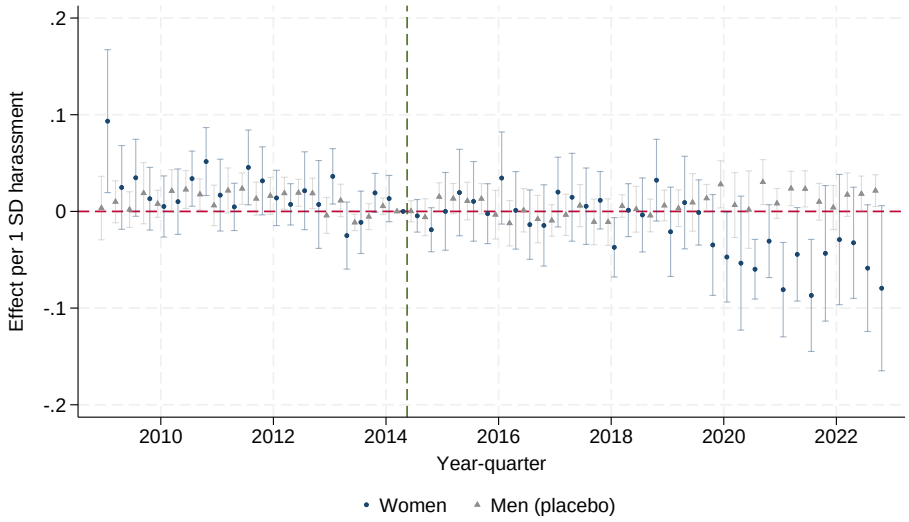

Figure 10: Event Study: Internal Communication

Note. Figure 10 reports the coefficients from equation (6.2) for internal communication, standardized across occupations. The coefficients are estimated separately for women and men with specification (1) of Table 3, that is, without individual controls and with governorate fixed effects, on employed individuals. The y-axis reports the coefficient on Intensity$_g$ interacted with each quarter, in standard deviations of the attribute per standard deviation of pre-law governorate harassment. The x-axis reports the calendar year-quarter. Navy circles are the estimates for women and gray triangles the estimates for men, which serve as a placebo, and the two series are shifted horizontally by a fifth of a quarter. The capped vertical bars show 90% confidence intervals calculated using standard errors clustered at the governorate level (27 governorates). The dashed horizontal line indicates zero, the dashed vertical line indicates the June 2014 law, and the reference period is 2014q2. The series starts in 2009.

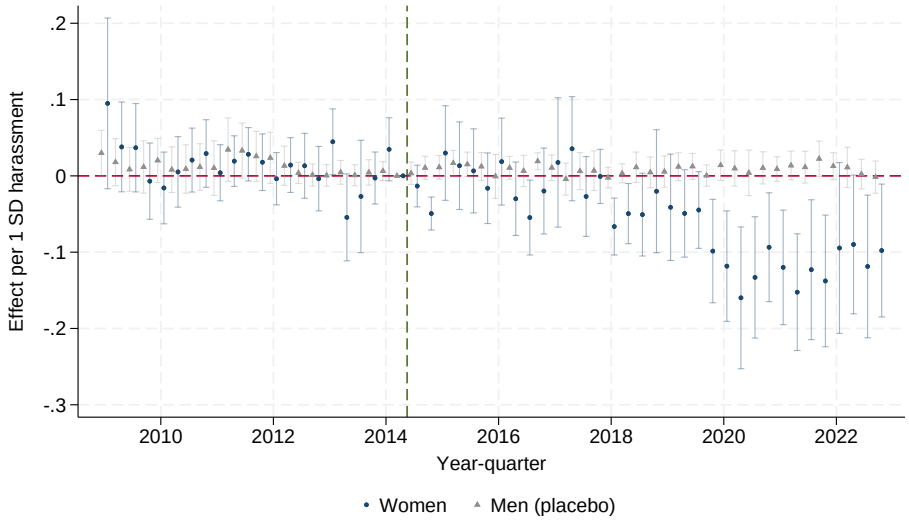

Figure 11: Event Study: Contact with Others

Note. Figure 11 reports the coefficients from equation (6.2) for contact with others, standardized across occupations. The coefficients are estimated separately for women and men with specification (1) of Table 3, that is, without individual controls and with governorate fixed effects, on employed individuals. The y-axis reports the coefficient on Intensity_g interacted with each quarter, in standard deviations of the attribute per standard deviation of pre-law governorate harassment. The x-axis reports the calendar year-quarter. Navy circles are the estimates for women and gray triangles the estimates for men, which serve as a placebo, and the two series are shifted horizontally by a fifth of a quarter. The capped vertical bars show 90% confidence intervals calculated using standard errors clustered at the governorate level (27 governorates). The dashed horizontal line indicates zero, the dashed vertical line indicates the June 2014 law, and the reference period is 2014q2. The series starts in 2009.

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Safer Streets, Re-Segregated Workplaces

Suzanna Khalifa and Sampreet Singh Goraya

Online Appendix

A Additional Results

Table A.1: Few-Cluster Inference: Cluster-Robust versus Wild Cluster Bootstrap

	Estimate	*p*-value	
		Cluster-robust	Wild cluster bootstrap
	(1)	(2)	(3)
Labor-force participation	0.0230**	0.028	0.015
Harassment share of the governorate of work	0.0415***	0.000	0.000
Works in a higher-harassment governorate	0.0178***	0.007	0.018
Distance to the governorate of work (km)	4.3010**	0.015	0.005
Teamwork	-0.0310***	0.007	0.002
Internal communication	-0.0344***	0.003	0.003
Contact with others	-0.0507**	0.039	0.065

Note. Table A.1 reports inference with few clusters for every main coefficient of equation (6.1), including contact with others. Column (1) reports the triple-difference estimate, and column (2) the cluster-robust *p*-value. Column (3) reports the wild cluster bootstrap *p*-value. The three columns are based on the same specification, which includes the individual controls, governorate fixed effects and female $\times$ year-quarter fixed effects, with standard errors clustered at the governorate level (27 governorates). See MacKinnon, Nielsen, and Webb (2023). Significant at *** p$<$0.01, ** p$<$0.05, * p$<$0.1.

Table A.2: Triple Difference: District-Level Harassment Share, Labor-Force Participation

Treatment defined at the level of:	Governorate	District	
			+ Governorate × year-quarter FE
	(1)	(2)	(3)
Dependent variable: Labor-force participation			
Intensity$_d$ × Post$_t$ × Female$_i$	0.0230**	0.0189***	0.0190***
	(0.0098)	(0.0056)	(0.0057)
	[0.028]	[0.001]	[0.001]
N: Observations	2,506,897	1,336,919	1,336,919
N: Clusters	27	219	219
Treatment measured at	Governorate	District	District
Individual controls	Y	Y	Y
Female × year-quarter FE	Y	Y	Y
Governorate FE	Y	Y†	Y†
District FE	N	Y	Y
Governorate × year-quarter FE	N	N	Y
Clustered on	Governorate	District	District

Note. Table A.2 reports the triple-difference estimates of equation (6.1) for labor-force participation, where the SYPE 2009 harassment share is measured at the district level. The dependent variable is an indicator for being in the labor force. Column (1) reports the governorate-level baseline from Table 1 in the main text. Column (2) uses the district-level treatment and district fixed effects. Column (3) additionally includes a full set of governorate × year-quarter fixed effects, which absorb any shock common to women and men in a governorate and quarter. In the lower panel, a dagger indicates a set of fixed effects that is absorbed by a more saturated set of fixed effects in that column. All columns include the individual controls (age, age^2, an ever-married indicator, an urban indicator and household size) and female × year-quarter fixed effects. SYPE provides a district share for 222 of Egypt's 365 districts, and the Labor Force Survey identifies the district of residence in 2006–2010, 2012–2015 and 2017–2018, which are the years used in columns (2) and (3). The sample consists of adults aged 25–65. Intensity$_d$ is the standardized governorate share in column (1) and the standardized district share in columns (2) and (3), so that the coefficient captures the change in the gap between women and men after the law across places with different levels of pre-law harassment. Standard errors (in parentheses) and p-values (in brackets) are clustered at the governorate level (27 governorates) in column (1) and at the district level (219 districts) in columns (2) and (3). Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Triple Difference: District-Level Harassment Share, Workplace Location

Treatment defined at the level of:	Governorate	District	
			+ Governorate × year-quarter FE
	(1)	(2)	(3)
Dependent variable: Harassment share of the governorate of work			
$\text{Intensity}_d \times \text{Post}_t \times \text{Female}_i$	0.0415***	0.0364***	0.0374***
	(0.0091)	(0.0072)	(0.0077)
	[0.000]	[0.000]	[0.000]
N: Observations	461,524	177,880	177,880
N: Clusters	27	218	218
	(4)	(5)	(6)
Dependent variable: Works in a higher-harassment governorate			
$\text{Intensity}_d \times \text{Post}_t \times \text{Female}_i$	0.0178***	0.0175***	0.0186***
	(0.0060)	(0.0036)	(0.0040)
	[0.007]	[0.000]	[0.000]
N: Observations	461,524	177,880	177,880
N: Clusters	27	218	218
	(7)	(8)	(9)
Dependent variable: Distance to the governorate of work (km)			
$\text{Intensity}_d \times \text{Post}_t \times \text{Female}_i$	4.3010**	4.8632***	4.4576***
	(1.6461)	(1.0348)	(0.9741)
	[0.015]	[0.000]	[0.000]
N: Observations	461,524	177,880	177,880
N: Clusters	27	218	218
Treatment measured at	Governorate	District	District
Individual controls	Y	Y	Y
Female × year-quarter FE	Y	Y	Y
Governorate FE	Y	Y†	Y†
District FE	N	Y	Y
Governorate × year-quarter FE	N	N	Y
Clustered on	Governorate	District	District

Note. Table A.3 reproduces Table A.2 for the three workplace-location outcomes. The dependent variable is the standardized harassment share of the governorate of work in columns (1)–(3), an indicator for working in a governorate with more harassment than the governorate of residence in columns (4)–(6), and the great-circle distance in kilometers between the governorate of residence and the governorate of work in columns (7)–(9). For each outcome, the three columns correspond to the specifications of Table A.2, with the same controls, fixed effects and years. The sample consists of employed individuals in the quarters in which the workplace module was collected. Standard errors (in parentheses) and p-values (in brackets) are clustered at the governorate level (27 governorates) in columns (1), (4) and (7), and at the district level (218 districts) in the other columns. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Triple Difference: Dropping the Revolution Years (2011–2013)

	Full window 2006–2022 (1)	Excluding 2011–2013 (2)
Labor-force participation	0.0230**	0.0219**
	(0.0098)	(0.0092)
N: Observations	2,506,897	2,023,588
Harassment share of the governorate of work	0.0415***	0.0407***
	(0.0091)	(0.0094)
N: Observations	461,524	392,983
Works in a higher-harassment governorate	0.0178***	0.0160**
	(0.0060)	(0.0061)
N: Observations	461,524	392,983
Distance to the governorate of work (km)	4.3010**	3.8568**
	(1.6461)	(1.5764)
N: Observations	461,524	392,983
Teamwork	-0.0310***	-0.0338**
	(0.0106)	(0.0141)
N: Observations	1,043,289	792,226
Internal communication	-0.0344***	-0.0399***
	(0.0107)	(0.0127)
N: Observations	1,043,289	792,226
Contact with others	-0.0507**	-0.0567**
	(0.0233)	(0.0265)
N: Observations	1,043,289	792,226

Note. Table A.4 reports the estimates of equation (6.1) with 2006–2010 as the pre-period and 2014–2022 as the post-period. Column (1) reports the estimates on the full sample period, and column (2) on 2006–2010 and 2014–2022. The rows correspond to labor-force participation, the three workplace-location outcomes and the three O*NET measures of the interaction content of the occupation, including contact with others. All specifications include the individual controls, governorate fixed effects and female $\times$ year-quarter fixed effects. Standard errors clustered at the governorate level (27 governorates) are in parentheses. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.5: Triple Difference: Sampling-Weighted Estimates, Participation and Workplace Location

Dependent variable:	Labor-force participation			
	(1)	(2)	(3)	(4)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	0.0238**	0.0238**	0.0239**	0.0242**
	(0.0110)	(0.0109)	(0.0111)	(0.0111)
	[0.040]	[0.038]	[0.040]	[0.038]
N: Observations	2,505,853	2,505,848	2,505,848	2,505,848
Dependent variable:	Harassment share of the governorate of work			
	(5)	(6)	(7)	(8)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	0.0528***	0.0524***	0.0549***	0.0546***
	(0.0092)	(0.0094)	(0.0090)	(0.0088)
	[0.000]	[0.000]	[0.000]	[0.000]
N: Observations	461,197	461,197	461,197	461,197
Dependent variable:	Works in a higher-harassment governorate			
	(9)	(10)	(11)	(12)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	0.0242***	0.0239***	0.0255***	0.0250***
	(0.0063)	(0.0063)	(0.0057)	(0.0056)
	[0.001]	[0.001]	[0.000]	[0.000]
N: Observations	461,197	461,197	461,197	461,197
Dependent variable:	Distance to the governorate of work (km)			
	(13)	(14)	(15)	(16)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	5.4669***	5.3574***	4.7912***	4.5963***
	(1.6235)	(1.6148)	(1.5366)	(1.5208)
	[0.002]	[0.003]	[0.004]	[0.006]
N: Observations	461,197	461,197	461,197	461,197
Individual controls	N	Y	Y	Y
Female × year-quarter FE	Y	Y	Y	Y
Governorate FE	Y	Y	Y	Y†
Governorate linear trends	N	N	Y	N
Governorate × year-quarter FE	N	N	N	Y
Sampling weights	Y	Y	Y	Y

Note. Table A.5 reports the triple-difference estimates of equation (6.1) for participation and the three workplace-location outcomes, using the sampling weights of the Labor Force Survey. The dependent variable is an indicator for labor-force participation in columns (1)–(4), the standardized harassment share of the governorate of work in columns (5)–(8), an indicator for working in a governorate with more harassment than the governorate of residence in columns (9)–(12), and the great-circle distance in kilometers between the governorate of residence and the governorate of work in columns (13)–(16). For each outcome, the first column does not include individual controls, and the second adds age, age^2, an ever-married indicator, an urban indicator and household size. The third column adds governorate-specific linear trends, and the fourth replaces the governorate fixed effects with governorate × year-quarter fixed effects. In the lower panel, a dagger indicates a set of fixed effects that is absorbed by a more saturated set of fixed effects in that column. Participation is estimated on all adults aged 25–65, and the location outcomes on employed individuals in the quarters in which the workplace module was collected. Sixteen of the 2,507,489 observations have no sampling weight and are dropped from the weighted estimates. All specifications include female × year-quarter fixed effects. Standard errors clustered at the governorate level (27 governorates) are in parentheses, and p-values are in brackets. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Triple Difference: Sampling-Weighted Estimates, Interaction Content

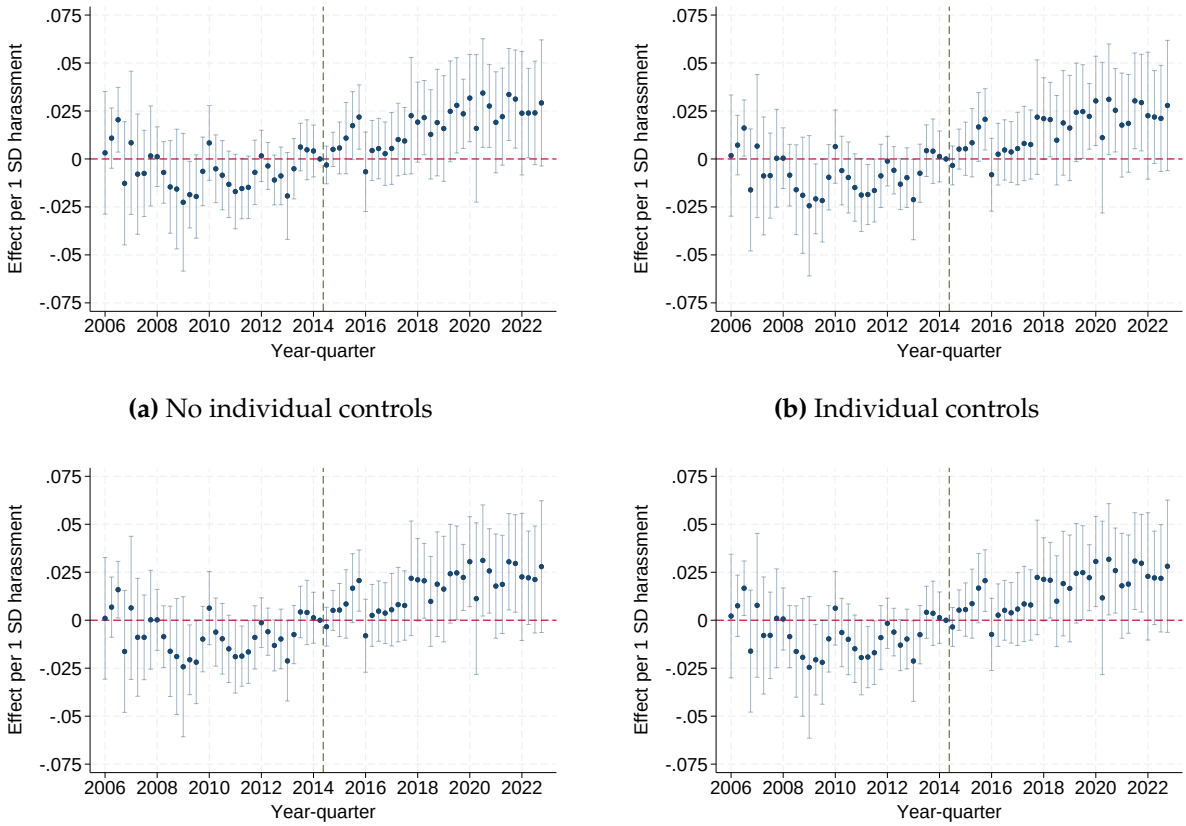
Dependent variable:	Teamwork			
	(1)	(2)	(3)	(4)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	-0.0342***	-0.0310***	-0.0295***	-0.0279***
	(0.0093)	(0.0087)	(0.0093)	(0.0095)
	[0.001]	[0.001]	[0.004]	[0.007]
N: Observations	1,042,799	1,042,799	1,042,799	1,042,799
Dependent variable:	Internal communication			
	(5)	(6)	(7)	(8)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	-0.0349***	-0.0307***	-0.0284***	-0.0269**
	(0.0095)	(0.0099)	(0.0101)	(0.0101)
	[0.001]	[0.004]	[0.009]	[0.013]
N: Observations	1,042,799	1,042,799	1,042,799	1,042,799
Dependent variable:	Contact with others			
	(9)	(10)	(11)	(12)
$\text{Intensity}_g \times \text{Post}_t \times \text{Female}_i$	-0.0565**	-0.0480**	-0.0431*	-0.0433**
	(0.0221)	(0.0225)	(0.0210)	(0.0209)
	[0.017]	[0.042]	[0.050]	[0.048]
N: Observations	1,042,799	1,042,799	1,042,799	1,042,799
Individual controls	N	Y	Y	Y
Female × year-quarter FE	Y	Y	Y	Y
Governorate FE	Y	Y	Y	Y†
Governorate linear trends	N	N	Y	N
Governorate × year-quarter FE	N	N	N	Y
Sampling weights	Y	Y	Y	Y

Note. Table A.6 reproduces Table A.5 for the three O*NET measures of the interaction content of a woman's occupation, each standardized across occupations. The dependent variable is teamwork in columns (1)–(4), internal communication in columns (5)–(8) and contact with others in columns (9)–(12). For each outcome, the four columns correspond to the four specifications of Table A.5, in the same order. The sample consists of employed individuals with a four-digit occupation code. All specifications include female × year-quarter fixed effects. Standard errors clustered at the governorate level (27 governorates) are in parentheses, and p-values are in brackets. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Triple Difference: Establishment Results with Governorate × Wave Fixed Effects

Dependent variable:	Female employment share		
	(1)	(2)	(3)
$\text{Intensity}_g \times \text{Post}_s \times \text{Mixedness}_{k(j)}$	-0.0074***	-0.0073***	-0.0069***
	(0.0017)	(0.0017)	(0.0014)
$\text{Intensity}_g \times \text{Post}_s \times \text{Integration}_{k(j)}$	-0.0071***	-0.0091***	-0.0098***
	(0.0024)	(0.0022)	(0.0022)
$\text{Intensity}_g \times \text{Post}_s \times \text{Teamwork}_{k(j)}$	-0.0068***	-0.0047***	-0.0049***
	(0.0018)	(0.0016)	(0.0014)
$\text{Intensity}_g \times \text{Post}_s \times \text{Communication}_{k(j)}$	-0.0062**	-0.0056***	-0.0050***
	(0.0030)	(0.0017)	(0.0012)
$\text{Intensity}_g \times \text{Post}_s \times \text{Contact}_{k(j)}$	-0.0059***	-0.0056**	-0.0063***
	(0.0019)	(0.0020)	(0.0019)
N: Observations	232,636	232,636	232,635
Governorate × wave FE	Y	Y	Y
Census wave FE	Y†	Y†	Y†
Governorate FE	Y†	Y†	Y†
Industry FE (2-digit)	N	Y	Y†
Industry × wave FE	N	N	Y

Note. Table A.7 reproduces Tables 4 and 5 in the main text with a full set of governorate × census-wave fixed effects, which absorb any shock common to all establishments of a governorate in a wave, including $\text{Intensity}_g \times \text{Post}_s$. The coefficient on the triple interaction is therefore identified only from differences across industries within a governorate and wave. Column (1) includes governorate × wave fixed effects only, column (2) adds two-digit industry fixed effects, and column (3) replaces them with industry × wave fixed effects. The dependent variable is the female employment share of the establishment, and the industry characteristic is mixedness or integration (Table 4) or one of the three O*NET measures of interaction content (Table 5), each averaged to the one-digit section $k(j)$ and standardized with establishment weights. In the lower panel, a dagger indicates a set of fixed effects that is absorbed by a more saturated set of fixed effects in that column. Estimates are weighted with sampling weights, and standard errors clustered at the governorate level (27 governorates) are in parentheses. Significant at *** p<0.01, ** p<0.05, * p<0.1.



(a) No individual controls

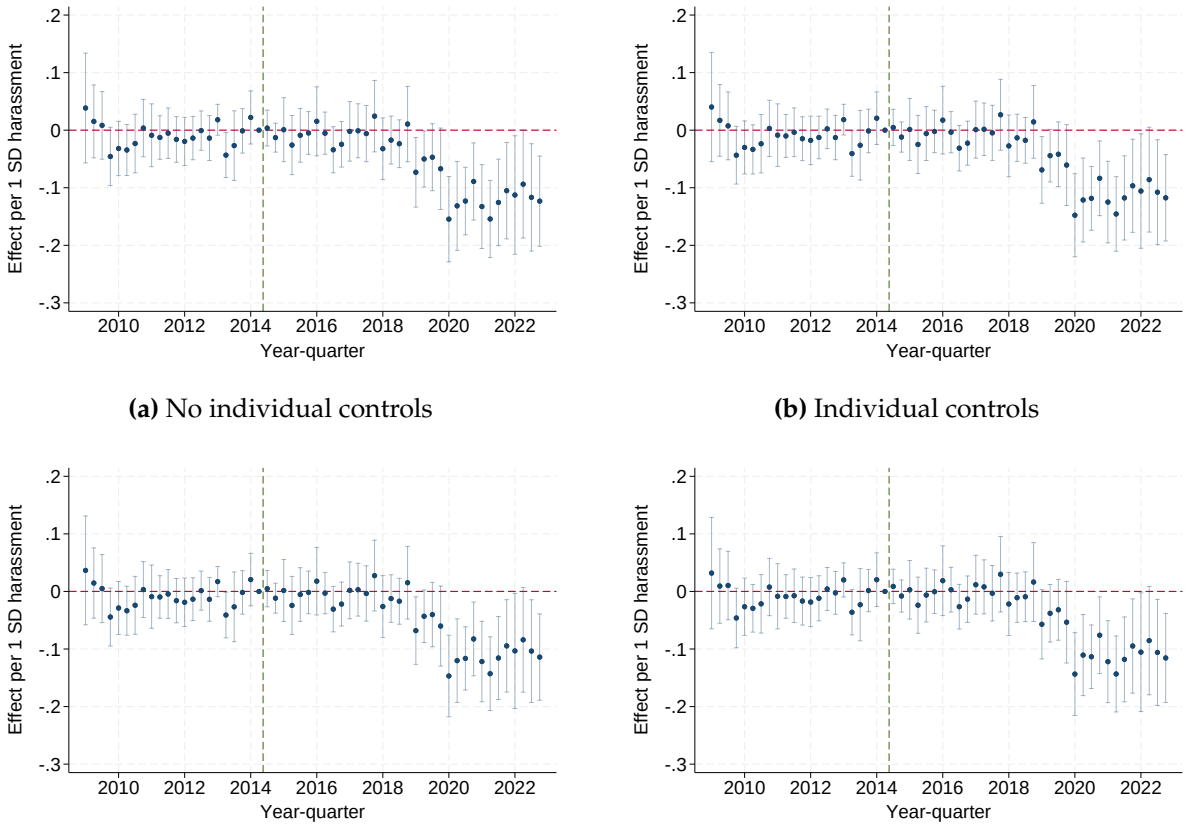
(b) Individual controls

(c) Individual controls and governorate linear trends

(d) Individual controls and governorate × year-quarter fixed effects

Figure A.1: Event Studies under the Four Specifications: Labor-Force Participation

Note. Figure A.1 reports the triple-difference event-study coefficients of the pooled version of equation (6.2), in which each quarter is interacted with Intensity$_g$ × Female$_i$, for labor-force participation under each of the four specifications of Table 1 in the main text, in separate panels. The y-axis reports the coefficient on Intensity$_g$ ×Female$_i$ interacted with each quarter, expressed per standard deviation of pre-law governorate harassment. The x-axis reports the calendar year-quarter. Markers are point estimates and capped spikes are 90% confidence intervals. The reference period is 2014q2, the quarter before the law, the dashed vertical line indicates the June 2014 law and the dashed horizontal line indicates zero. Panel (a) does not include individual controls, and panel (b) adds age, age^2, an ever-married indicator, an urban indicator and household size. Panel (c) adds governorate-specific linear trends to panel (b), and panel (d) replaces the governorate fixed effects of panel (b) with governorate × year-quarter fixed effects. The four panels have the same vertical scale. All specifications include female × year-quarter fixed effects. The sample consists of adults aged 25–65, and standard errors are clustered at the governorate level (27 governorates).



(a) No individual controls

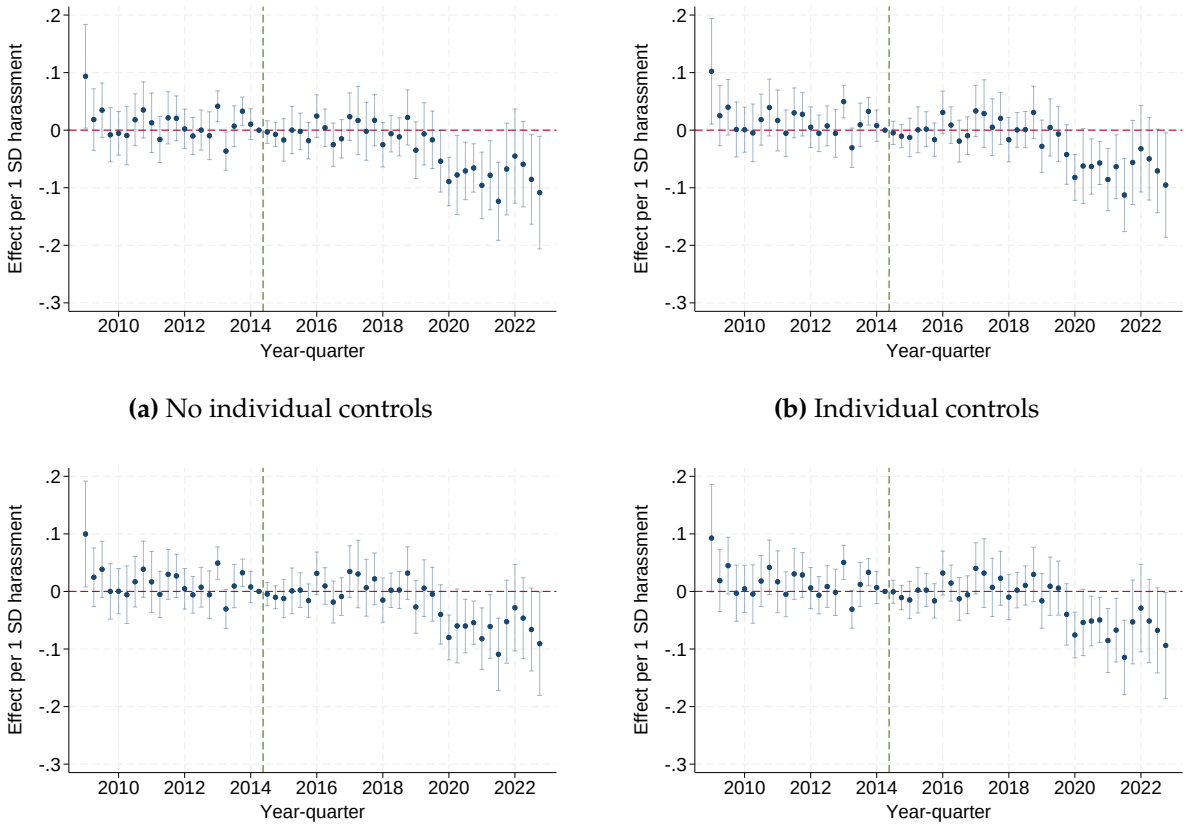
(b) Individual controls

(c) Individual controls and governorate linear trends

(d) Individual controls and governorate × year-quarter fixed effects

Figure A.2: Event Studies under the Four Specifications: Teamwork

Note. Figure A.2 reproduces Figure A.1 for teamwork, standardized across occupations, under the four specifications of Table 3 in the main text. The panels, axes, markers and confidence intervals are the same as in Figure A.1, and the y-axis is expressed in standard deviations of the attribute. The sample consists of employed individuals with a four-digit occupation code, from 2009 onwards, and standard errors are clustered at the governorate level (27 governorates).



(a) No individual controls

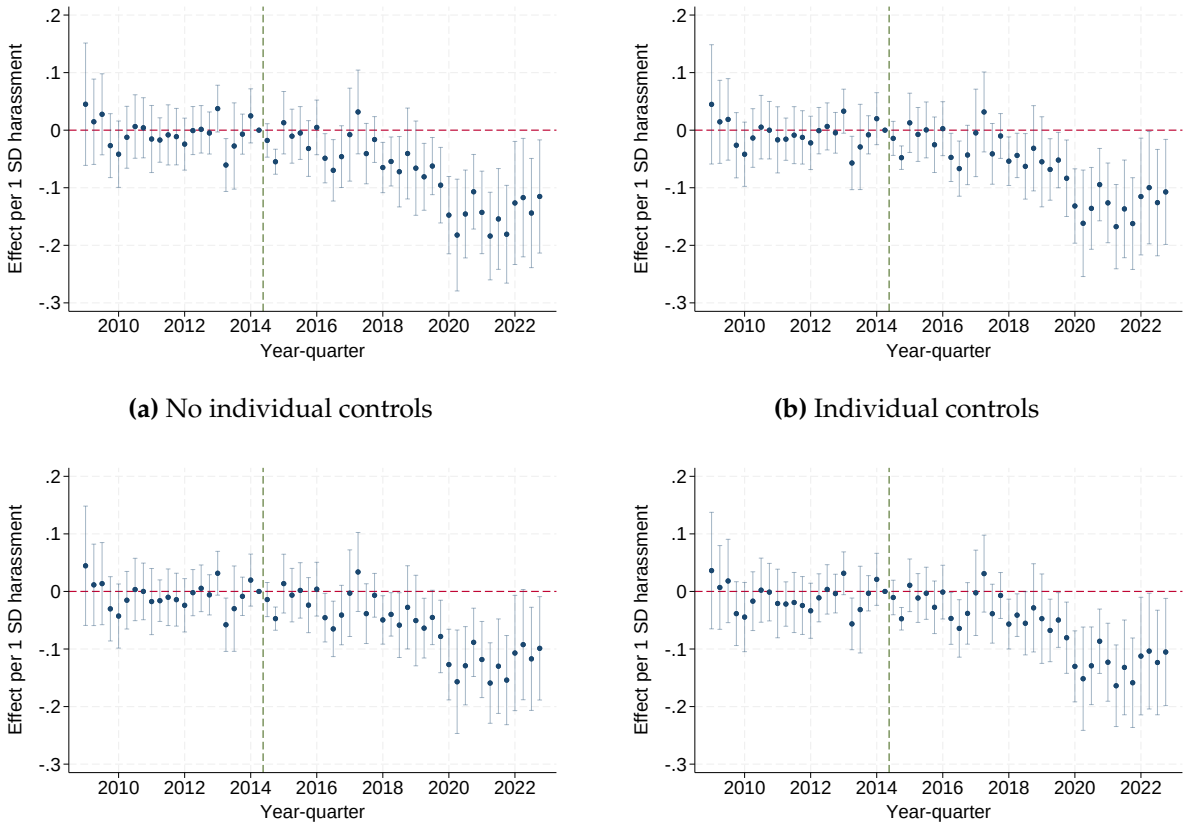
(b) Individual controls

(c) Individual controls and governorate linear trends

(d) Individual controls and governorate × year-quarter fixed effects

Figure A.3: Event Studies under the Four Specifications: Internal Communication

Note. Figure A.3 reproduces Figure A.1 for internal communication, standardized across occupations, under the four specifications of Table 3 in the main text. The panels, axes, markers and confidence intervals are the same as in Figure A.1, and the y-axis is expressed in standard deviations of the attribute. The sample consists of employed individuals with a four-digit occupation code, from 2009 onwards, and standard errors are clustered at the governorate level (27 governorates).



(a) No individual controls

(b) Individual controls

(c) Individual controls and governorate linear trends

(d) Individual controls and governorate × year-quarter fixed effects

Figure A.4: Event Studies under the Four Specifications: Contact with Others

Note. Figure A.4 reproduces Figure A.1 for contact with others, standardized across occupations, under the four specifications of Table 3 in the main text. The panels, axes, markers and confidence intervals are the same as in Figure A.1, and the y-axis is expressed in standard deviations of the attribute. The sample consists of employed individuals with a four-digit occupation code, from 2009 onwards, and standard errors are clustered at the governorate level (27 governorates).

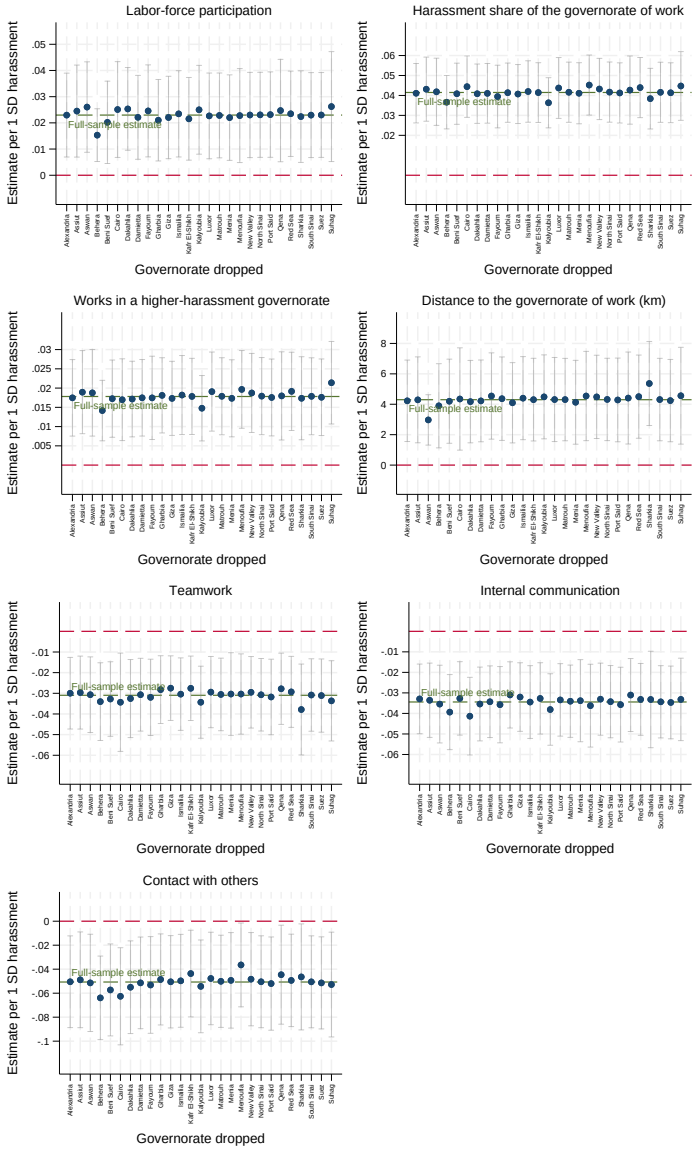
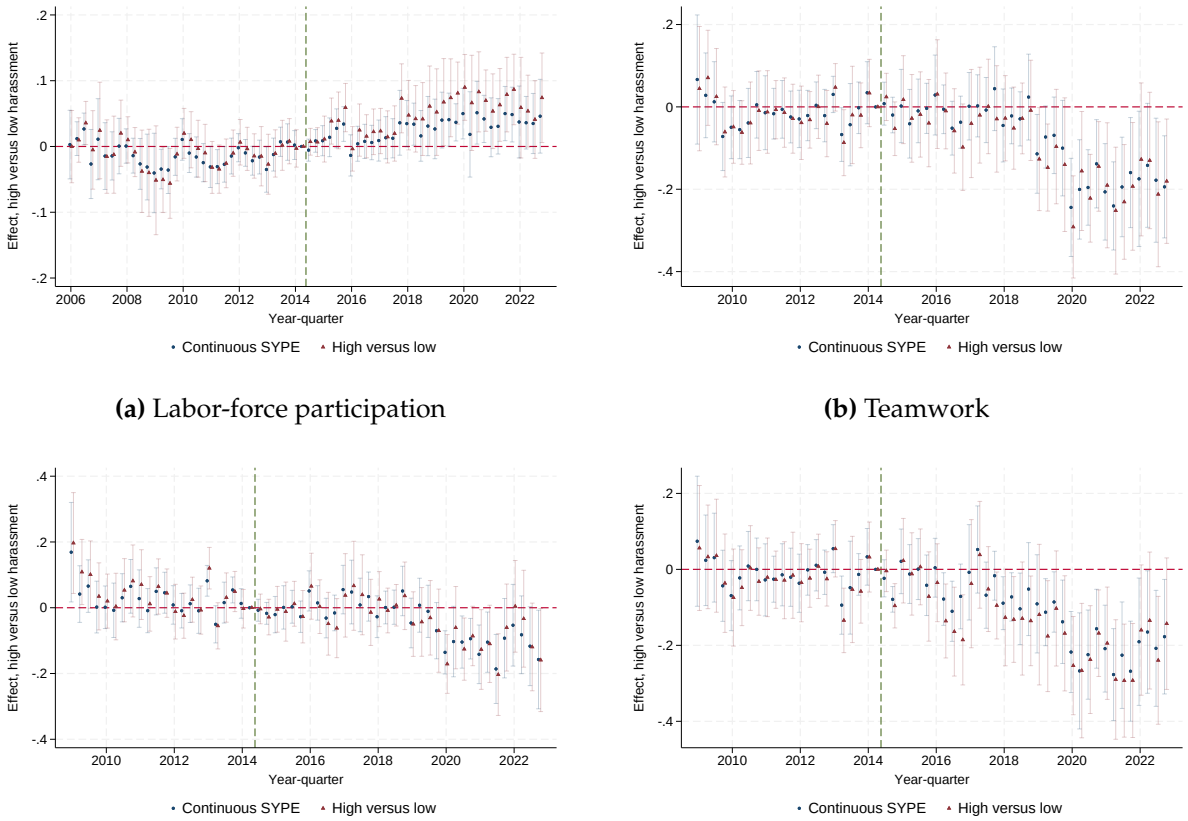


Figure A.5: Leave-One-Governorate-Out Estimates, by Outcome

Note. Figure A.5 reports, for each of the seven outcomes, the triple-difference estimates of equation (6.1) from the 27 samples that each omit one governorate. Each marker is one estimate, and the capped spike shows its 90% confidence interval (± 1.645 standard errors). The label below each marker indicates the governorate that is dropped, in alphabetical order. The green dashed line is the full-sample estimate, and the cranberry dashed line indicates zero. The panels correspond, in reading order, to labor-force participation, the harassment share of the governorate of work, working in a higher-harassment governorate, the distance to the governorate of work in kilometers, teamwork, internal communication and contact with others. All estimates use the specification of column (2) of Tables 1–3 in the main text, which includes the individual controls, governorate fixed effects and female $\times$ year-quarter fixed effects, with standard errors clustered at the level of the remaining 26 governorates.



(a) Labor-force participation

(b) Teamwork

(c) Internal communication

(d) Contact with others

Figure A.6: Event Studies under Two Definitions of the Treatment

Note. Figure A.6 reports the triple-difference event-study coefficients of the pooled version of equation (6.2), in which each quarter is interacted with Intensity$_g$ × Female$_i$, under two definitions of the treatment, for labor-force participation (panel a) and for the three measures of the interaction content of a woman's occupation (panels b to d). The y-axis reports the coefficient on the interaction between the treatment, each quarter and Female$_i$, and the reference period is 2014q2. The x-axis reports the calendar year-quarter. The dashed vertical line indicates the June 2014 law, and the dashed horizontal line indicates zero. *High versus low* uses an indicator for governorates above the 70th percentile of the SYPE 2009 harassment share, so that the coefficient measures the difference between the governorates with the most harassment and the other governorates. *Continuous SYPE* is the standardized share used throughout the paper, with coefficients multiplied by 1.652, which is the difference in the standardized share between the average governorate above the median and the average governorate below it (1.694 for the 70th-percentile split). The two series are shifted horizontally. All estimates include the individual controls, governorate fixed effects and female × year-quarter fixed effects, and standard errors are clustered at the governorate level (27 governorates).

B Data Construction

B.1 The firm survey

Source. The data on establishments come from the Economic Census of CAPMAS, which the Economic Research Forum (ERF) distributes to researchers. Although it is called a census, it is a sample survey, and the methodology volume of CAPMAS names the 2017–18 round the "Fifth Economic Census (by sample)". In each round, CAPMAS starts from lists of establishments, includes the large establishments in full, draws a sample of the small ones and computes sampling weights. Government establishments are not part of these lists.

The 2013 round. The 2013 round (2012/2013 in the publications of CAPMAS) was collected between November 2013 and February 2014, before the law. Its target sample of 164,000 establishments combines several lists. The largest part (96,000) is an area sample that covers small establishments. CAPMAS selects 1,800 enumeration areas of the 2006 establishment census and lists all the establishments in these areas. It then surveys all the establishments with five or more workers and, within each one-digit industry of an area, up to ten establishments with fewer than five workers. In addition, CAPMAS surveys in full the establishments with ten or more workers. The final sample includes 124,213 establishments.

The 2017–18 round. The 2017–18 round was collected between November 2018 and June 2019, after the law. The list is the establishment census of 2017, which includes 4,119,716 active and temporarily closed establishments. The sample is stratified by governorate, two-digit industry and size. CAPMAS surveys all the establishments with ten or more workers. It also surveys 50 percent of the establishments with five to nine workers, 5 percent of those with one to four workers, and 10 percent of those that were temporarily closed when the list was made. The total sample includes 366,168 establishments.

Released data and weights. In both rounds, ERF releases 50 percent of the records collected by CAPMAS, which gives 62,306 establishments in 2013 and 170,330 in 2017–18. The sampling weights are the inverse of the probability of selection, adjusted for non-response. In the released files, they sum to 2,410,365 establishments in 2013 and 3,742,562 in 2017–18, which are the totals published by CAPMAS. Hence, the released files include 2.6 percent of the establishments that they represent in 2013 and 4.6 percent in 2017–18. Since larger establishments are sampled at higher rates, this share increases with size. In 2013, it is 2 percent for establishments with one to four workers, 5 percent

for those with five to nine and 17 percent for those with ten or more. In 2017–18, the shares are 3, 10 and 25 percent. We use the weights in all the establishment-level estimates.

B.2 The HarassMap reporting panel

Source and retrieval. HarassMap is an Egyptian non-governmental initiative, launched in Cairo in December 2010, which allows women to report incidents of sexual harassment anonymously by SMS, through its website or through social media. Each report includes a free-text description, a date and a location, which the platform geocodes and displays on a public map. We scrape the full public archive of individual reports, and we use the date and the point coordinates of each report. The archive contains 1,852 reports in total. Twenty-seven reports have dates before 2010, and sixteen of them are among the reports with usable coordinates. These are retrospective entries that describe incidents that took place before the platform existed, one of them as early as 1902. Our series covers 2010–2020.

From points to administrative units. Each report comes with a latitude and a longitude. We assign each report to one of the 27 governorates by matching its coordinates to the polygons of the CAPMAS shiakha (ADM3) boundary file, which is the finest administrative level, and we then use the governorate of the shiakha. Reports with coordinates outside all polygons, such as offshore points and coordinates with obvious entry errors, are dropped at this stage. We then collapse the matched reports to counts by governorate and year, which is the unit of observation of the HarassMap series in the paper.

Balancing the panel. The collapsed file only contains the governorate-years in which at least one report was filed. Since the outcome is a count, a governorate-year with no reports corresponds to a zero. Hence, we construct all the combinations of units and periods and fill the empty cells with zeros. The governorate-year panel used in Section 5.2 has 297 cells, from the 27 governorates over the eleven years. The quarterly version has 1,161 cells, and 77.3 percent of them are zero.

Coverage. Four governorates, namely New Valley, Matrouh, North Sinai and South Sinai, have no HarassMap report during the whole sample period. We keep them in the panel with zero reports in every year, since we cannot tell whether the absence of reports reflects an absence of harassment or an absence of reporting. These are the desert and frontier governorates, and together they account for 1.2 percent of the

population aged 25–65. Their mean pre-law harassment prevalence (0.36) is close to the mean of the 23 governorates with at least one report (0.41). At the district level, the SYPE harassment share is available for 222 of Egypt's 365 districts, which is the sample we use for the district-level specifications.

Caveats. Two limitations should be kept in mind when interpreting these data. First, a report results from an incident *and* from the decision to report it on this specific platform, so the series measures reporting. Hence, a decline in reports is consistent both with less harassment and with reporting moving to another channel. Second, reporting requires internet or mobile access and knowledge of the platform, which are both correlated with urbanization and education. Therefore, the geographic distribution of reports reflects the reach of the platform as well as harassment.

B.3 The *Youm7* news-coverage measures

Source and retrieval. *Youm7* (*Al-Youm Al-Sabaa*) is one of the Egyptian daily newspapers with the highest circulation, and its online archive is continuous since 2008 and openly searchable. We use the full-text search of the newspaper for the Arabic root for harassment, *taharrush*, and go through all the pages of results. The URL of each result includes the publication date of the article (in the form `/story/YYYY/M/D/...`), so that we can date every article to the day without downloading its content. After removing duplicates based on the numeric article identifier, the search returns 10,517 unique articles over 2008–2026. In Section 5.1, we use the 2009–2018 window.

Unit and text field. The unit of observation is an article. We classify articles based on their *headline*, which we recover from the article URL slug, which carries at most its first ten words. Since the search covers the full text of each article, some of the results mention harassment only in passing. We therefore keep the 7,479 articles whose headline contains the root *taharrush*, which form our harassment corpus. Egyptian news headlines are descriptive and usually mention the act, the people involved and the location (e.g. "arrest of a man for harassing a girl in Maadi"), which makes them well suited to keyword classification. Using headlines also avoids contamination from unrelated text and keeps the procedure transparent. We only keep the date and the headline of each article.

Corpus and the three core measures. Let A_t denote the set of harassment articles published in period t (a month or a year), i.e. those whose headline contains the root

taharrush. Let K^{enf} be the set of enforcement keywords listed below. We define

$$\underbrace{N_t = |A_t|}_{\text{all harassment coverage}} \quad , \quad \underbrace{E_t = \left|\{\, a \in A_t : \text{headline}(a) \text{ contains a term in } K^{\text{enf}} \,\}\right|}_{\text{enforcement-themed coverage}},$$

$$\underbrace{s_t = E_t / N_t}_{\text{enforcement share}} \quad .$$

N_t is plotted as the bars of Figure 3 in the main text and E_t as the line. N_t measures the salience of harassment in public discourse, E_t measures the volume of coverage about the application of the law (arrests, prosecutions, trials), and s_t measures the share of harassment coverage that is about enforcement. Since it is a ratio, the share does not depend on the overall volume of attention, so an increase in s_t reflects a change in the *composition* of coverage toward enforcement. We report it in the text because over this period it follows E_t closely.

Keyword lists. We classify articles by matching curated lists of Arabic keywords to the headline, except for the workplace setting, which we code by hand. An article can belong to more than one series. The lists, with their English translation, are the following:

- Enforcement (K^{enf}): arrest, apprehension/seizure, detention, imprisonment, sentence/verdict, conviction, the Public Prosecution, court, investigation, misdemeanor/felony chambers, the Interior Ministry, the police, accused/defendant, acquittal. We do not include the word for security, *amn*, since it mostly appears in headlines about security deployments rather than arrests or trials.
- Street and public space setting (Figure 4 in the main text): street, road, square, Tahrir, corniche, park, downtown, feast-day/Eid, Eid al-Adha, Eid al-Fitr, festivals, *mawlid*, and public transport (metro, bus, microbus, train, public transport, station, stop). Before matching, we remove the words that contain one of these keywords without referring to a public place: the city of Port Said and the name Said, the verbs for "to restore" and the word for "far", which contain the word for Eid, and the expressions for "by means of" and "a stance", which contain the words for road and stop.

Workplace setting. Keywords perform poorly for this setting. Words such as worker, employee or manager often describe the offender's job when the harassment takes place in the street, and the word for office also matches the word for library. When we read the headlines matched by a list of workplace keywords (work, company, factory, office, manager, employee, colleague, employer, workplace, worker), fewer than one in three

are about harassment at work. We therefore read all the 752 headlines of 2009–2018 that contain either one of these keywords or another word for an occupation or a workplace (for instance nurse, flight attendant, secretary, teacher, journalist, hospital, hotel, shop or ministry). We code a headline as workplace harassment when the harassment happens at the victim's work or between people linked by work (a manager, colleague, employer or client), or when the article is about harassment in workplaces. We keep only stories about Egypt, since about half of the workplace headlines after the law are about other countries, mostly following the #MeToo movement of 2017–2018. The coded headlines are provided with the replication package.

The setting series of Figure 4 in the main text are constructed in the same way as the enforcement series. For public space, we count the harassment articles whose headline matches the list of the setting, and for the workplace we count the hand-coded articles. We index each count to its 2009–2013 mean.

An illustrative article. To give an idea of the content of the corpus, a representative example is the *Youm7* article "*I'rafi haqqik: al-sijn 7 sanawat 'uqubat al-taharrush fi al-'amal*" ("Know your rights: seven years in prison is the penalty for harassment at work"), published on January 6, 2024 (Figure 2 in the main text).[8] The article explains that harassment committed in the workplace or on public transport is punished by a minimum of seven years, and up to ten years when two or more aggravating circumstances apply, and it directly cites Article 306-bis B. These are the penalties in force in 2024, after later amendments raised those set in 2014. Since it mentions the setting and reports the sanction, the article would be classified as both *workplace* and *enforcement* coverage, although it falls outside the 2009–2018 window of our series. The enforcement and workplace series are built from articles of this kind.

[8] https://www.youm7.com/6436445

Appendix References

James G. MacKinnon, Morten Ørregaard Nielsen, and Matthew D. Webb. Cluster-robust inference: A guide to empirical practice. *Journal of Econometrics*, 232(2):272–299, 2023.