Discrimination Against Women in Hiring[*]

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Abstract

We study discrimination in hiring and its consequences for the discriminators using a unique survey of Egyptian businesses. Discrimination against women is widespread and overt: about half (51%) of establishments directly admit that they prefer to hire men over women. Using list randomization, we find that the true share of discriminators is similar to the share that directly admit to discriminating. We then provide novel evidence showing that discriminating against women is associated with productivity costs for establishments. Discriminators end up with lower-quality workers than the non-discriminators, with the negative effects concentrated among independent and informal establishments. However, we find that most establishments do not pay a penalty in terms of longer times to hire. This evidence is more consistent with taste-based discrimination than with statistical discrimination, implying that efforts to combat gender discrimination will likely require changing employer behavior rather than just increasing females' pre-market human capital.

1 Introduction

Gender discrimination is a topic of major concern for reasons related to both equity and potential economic impact. Numerous studies have demonstrated that gender influences labor market outcomes, albeit in complex ways (e.g., Altonji and Blank (1999); Baert (2018); Blau and Kahn (2017); Goldin (2014); Neumark (2018)). While the existence of discrimination in many contexts has been well-documented, far less is known about the nature of economic costs for employers and economies that discriminate against women (Duflo (2012); Kabeer and Natali (2013); Kawaguchi (2007)). In particular, do gender-biased employers experience penalties in the form of lower productivity or higher costs (or, conversely, do gender-neutral employers realize economic benefits)?

These issues are particularly important in a developing country context where discrimination is common (Jayachandran, 2015). There exists macroeconomic evidence that gender discrimination holds back economic growth (Klasen and Lamanna, 2009); however, these results are mixed (Baliamoune-Lutz and McGillivray (2007); Kabeer and Natali (2013)). Evidence on discriminatory attitudes and behavior among individual employers in such a context, and the potential consequences of that discrimination, is difficult to come by (Demirgüç-Kunt et al. (2013); Knight and Sabot (1982)).

In this paper, we study discriminatory attitudes in hiring decisions and their consequences for employers, with our primary focus on discrimination against women. Using a unique survey of Egyptian businesses with direct and indirect questions about discrimination, we estimate both the true extent of discrimination and how willing establishments are to directly admit to this behavior. Our survey also collects data on hiring needs, including detailed data on the types of skills demanded, as well as data on hiring outcomes. We use these data to look at consequences for establishments that discriminate by connecting their hiring outcomes to their biases. Rather than looking at the macro-level consequences of discrimination, we are able to look at the consequences for individual employers.

We find that discrimination against women in hiring is widespread, overt, and has economic costs for the employer. Over half (51%) of establishments directly admit preferring to hire men over women. This share is highest in retail (60%) and lowest in

Information Technology (17%). To estimate the "true" share of discriminating establishments, we use a list randomization method which allows establishments to admit discriminatory preferences in an anonymized way (e.g., Karlan and Zinman (2012)). Surprisingly, this approach gives an estimate similar to the direct question method. This result suggests that discriminatory attitudes against women are not stigmatized in Egypt. The willingness of establishments to admit directly to this behavior enables us to interpret our direct survey responses as accurate measures of discrimination at the establishment level.[1]

We then turn to the consequences of discrimination against women for the employers. Theoretically, these consequences may depend on what economic model of discrimination accurately describes firms' behavior. To the extent that employers' gender bias is based on statistical discrimination - that is, based on accurate and rational expectations of gender-based productivity differentials in the presence of imperfect information - then we would not expect to see discriminating employers experience bias-related costs (Guryan and Charles, 2013). By contrast, if substantial taste-based gender discrimination exists, or if employers have inaccurate views of group productivity (e.g. Bohren et al. (2019)), then we would expect that some employers would experience associated economic costs as they engage in non-productivity-enhancing behaviors (Becker, 1957). In this latter case, remedying discrimination would require some change in employer behavior, incentives, or beliefs rather than just increasing females' pre-market human capital.

We find that discriminatory attitudes towards hiring women in Egypt are associated with reduced productivity, a result that is consistent with taste-based rather than statistical discrimination. Discriminating establishments end up with lower-quality workers than the non-discriminators; their new hires take about 1.4 weeks, or 24%, longer to reach an acceptable level of productivity, even when controlling for a large set of other factors. This is particularly true among informal, single-site establishments (a 36% longer time to productivity for those who discriminate), which are common in

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[1]While our focus here is on discrimination against women, we also asked establishments about discrimination by socioeconomic background, again using the list randomization method & direct questions. In contrast to the gender results, we find that establishments are reluctant to directly admit putting more weight on SES than on workers' skills, but do so indirectly. We present these results in the Appendix.

Egypt and other developing countries. While establishments that are larger in size discriminate less frequently, they also pay a larger productivity penalty when they do discriminate (62% longer).

We also find evidence that informal, single-site employers with discriminatory attitudes experience significantly greater levels of turnover from terminations of employees. The formal establishments that are part of larger organizations (e.g., chain stores) seem to be able to "afford" to discriminate without negative consequences, perhaps because their access to high-quality applicant pools means they can hire the most productive male workers without experiencing the quality/discrimination trade-off that smaller establishments face. Importantly, most discriminatory employers do not appear to pay a penalty in terms of longer time-to-hire. With the exception of employers seeking highly educated workers, the costs associated with gender discrimination show up over time rather than up front. To our knowledge, our study is the first to demonstrate these patterns of productivity and gender bias.

We make two key contributions to the literature. First, we provide evidence about gender discrimination at the establishment level, allowing us to demonstrate detailed conditional correlations between employers' gender preferences and productivity-relevant outcomes. We show that these patterns are consistent with taste-based rather than statistical discrimination. Other work has attempted to use gender diversity as a proxy for discrimination, with conflicting results (Azmat and Boring (2020); Kawaguchi (2007); Liu et al. (2010); Tsou and Yang (2019); Weber and Zulehner (2014)). Using observed gender diversity as a proxy limits the ability for researchers to directly study discrimination because firm gender composition could be due to an inability to hire women as opposed to an unwillingness to hire women. At the same time, not finding evidence of impacts to discrimination in a labor market in which both taste-based and statistical discrimination are present could be due to an improvement in outcomes due to statistical discrimination that masks the negative impacts of taste-based discrimination. While there is a literature on how discrimination affects economic growth and macro outcomes (e.g., Esteve-Volart (2000)), it is difficult to find evidence on the consequences for individual employers.

Our survey allows us to study the consequences of discrimination directly because of Egyptian employers' seeming honesty about their gender biases. Because employers

willingly admit discriminatory attitudes against women – as seen in the similarity between the direct question and the list randomization result - we can examine how their answers correlate with the hiring outcomes, and we can be confident that we have accurately identified the discriminators at the establishment level. Furthermore, we also collect detailed data on the skills these employers demand when hiring workers. By controlling for these factors in our analysis, we ensure that we are picking up effects of discriminatory attitudes rather than differing skill demands.

Second, we document the true extent of discriminatory behavior among the most common global employer: small and medium-sized enterprises in a developing country. Discrimination is notoriously difficult to detect accurately in observational data. Methods such as the Kitagawa-Oaxaca-Blinder decomposition (Blinder (1973); Kitagawa (1955); Oaxaca (1973)) may provide bounds on the extent of discrimination, but are limited by what variables can be directly observed. The most common way of assessing the "true" prevalence of discrimination in hiring is through correspondence studies, in which (typically) fake resumes are submitted to companies with random changes in race, gender, or other characteristics (e.g., Bertrand and Mullainathan (2004); Deming et al. (2016); Kline et al. (2021); Kroft et al. (2013); see Baert (2018) for an overview of these studies). In the context of developing countries, a correspondence study is less feasible because many smaller, informal employers do not use either resumes or job websites in their hiring processes.[2] Our results suggest that a correspondence study would undercount the true extent of discrimination in this context, because these biased attitudes are more prevalent among smaller, informal establishments - those least likely to advertise jobs online or use formal resume processes. Our surveys allow us to reach employers who would not be reached through other methods.

The paper proceeds as follows. Section 2 discusses our survey and the methods we use to detect discrimination and its consequences. Section 3 documents discrimination against women and studies its effects on the discriminating employers. Section 4 concludes.

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[2]Banerjee et al. (2009), Siddique (2011) and Arceo-Gomez and Campos-Vazquez (2014) are exceptions, but their studies are limited to jobs advertised online.

2 Our Survey and Techniques

Our study takes place in Egypt, a middle-income country with a PPP-adjusted GDP per-capita of about $12,000. Unemployment in Egypt has been persistently high for years, hovering around 11-12% from 2016 to 2019. Female labor force participation in Egypt is only 22% as of 2020, higher than some countries in the Middle East/North Africa region but still among the lowest in the world (ILO, 2016).

While there are likely multiple factors that contribute to high unemployment and the low female participation rate, we focus on discrimination, and particularly discrimination against women. In our conversations in Egypt with policymakers, businesses, and NGOs, we heard widespread anecdotal evidence that firms either prefer to hire men or simply refuse to hire women. In the course of our field research, labor market actors and analysts put forth several explanations for gender discrimination in Egypt. Some claimed that discrimination stemmed from traditional ideas about proper gender roles, while others emphasized a belief that men are more reliable workers than women. In other words, some assertions implied taste-based discrimination, while others were potentially consistent with statistical discrimination. Distinguishing between these scenarios is important for policy. If employers are willing to hire women, but are using gender as a statistical shortcut to obtain more skilled or productive workers, then it is necessary to focus policy attention on disparities in education and training (which, of course, may stem from discriminatory attitudes in other institutions). Alternatively, if employers are indulging taste-based prejudice by rejecting qualified female candidates, policy interventions must ultimately seek to change employer behaviors. Many stakeholders we spoke with believe that addressing gender discrimination is key to addressing the country's labor market problems. However, despite this general sense of the importance of the issue, little hard evidence exists on the exact prevalence and nature of the phenomenon.

To study these issues, we designed and conducted a survey of Egyptian businesses from four different industries in 2017-18. We surveyed a total of 1,180 establishments from the retail, information technology (IT), hotels, and restaurant sectors.[3] The sample was recruited using Egypt's 2018 enterprise census as a guide to ensure that the

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[3]The retail portion of the survey was also used for the analysis in Osman et al. (2021), where we study network-based hiring.

distribution of firm size by industry was nationally representative. The survey firm then identified locations that include well-known industry-specific clusters of establishments and aimed to survey the universe of establishments in that area. This strategy was implemented because we did not have access to a detailed list of all establishments in the country and so could not implement a random sampling methodology. We then produced sample weights that ensure that our sample lines up with the distribution of retail enterprises in Egypt using the census based employee sized groupings (5-9 employees, 10-24, 25-49, 50-99, 100-249 and 250+). The current sampling method likely leads us to sampling somewhat more capable and productive establishments since they have been able to survive in competitive clusters.

The surveys were conducted via interviews with human resources or hiring managers at each establishment. Upon arrival, the interviewer asked to speak with a manager who has authority over personnel and hiring matters. Each survey took approximately one hour and contained about 80 questions. We eliminate the establishments that have fewer than 5 employees because we do not have information on them from the Census. These make up only 2% of employment in our survey. Our final sample is made up of 1,076 establishments.

The sample is spread out geographically over Egypt. The greater Cairo area represents the largest population center in Egypt, and establishments there account for about 45% of our sample. Establishments in the greater Alexandria region, the second largest city in Egypt, account for 33% of our sample. The remaining 22% come from the southern population centers of Al-Minya and Assuit.

List Randomization

In addition to gathering large amounts of data on each establishment's characteristics and practices, we used a list randomization technique to elicit the true share of establishments that discriminate. List randomization (also known as the item count technique, introduced in Raghavarao and Federer (1979)) is a survey technique used to ask sensitive questions that might not elicit true responses if asked directly. In this technique, a portion of the sample, chosen randomly, was shown a short list of statements that have nothing to do with the sensitive behavior we are interested in

(discrimination against women). These include things like, for example, "My household owns an air conditioner." This portion of the sample is simply asked how many of the statements are true (not which ones are true).

The remainder of the sample was given the same list of statements, but with one added. The added statement is the "sensitive" statement, in our case "When hiring a new employee, I prefer to hire men over women." The respondents were also asked how many of these statements are true. Because the two groups are chosen randomly, one can interpret the difference in the mean number of true statements between the two groups as the proportion of the entire sample that agrees with the sensitive statement. Several studies have established that list randomization gives higher (and presumably more accurate) estimates of the prevalence of socially unacceptable or stigmatized behaviors (e.g., Holbrook and Krosnick (2010); LaBrie and Earleywine (2000); Tsuchiya et al. (2007)). For a detailed discussion of the benefits and potential limitations of list randomization, see Karlan and Zinman (2012) & Chuang et al. (2021).

For the half of the sample that was not given the sensitive statement about discrimination in the list randomization, we directly ask the sensitive question.[4] The results from these responses give us an idea of whether the discriminatory behavior is socially stigmatized or not. If discrimination against women is socially acceptable, for example, the estimates from direct elicitation and the list randomization should be similar. If discrimination is socially stigmatized, then list randomization should give a higher estimate than the direct question.[5]

Establishment Characteristics

We are interested in what establishment characteristics are associated with discriminatory attitudes. We are also interested in the outcomes associated with discriminating against women, which we can only measure accurately if establishments are honest

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[4]This is not a context in which discriminating against women is culturally encouraged (unlike some areas in Taliban-controlled Afghanistan, for example) and so we do not believe people would claim to discriminate but actually do not discriminate.

[5]Tsuchiya et al. (2007) find that list randomization gives a higher estimate than direct elicitation for stigmatized behavior (shoplifting) but not for more acceptable behavior (blood donation). LaBrie and Earleywine (2000) have a similar finding for unprotected sex (stigmatized) and drinking alcohol (acceptable) among undergraduate students.

about their attitudes. Our survey is highly detailed, with data on establishment-level labor-market outcomes, operational characteristics, and skill requirements. In this paper, we focus on three key hiring outcomes: average time needed to hire workers (a measure of the difficulty in finding workers), terminations (involuntary turnover), and average time it takes a worker to reach acceptable productivity. We interpret the latter item as a measure of the quality or initial productivity of the worker being hired, since all questions relate to the same type of entry-level job (within industry). In our regression analysis, we will also use several other establishment characteristics that come from the survey, including establishment size, use of formal vs. informal labor, whether the establishment is part of a larger firm (e.g., a multi-site firm), average pay, etc.

Because assertions of discrimination are often confounded by the differing skill demands across jobs, we also collected data on skill demands. We have detailed data on the skills demanded by each establishment for entry-level jobs. Within each of the four industries, we used a pilot survey to determine the most common entry-level job. Then, in our main survey, we asked about the importance of a variety of skills in that entry-level job. This allows us to distinguish establishments that demand "higher-level" or more specialized skills from those that do not.

Ultimately, our survey provides reliable estimates of the true share of establishments that discriminate (via the list randomization), as well as revealing how willing establishments are to admit to those attitudes. At the same time, the rich set of establishment characteristics and the skills they demand increase confidence that any relationship we observe between discrimination and outcomes is not driven by other factors.

3   Discrimination Against Women

Documenting Discrimination

We first document discriminatory attitudes against women in hiring. As described in the previous section, our expectation was that establishments would be reluctant to admit this discrimination, so we used a list randomization technique that allows establishments to admit sensitive behavior indirectly. We expected to see that the

"true" share of discriminators - as estimated by the list randomization - was higher than the share of establishments that directly admit discrimination.

The results are shown in Table 1. About half of the sample (541 establishments, randomly chosen), was asked directly, "When hiring a new employee, do you prefer to hire men over women?" The top row of Table 1 shows that over half (51.4%) of establishments asked this question admit to preferring men over women in hiring decisions.[6] The share of establishments that admit discrimination against women varies considerably by industry. It is highest in retail (60.1%) and lowest in IT (16.4%). The standard errors are too large here to draw firm conclusions, but IT seems to stand out from the others.

The second line of the table estimates the "true" share of establishments that prefer men over women, using the list randomization method. Here the 541 establishments that were asked directly were given a list of 4 innocuous statements without the statement about gender discrimination, and the remaining 535 establishments were given the same list with the statement about gender discrimination added to it. To estimate the true share that discriminate, we regress the number of statements the establishment agreed with on a dummy variable for being in the group that had a fifth statement about gender discrimination.

This method shows that about 41% of establishments prefer men over women, but the large standard error means that the confidence interval overlaps considerably with the estimate from the direct question. Regardless of which method is employed, the share of establishments that discriminate against women is very high.

These striking results imply two conclusions. First, discrimination against women is widespread in the Egyptian labor market. Both estimates are consistent with about half of establishments preferring to hire men over women. Second, and key to our analysis below, this discrimination seems to be overt and not stigmatized. If preferring men were stigmatized, we would expect that the list randomization method would give a higher estimate than the direct question. Here, the list randomization actually gives a lower estimate, but the large standard error means we should not make too much of the difference in point estimates. The establishments that discriminate against women

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[6]All results in this table are weighted by establishment size to match the Egyptian census of establishments. Results are robust to not using establishment weights.

Table 1: Discrimination by Gender

| Panel A: Percent of establishments that prefer men over women | | | | | |
	Overall	Retail	IT	Hotels	Restaurants
Direct question (n=541)	51.4%	60.1%	16.4%	43.7%	55.5%
	[45.8, 56.9]	[52.1, 68.0]	[5.7, 27.2]	[20.4, 66.9]	[47.1, 63.9]
List randomization (n=1,076)	41.0%	57.4%	25.9%	40.2%	27.0%
	[21.3, 60.6]	[30.5, 84.4]	[-11.7, 63.5]	[-31.3, 111.7]	[-6.4, 60.4]

| Panel B: Predictors of admitting a preference for men over women | | | | | | |
	(1)	(2)	(3)	(4)	(5)	(6)
Part of larger firm?	0.062					0.064
	(0.052)					(0.065)
Number of employees		-0.004*				-0.005**
		(0.002)				(0.002)
Pays above competitors			-0.100			-0.158*
			(0.089)			(0.088)
Pays below competitors			0.084			0.067
			(0.141)			(0.139)
% workers w/institute/college ed				0.109		0.108
				(0.070)		(0.072)
% workers formal					0.001	0.001
					(0.001)	(0.001)
Survey fixed effects?	Y	Y	Y	Y	Y	Y
Observations	541	541	541	535	514	514
R-squared	0.077	0.076	0.080	0.080	0.079	0.103

Panel A note: The 95% confidence interval is given in brackets below each figure. The direct question is the percentage of establishments answering yes to the question, "When hiring a new employee, do you prefer men over women?" The list randomization figure is the difference in mean number of true statements reported by the treatment group and the control group. Robust standard errors are used. Panel B note: Each column reports results from an OLS regression of admitting a preference for men over women. Robust standard errors are are shown in parentheses. All regressions include industry fixed effects and a constant term. All estimates are weighted by establishment size to match the size distribution in the Egyptian census of businesses. We exclude all establishments with fewer than 5 employees due to a lack of data about them in the Egyptian census of establishments. The pays above/pays below variables are missing for all of the retail sector, we replace them with a constant which is made irrelevant by the industry fixed effects.*$p<0.10$ **$p<0.05$ ***$p<0.01$.

are clearly not shy about it.

For the 541 establishments that were directly asked about gender preference, we can look at what characteristics predict the admission of preferring men over women. In Panel B, we report results from OLS regressions of admitting discrimination on several establishment characteristics. All regressions also include survey fixed effects and a constant term. It is difficult to predict discrimination using these variables. In the combined regression, larger establishments and those that pay above-market wages are less likely to discriminate, but formality, the education of workers, and being part of a larger firm do not predict this phenomenon. With all variables and survey fixed

effects included, the R^2 is only 0.103 (without survey fixed effects, it falls to 0.058).[7]

While our focus here is on gender, we also used the list randomization method to ask about discrimination by socioeconomic background. We present these results in the Appendix. We find that while only one-third of establishments admit to putting more weight on SES than on workers' skills, the true share is 54% (as given by the list randomization), and the two are significantly different. This suggests that discrimination by SES is stigmatized to some degree, while discrimination against women - which establishments admitted freely - may not be. This contrast between SES and gender also shows that we have sufficient power to detect differences between the direct question and the list randomization.

Consequences of Gender Discrimination

Because Egyptian establishments openly admit to discriminatory attitudes against women, we are able to look at the consequences of discrimination at the micro level. Specifically, we ask if discrimination against women can be linked to hiring difficulties on the part of establishments. Classic economic theory on taste-based discrimination (e.g., Becker (1957)) predicts that firms that discriminate should have lower-quality workers and lower profits, relative to those that do not discriminate, as long as the discrimination is driven by firm preferences rather than customer preferences. However, if the discrimination is statistical rather than taste-based - that is, based on accurate and rational expectations of gender-based productivity differentials in the presence of imperfect information - then we would not expect to see discriminating employers experience bias-related costs (Bohren et al. (2019); Guryan and Charles (2013)).

Although our cross-sectional data do not permit us to establish causality, we can nevertheless test which economic stories are consistent with precisely measured data at the establishment level. In this study, we analyze three basic hiring outcomes: average time to make a hire, average time it takes a new hire to reach "acceptable productivity", and employee terminations (involuntary turnover). The first is a measure of how easily the establishment can find workers. The second and third are related to the quality of

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[7]The survey fixed effects (not shown here) show that establishments in IT are least likely to discriminate, followed by hotels. This is consistent with the results from Panel A.

those new hires.[8] To the extent that employers engage in taste-based discrimination against women, we would expect the average worker quality of discriminating establishments to be relatively lower. Such establishments should have a longer time to acceptable productivity and higher involuntary turnover. The relationship with time to hire is less clear: an establishment that only considers male candidates, for example, might take longer to find a potential worker but be quicker at evaluating candidates and making an offer.

Time to hire and time to productivity are measured in weeks. We model all of these items as count data using negative binomial models, estimating versions of the following base equation for establishment i in industry j:

$$\text{Outcome}_i = \alpha_i + GenderDisc_i\beta_1 + SkillReq_i\beta_2 + X_i\beta_3 + Industry_i + \epsilon_i \quad (1)$$

All regressions are weighted based on establishment industry-size frequencies in the Egyptian enterprise census. We use Huber-White standard errors. The vector X_i includes various organizational characteristics. In our full specifications, we include interactions between gender discrimination and three organizational attributes: (1) an indicator for multi-site operations and formal labor practices (vs. single-site or informal), (2) high and low levels of educational attainment among the establishment's current workforce, and (3) an indicator for above-median establishment-level employment. These interactions are important as the the hiring frictions and productivity attributes that are associated with gender bias likely show heterogeneity across employer types. Multi-site formal organizations tend to have more resources and higher job quality. Consequently, they may incur lower market penalties for biased behavior as they are able to hire the "cream" of even a more limited pool of male-only workers.

When estimating effects associated with formality/job-quality, it is important to also control for establishment-level employment size. Although formality and employment size are positively correlated, they are very different phenomena. Indeed, more than a quarter of our sample consists of informal or single-site employers who nevertheless have above-median levels of employment at the establishment level. Finally, our education interactions test whether employers with higher skill requirements experience different bias-related costs (Kuhn and Shen 2013). We also include industry

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[8]See Osterman and Weaver (2013) and Osman et al. (2021) for more on using these measures.

fixed effects as our target industries vary widely in both hiring practices and gender representation.

The results are shown in Table 2.[9] All figures are marginal effects. In the first specification in column (1), which measures the relationship between gender discrimination and time-to-hire, we can see that in a simple model that only controls for employment size and growth, discriminatory attitudes toward female job candidates are associated with an insignificant half-week reduction in time-to-hire. A full model with controls for skill requirements, organizational characteristics, and industry effects also shows no significant correlation. However, the interaction effects do reveal a relative penalty: employers who exhibit gender bias and whose workforce is above the 75th percentile of standardized institute/college-level education experience a 1.7 week penalty in recruitment time relative to biased employers with less-educated workers ($p<0.05$). Other than employers seeking very highly educated workers, the results thus generally indicate that Egyptian employers with discriminatory attitudes toward women do not pay a penalty in terms of extended hiring times.[10]

The termination results (columns 3 and 4) show somewhat more nuanced results. Although a simple model shows a significant correlation between gender discrimination and greater terminations (thus theoretically implying a match-quality penalty for discrimination), this effect becomes smaller and insignificant in the presence of organizational controls and industry effects. However, the insignificant marginal effect of discrimination in the full model masks heterogeneous effects. Informal or single-site establishments (hereafter single/informal) that discriminate experience a significant increase in annual terminations equal to an additional half of an employee (0.477) per year. By contrast, formal establishments that are part of a multi-site organization (hereafter multi/formal) and that discriminate by gender experience insignificantly fewer terminations. The difference between these two marginal effects is significant at the 90%

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[9]We omit reporting some of the covariates from columns 2, 4, and 6, but the full results are found in Appendix Table A3.

[10]About 22% of the sample did not respond to the question regarding the number of weeks it takes to hire. In Appendix Table A4 we regress a binary for non-response to this question on a wide ranging list of variables. We find that non-response is associated with establishments who pay a bit better, have fewer employees, are less likely to provide training and are less likely to discriminate. Importantly there is no difference in non-response based on the establishment's educational needs, which is the primary margin for which we see differences in our analysis of this metric. This result makes us less concerned that differential attrition is driving our results on time to hire.

Table 2: Gender Discrimination and Hiring Outcomes

| Dependent variable: | (1) | (2) | (3) | (4) | (5) | (6) |
	Wks to Hire		Terminations		Wks to Productivity	
Bias against female candidates	-0.482	-0.187	0.446**	0.172	0.587	1.312**
	(0.513)	(0.458)	(0.212)	(0.222)	(0.528)	(0.553)
Above-median estab. employment	-0.207	-0.104	-0.247	-0.275	1.755***	1.608**
	(0.424)	(0.383)	(0.163)	(0.185)	(0.555)	(0.638)
Formal, part of larger org.		1.077**		0.167		0.890
		(0.509)		(0.226)		(0.699)
Institute/college ed. (std. proportion)		0.217		-0.094		0.326
		(0.297)		(0.095)		(0.275)
Unemployment rate		-0.217**		-0.057		-0.110
		(0.090)		(0.047)		(0.105)
Marginal interaction effects of employer gender discrimination:						
Formal, part of larger org.		-0.993		-0.398		-0.001
		(0.794)		(0.410)		(1.149)
Other type		0.071		0.477**		1.744***
		(0.521)		(0.236)		(0.618)
Difference in marginal effects		-1.064		-0.875*		-1.745
		(0.896)		(0.458)		(1.270)
Institute/college ed.–75th pctile		0.400		0.076		1.195**
		(0.561)		(0.224)		(0.609)
Institute/college ed.–25th pctile		-1.311**		0.393		1.513**
		(0.521)		(0.285)		(0.770)
Difference in marginal effects		1.711***		-0.317		-0.318
		(0.645)		(0.263)		(0.798)
Above-median estab. emp.: Yes		-0.830		0.116		3.716***
		(0.506)		(0.251)		(1.119)
Above-median estab. emp.: No		0.091		0.171		0.404
		(0.574)		(0.288)		(0.602)
Difference in marginal effects		-0.921		-0.056		3.313***
		(0.675)		(0.359)		(1.204)
Observations	443	423	537	514	541	516
Pseudo R-squared	0.001	0.033	0.012	0.085	0.005	0.027

Notes: Each column reports results from negative binomial regressions using establishment employment weights. The regressions in columns 2, 4, and 6 also include industry fixed effects; dummies for requiring advanced writing skills, English speaking, and computer usage; whether the establishment has higher technology and higher or lower wages than competitors; whether they use owner connections in hiring; whether they provide training; whether they have shifted to internal hiring in recent years; whether they experience frequent poaching of workers; and the unemployment rate in their governorate. All estimates are weighted by establishment size to match the size distribution in the Egyptian census of businesses. We exclude all establishments with fewer than 5 employees due to a lack of data about them in the Egyptian census of establishments. The pays above/pays below variables are missing for all of the retail sector, we replace them with a constant which is made irrelevant by the industry fixed effects. The full results are available in Table A3. Robust standard errors in parentheses. *p<0.10 **p<0.05 ***p<0.01

level and equates to about one additional termination per year for single/informal establishments. One interpretation of this result is that multi/formal establishments can indulge a taste for discrimination by relying on their superior resources, greater male application rates, and access to better hiring networks (Hellerstein et al. (2002); Osman et al. (2021)).

The most pronounced direct effects associated with gender discrimination involve worker quality or productivity, as measured by time to acceptable productivity for new hires. In the full specification (column 6), establishments that admit discriminatory attitudes toward women experience an average of 1.3-week delay in time-to-productivity (a 24% increase over non-discriminators in our estimation sample), significant at the 95% level. As with terminations, the interaction results show that this marginal effect is most concentrated among single/informal establishments. The marginal effect for single/informal employers is large and significant (1.7-week delay, a 36% increase over the mean for non-discriminators in that group), although the difference in marginal effects between formal and informal employers is not precisely estimated.

The interaction effects also indicate, perhaps surprisingly, that the productivity penalty for gender discrimination is more pronounced among establishments with above-median employment. Larger establishments discriminate less frequently (see Table 1), but it seems they pay a larger penalty when they do discriminate. The above-median-size establishments that discriminate face a nearly 4-week delay–a 62% increase over non-discriminators in that group–compared with no delay for the smaller establishments, and the difference is highly significant. It is also worth noting that establishments at both high and low points on the education distribution experience similar gender-bias-related productivity penalties, thus revealing a nonlinear pattern that is worthy of further investigation with other datasets. Based on our results, it appears that the differential frictions associated with seeking highly educated workers but excluding female candidates are most clearly evident in longer times to hire rather than reduced worker quality.

These effects reveal a number of interesting patterns. First, formality and employer size are very different phenomena, and they have different relationships with gender discrimination. While multi-site/formal employers tend to have larger establishment-level employment, it is the formality and its associated job quality that appears to allow dis-

criminating employers to hire high-productivity male workers and avoid a diminution of average productivity. Net of formality, larger employers are not able to use attributes of size (e.g., potential better matching due to economies of scale in hiring) to compensate for the negative productivity effects of bias against a class of (female) candidates. Indeed, larger establishments incur one of the largest penalties in our estimations. It could be that the business model of larger establishments, which are significantly more likely to show positive employment growth than smaller establishments, depends more heavily on throughput or uninterrupted growth (and thus makes these employers more willing to compromise on productivity). The results also show that the educational profile of an establishment's workforce is relevant as well, although the penalty shows up in recruitment time.

Overall, our results indicate that discrimination against women in hiring has negative economic consequences for some establishments, a result more consistent with taste-based than statistical discrimination. Across all employers, the primary penalty comes through lower worker quality - in line with predictions from classic economic theory on taste-based discrimination (Becker (1957)) - and not through increased hiring times. Single/informal employers may also pay a penalty in terms of greater terminations due to lower match quality, while establishments that employ highly educated workers do experience hiring delays. The negative consequences of discrimination are greatly attenuated for multi-site/formal employers. These effects are net of detailed controls for skill demands, including writing, English language requirements, and level of computer usage (standardized within each industry). Thus the effects that we estimate are not simply reflecting the fact that male jobs tend to involve higher skill requirements.

4   Discussion

Using a detailed and unique survey of Egyptian establishments across four industries, we have shown convincing evidence of widespread discrimination against women in hiring. Roughly half of establishments directly admit to preferring to hire men over women. The direct estimate is similar to that given by a list randomization method, showing that this discrimination is accepted as normal rather than stigmatized.

Most significantly, because of establishments' willingness to admit to discriminatory attitudes, we can look at the consequences of discrimination at the micro level. Gender discrimination seems to have costs for establishments in terms of lower productivity, a result more consistent with taste-based discrimination than statistical discrimination. While most discriminators do not take longer to find workers, our regression results suggest that they do end up with lower-quality workers than non-discriminators; their workers take 24% longer to reach acceptable productivity, even when controlling for a large set of other factors.

These consequences are concentrated among the informal, single-site establishments, which are very common in developing countries. Those that discriminate among this group see a 36% rise in their workers' time to reach acceptable productivity. These enterprises are difficult to study by other means. Alternative methods, such as correspondence studies, typically rely on online job advertisements, a method rarely used by these employers. Our survey not only allows us to reach these establishments, but also to study their behavior and associated labor-market outcomes. The formal, multi-site establishments seem to be able to "afford" to discriminate without consequence, perhaps because their access to high-quality applicant pools means they can hire the most productive male workers without experiencing the quality/discrimination trade-off that smaller establishments face. It is worth emphasizing that the formality effect is quite distinct from the establishment-size effect. Larger scale operations do not insulate employers from the economic correlates of gender discrimination in the same manner as formality.

The relationship between discrimination and establishment-level skill/educational requirements is also worth noting. Higher skilled industries, such as IT, have lower rates of explicit gender bias. However, our results indicate that when an employer with higher educational requirements does exhibit anti-female bias, the establishment is more likely to incur costs in the form of extended recruitment times.

Overall, our results provide perhaps the clearest evidence yet of widespread discriminatory attitudes and practices in the labor market of a developing country. While there are negative consequences for establishments that engage in discrimination against women, these consequences are not evenly distributed. Our results indicate that market forces are unlikely to pressure formal, multi-site discriminators into eliminating the

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discriminatory practices, as these employers appear to be shielded from productivity-related penalties. Single/informal employers do incur these penalties, but they appear willing to make this trade-off. Given the taste-based nature of gender discrimination in this context, public policies and labor-market interventions designed to lessen gender bias will likely need to focus on changing employer behavior rather than simply attempting to achieve gender parity via increased investments in female human capital.

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Appendix: Discrimination by Socioeconomic Background

While the focus of our paper is discrimination by gender, we were also told told by our Egyptian partners that discrimination by socioeconomic status (SES) or family background is important. Therefore, we also included questions about this discrimination both because of its interest to our partners and to test and validate our list randomization method. SES is difficult to define in Egypt, as there are no major ethnic or linguistic minorities that would automatically be seen as of lower background. Anecdotally, it could be that language accents and being from a rural area are part of how SES is seen by employers. Other studies that look at SES in Egypt define SES measures like education and parental education (Mowafi et al. (2014)).

We perform the same analysis for SES as we did for gender, comparing the "direct question" and list randomization methods. Table A1 shows the results. Here the question and list statement are, "When hiring a new employee, do you put more weight on their socioeconomic background than on their technical skills?" About one-third of establishments asked this question admit to this behavior directly, with the share similar across industries.

Using the list randomization, however, the figure is 54%, economically and statistically significantly higher than implied by the direct question. Discrimination by SES is widespread, about as prevalent as discrimination by gender. The key difference is that it seems to be more stigmatized than preferring men over women, with the list method giving a significantly higher estimate than the direct question. The fact that we find this large difference for SES gives us confidence that our list randomization has enough power to detect true differences. This gives us increased confidence in our gender results.

Using the list randomization, we see a wide variance by industry that mirrors that by gender: half of retail establishments and most hotels and restaurants put more weight on SES, while very few IT establishments do. The standard errors are again large, but the pattern is striking. So while the direct question implied that discrimination was similar across industries, the list randomization method is more in line with the gender discrimination results, where the IT industry appears to have the lowest rates

Table A1: Discrimination by Socioeconomic Background

| | Percent of establishments that put more weight on SES than skills | | | | |
	Overall	Retail	IT	Hotels	Restaurants
Direct question (n=525)	32.3%	35.6%	33.4%	15.7%	33.5%
	[27.2, 37.5]	[28.0, 43.3]	[19.2, 47.7]	[-1.4, 32.8]	[25.4, 41.5]
List randomization (n=1,072)	53.8%	49.1%	1.92%	75.2%	65.2%
	[36.9, 70.6]	[22.2, 75.9]	[-40.8, 44.6]	[9.7, 140.7]	[39.8, 90.5]

The 95% confidence interval is given in brackets below each figure. The direct question is the percentage of establishments answering yes to the question, "When hiring a new employee, do you put more weight on socioeconomic background than on their technical skills?" The list randomization figure is the difference in mean number of true statements reported by the treatment group and the control group. Robust standard errors are used. We exclude all establishments with fewer than 5 employees due to a lack of data about them in the Egyptian census of establishments.

of discrimination.

Note that, because establishments are not all being honest here, we are unable to convincingly analyze the determinants of being a discriminating establishment (as we did in Table 1, Panel B for gender), nor are we able to analyze the consequences of discrimination by SES (as we did in Table 2 for gender).

Appendix: Additional Tables

Table A2: Gender Discrimination and Hiring Outcomes (Uniform Sample)

Dependent variable:	(1) Wks to Hire	(2) Wks to Hire	(3) Terminations	(4) Terminations	(5) Wks to Productivity	(6) Wks to Productivity
Bias against female candidates	-0.461	-0.179	0.519*	0.143	0.310	1.377**
	(0.534)	(0.458)	(0.267)	(0.253)	(0.654)	(0.652)
Above-median estab. employment	-0.199	-0.109	-0.353*	-0.199	1.880***	1.700**
	(0.441)	(0.384)	(0.209)	(0.191)	(0.674)	(0.735)
Formal, part of larger org.		1.081**		0.259		0.880
		(0.511)		(0.267)		(0.713)
Institute/college ed. (std. proportion)		0.210		-0.100		0.500*
		(0.298)		(0.117)		(0.302)
Unemployment rate		-0.217**		-0.068		-0.177
		(0.090)		(0.071)		(0.146)
Marginal interaction effects of employer gender discrimination:						
Formal, part of larger org.		-0.993		-0.494		0.379
		(0.796)		(0.472)		(1.170)
Other type		0.083		0.448*		1.768**
		(0.521)		(0.263)		(0.740)
Difference in marginal effects		-1.075		-0.942*		-1.389
		(0.900)		(0.489)		(1.298)
Institute/college ed.–75th pctile		0.403		0.115		1.541**
		(0.561)		(0.259)		(0.738)
Institute/college ed.–25th pctile		-1.300**		0.184		0.983
		(0.523)		(0.354)		(0.810)
Difference in marginal effects		1.703***		-0.069		0.558
		(0.647)		(0.323)		(0.847)
Above-median estab. emp.: Yes		-0.837*		0.064		3.659***
		(0.507)		(0.294)		(1.272)
Above-median estab. emp.: No		0.106		0.163		0.347
		(0.576)		(0.327)		(0.692)
Difference in marginal effects		-0.943		-0.100		3.311***
		(0.678)		(0.411)		(1.369)
Observations	421	421	421	421	421	421
Pseudo R-squared	0.001	0.032	0.014	0.076	0.005	0.030

Notes: This table repeats the analysis in Table 2 of the main paper but holds the sample constant across regressions (dropping any observations with any missing values). Robust standard errors in parentheses. *p<0.10 **p<0.05 ***p<0.01

Table A3: Gender Discrimination and Hiring Outcomes (All Controls)

Dependent variable:	(1) Wks to Hire	(2)	(3) Terminations	(4)	(5) Wks to Productivity	(6)
Bias against female candidates	-0.482	-0.187	0.446**	0.172	0.587	1.312**
	(0.513)	(0.458)	(0.212)	(0.222)	(0.528)	(0.553)
Above-median estab. employment	-0.207	-0.104	-0.247	-0.275	1.755***	1.608**
	(0.424)	(0.383)	(0.163)	(0.185)	(0.555)	(0.638)
Formal, part of larger org.		1.077**		0.167		0.890
		(0.509)		(0.226)		(0.699)
Institute/college ed. (std. proportion)		0.217		-0.094		0.326
		(0.297)		(0.095)		(0.275)
Unemployment rate		-0.217**		-0.057		-0.110
		(0.090)		(0.047)		(0.105)
Advanced writing		0.307		0.429		0.458
		(0.616)		(0.375)		(0.659)
Computer usage (standardized)		-0.124		0.359**		-0.361
		(0.229)		(0.145)		(0.262)
English speaking		0.118		0.053		0.862
		(0.513)		(0.218)		(0.586)
High level of technology		-0.450		-0.369*		0.026
		(0.478)		(0.209)		(0.641)
Provide formal/informal training		-0.106		0.131		0.234
		(0.529)		(0.195)		(0.596)
Shift to inside hiring		-0.429		0.082		-0.157
		(0.477)		(0.200)		(0.553)
IT industry		1.210		-1.129***		3.601***
		(0.985)		(0.326)		(1.265)
Hotel industry		1.485		-0.742*		0.803
		(1.161)		(0.410)		(1.043)
Restaurant industry		0.377		-0.729**		-0.499
		(0.637)		(0.290)		(0.553)
Wage above competitors		-1.277***		0.955**		-0.081
		(0.392)		(0.409)		(0.605)
Wage below competitors		-1.057*		-0.177		-1.807*
		(0.549)		(0.448)		(1.018)
Experience frequent poaching		-0.510		2.083*		-1.006
		(0.411)		(1.087)		(0.703)
Marginal interaction effects of employer gender discrimination:						
Formal, part of larger org.		-0.993		-0.398		-0.001
		(0.794)		(0.410)		(1.149)
Other type		0.071		0.477**		1.744***
		(0.521)		(0.236)		(0.618)
Difference in marginal effects		-1.064		-0.875*		-1.745
		(0.896)		(0.458)		(1.270)
Institute/college ed.–75th pctile		0.400		0.076		1.195**
		(0.561)		(0.224)		(0.609)
Institute/college ed.–25th pctile		-1.311**		0.393		1.513**
		(0.521)		(0.285)		(0.770)
Difference in marginal effects		1.711***		-0.317		-0.318
		(0.645)		(0.263)		(0.798)
Above-median estab. emp.: Yes		-0.830		0.116		3.716***
		(0.506)		(0.251)		(1.119)
Above-median estab. emp.: No		0.091		0.171		0.404
		(0.574)		(0.288)		(0.602)
Difference in marginal effects		-0.921		-0.056		3.313***
		(0.675)		(0.359)		(1.204)
Observations	443	423	537	514	541	516
Pseudo R-squared	0.001	0.033	0.012	0.085	0.005	0.027

Notes: Each column reports results from negative binomial regressions using establishment employment weights. Robust standard errors reported in parentheses. All regressions include industry fixed effects. All estimates are weighted by establishment size to match the size distribution in the Egyptian census of businesses. We exclude all establishments with fewer than 5 employees due to a lack of data about them in the Egyptian census of establishments. The pays above/pays below variables are missing for all of the retail sector, we replace them with a constant which is made irrelevant by the industry fixed effects. *p<0.10 **p<0.05 ***p<0.01.

Table A4: Non-response on Primary Outcomes

Outcome: Non-response = 1	(1) Wks to Hire	(2) Terminations	(3) Wks to Productivity
Formal, part of larger org.	-0.012	-0.008	-0.012**
	(0.033)	(0.007)	(0.005)
Education level	-0.018	-0.002	0.000
	(0.016)	(0.002)	(0.002)
Advanced writing required	-0.006	0.006	-0.005
	(0.040)	(0.014)	(0.008)
Computer usage required	-0.007	-0.001	-0.002
	(0.015)	(0.003)	(0.003)
English speaking required	-0.040	-0.005	0.003
	(0.035)	(0.009)	(0.005)
High level of technology	-0.003	0.003	0.012
	(0.034)	(0.010)	(0.008)
Provides training	-0.175***	0.014**	0.013**
	(0.030)	(0.007)	(0.006)
Wages above competitors	0.073**	0.012	0.005
	(0.037)	(0.013)	(0.008)
Wages below competitors	0.080	0.000	0.003
	(0.091)	(0.011)	(0.010)
Unemployment Rate	0.001	0.000	0.000
	(0.006)	(0.001)	(0.000)
Bias against female candidates	-0.066**	0.009	0.003
	(0.030)	(0.007)	(0.006)
Number of Employees	-0.093***	-0.003	-0.002
	(0.030)	(0.006)	(0.005)
Shift to internal hiring	0.044	0.004	0.007
	(0.030)	(0.007)	(0.005)
Experience frequent poaching	-0.059	0.019	0.024**
	(0.036)	(0.012)	(0.012)
N	541	541	541
R-squared	0.382	0.845	0.915

Notes: Each row reports the results of a regression of a binary variable that is equal to one if the establishment did not response to the question related to the outcome variable in the column, on the variables reported in each row. All regressions include industry fixed effects. Robust standard errors reported in parentheses. The pays above/pays below variables are missing for all of the retail sector, we replace them with a constant which is made irrelevant by the industry fixed effects.*p<0.10 **p<0.05 ***p<0.01.