# Designing Gender Equity: Evidence from Hiring Practices and Committees*

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**Abstract**

This paper analyzes how the design of hiring practices and who conducts them affect gender equity in hiring. I transform tens of millions of high-dimensional, unstructured records from Brazil's public sector into detailed information on candidates, evaluators, screening tools, and scores. Exploiting exogenous variation in the mix of screening methods used by employers, induced by a federal policy, I find that increasing screening impartiality improves women's evaluation scores, application rates, and probability of being hired. To isolate and quantify the roles of disparate treatment, disparate impact, and screening precision from individual hiring practices, I develop a framework exploiting multiple treatment types generated by the reform. I find that the most effective changes to increase women's hiring odds involve i) adding blind written tests to a hiring process that already uses subjective methods, such as interviews, or ii) converting subjective rounds into only blind written tests. However, when employers remove subjective stages, gender hiring gaps remain unchanged, revealing that screening precision loss dominates disparate treatment removal. Finally, more gender-balanced hiring committees induce male evaluators to become more favorable toward female candidates in subjective stages. This is consistent with more costly bias expression in the presence of minority colleagues. Overall, my findings show that gender disparities in hiring arise from both screening practices and decision makers.

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# 1 Introduction

In recent decades, firms have increasingly devoted resources to grapple with a lack of gender diversity and under-representation of women at various levels of the corporate ladder. US firms alone spent more than $10 billion in 2003 in initiatives to reduce unconscious and intentional bias in recruiting (Hansen (2003)). Most strategies to improve hiring rates of women and other minorities focus on diversity training programs and goals. By most accounts, these initiatives have proven disappointing, as they fail to recognize that the design, implementation, and decision-making during the hiring stage is a key source of low diversity.[1] Certain hiring practices that are considered important predictors of future productivity may disadvantage a particular group, hiring managers may be biased in difficult ways to observe, or firms may simply fail to attract enough applicants from minority groups.

Even though screening and the selection of employees is a central part of every firm and organization, the questions of how to best design processes that are bias-free, improve employee diversity, and select the best candidates, remain open. Employers are reluctant to share hiring practices or details on hiring processes, often engaging in lengthy legal battles to keep the information from going public. Moreover, even if researchers were able to get detailed data on hiring practices and decisions, generating appropriate variation for causal inference would remain a challenge. As Oyer and Schaefer (2011) put it: "What manager, after all, would allow an academic economist to experiment with the firm's screening, interviewing or hiring decisions?".

In this paper, I study how the design of hiring practices and who conducts them determine gender disparities in labor market outcomes. I open the black box of hiring processes by constructing uniquely-detailed information on the universe of selection processes from Brazil's public sector. Brazilian law mandates that every step of public servant selection be carefully documented. To access, extract, and transform these records into data, I develop a natural language processing algorithm that distills over 35 million official government text documents. This generates a rich database that includes all job openings, job offers, characteristics of job postings, job-applicant and hiring-manager names, and applicant scores by screening method and manager.

Equipped with a large-scale data set detailing job applicant performance and evaluators' decision making process, I first exploit a reform in the provisions regulating the selection of

[1] Diversity programs are intended to foster firm diversity and encompass a range of company-specific initiatives, including diversity training and evaluations, de-biasing, networking and mentoring programs. Kalev et al. (2006) find no relationship between diversity training programs and employee diversity in a sample of over 800 U.S. companies, and some are even associated with a decline in the representation of disadvantaged groups. Furthermore, diversity training aimed at raising awareness about gender inequality can backfire due to moral licensing (Bohnet (2016)).

public sector employees in Brazil's 1988 Constitution. The reform required government employers at all levels to conduct impersonal and impartial hiring processes, although only federal employers implemented it immediately. State and local governments conduct employee selection independently from central authority, and started addressing the legal changes necessary to implement impartial hiring processes only much later than the federal sector.

The impartiality reform induced variation in the mix of screening tools and hiring practices for federal jobs on multiple fronts. First, using written exams without concealing candidates' identity would be a clear violation of the new rules, leading employers to blind tests. Second, because the reform did not specify which hiring practices had to be implemented to achieve impartiality, multiple treatments to changes in screening methods were generated. Employers modified their mix of hiring tools following occupation-specific historical reliance on certain stages (e.g., oral exams for judges) and customary practices (e.g., typing speed and accuracy for secretaries).

The presence of several complier types allows me to tease out different forces that determine hiring rates, as well as to understand which design changes in selection processes are effective at increasing gender diversity. Should an employer remove screening practices that entail high discretion even if they may provide employers with important information for screening? Does replacing interviews with objective or standardized tests help or hurt female candidates? Does blinding written exams help at all if no other changes are made in the screening process? To answer these questions, the variation generated by the policy allows me to construct counterfactuals not only with respect to the usual untreated group (no changes in screening methods), but that compare alternative choice paths for screening practices.

To establish the policy take-up, I analyze how the design of hiring processes changed in federal jobs relative to states. Federal employers responded sharply to the impartiality reform. Job announcements started including rules detailing written examinations that were to be conducted without information on candidate names, clearly indicating an effort to comply with the impartiality requirement. Relative to the same occupation in state hiring processes, federal jobs became more likely to use written (or multiple-choice) exams, less likely to use a non-written tool, and decreased the number of job processes that relied solely on non-written stages by 25 percentage points.

How did greater impartiality affect male and female job candidates? To study the overall effects of the reform, I employ a difference-in-differences design comparing job processes in the same occupation in federal and state governments. I estimate that following the reform, women's final score in job processes increased by 0.07 standard deviation, accompanied by a decrease of a similar magnitude in men's scores. This resulted in a drop in the gender score gap of 0.14 standard deviation. Confirming that the policy induced intensive margin changes

only in the scores of written exams — which had to be blind — the gender gap in these stages also decreased, while relative scores in non-written tools between men and women had no statistically significant changes.

The decrease in the gender final score gap translates into improved hiring rates of women and a narrower gender hiring gap. Using information on the entire candidate pool – which is rarely available to researchers – allows me to look at applicants' probability of being hired conditional on gender instead of measuring hiring gaps from a sample of hired workers, which confounds employer behavior with application rates. This distinction is important because the design of hiring practices may affect both the minority and majority candidate pool sizes. More broadly, extensive margin responses in job processes with a fixed number of openings may differentially crowd out male and female candidates via increased competition.

I estimate that women became 0.3 percentage points more likely to be hired and men's hiring rates decreased by 0.4 points, implying a reduction in the gender hiring gap in federal jobs of about 44% of the pre-treatment level, even after controlling for job process competitiveness. Interpreting these estimates in light of two advantages to my setting — screening methods for a job process are decided at higher bureaucratic levels and not by hiring managers, and results from all screening stages had to be incorporated into the job offer decision following pre-determined rules — indicate that changes in the underlying mix of screening practices and conducting blind written exams successfully reduced gender disparities originating from an employer's behavior in the federal sector hiring.

The slow progress made by women in the labor market has been sometimes attributed to supply-side explanations.[2] By investigating the separate response of application behavior induced by the policy, I find that application rates of women relative to men increased about 1 percentage point, implying a supply-side response 40% larger than the increase in employer's demand. Taken together, both a larger employer demand for female candidates and application growth result in a 13% increase in gender diversity among employers just a few years after the policy came into effect. This is an important finding, as it shows that the way employers screen applicants can increase women representation among applicants.[3]

To guide the interpretation of the effects on gender hiring equity from multiple changes in screening practices, I build on a classic statistical discrimination model by incorporating

[2]The gender gap in competition participation is a potential factor in explaining gender differences in career choices and labor market outcomes (Bertrand (2011)). Several studies document gender differences in applying for promotions (Hospido et al. (2019), Bosquet et al. (2019)), as well as selection into competitive environments more generally (Niederle and Vesterlund (2007)). Suboptimal entry by high-performing women can also be costly for employers as it prevents firms from hiring the best candidates.

[3]A few recent studies show how the composition of the job applicant pool responds to simple changes in the wording of job vacancies (Del Carpio and Guadalupe (2021), Abraham and Stein (2020)), as well as to signaling a preference for employee diversity in the content of recruiting information (Flory et al. (2021)).

screening tool characteristics and the role of managers, who conduct the screening on behalf of the employer. I allow hiring managers to be biased toward a certain demographic group, with the degree of expression of this bias regulated by how much discretion a specific hiring practices enables. Interviews allow for high levels of discretion due to their subjective nature, while the results from formal tests are more easily observable to the firm, making bias expression more costly. Independently of manager preferences, screening tools provide a productivity signal with certain precision and potentially mean-biased — where the bias term absorbs group-favoring characteristics of a given practice — generating disparate impact even if managers are unbiased. The conceptual framework pins down different considerations that employers face when designing hiring processes with the goal of minimizing inequities without necessarily an efficiency trade-off.

To test the model predictions, I conduct a multi-treatment analysis. Under a set of assumptions that enable the construction of counterfactuals between changes in screening tools that had the same pre-policy mix, I first estimate the treatment effects of keeping the pre-existing written exam and blinding it. The estimated reduction in the gender hiring gap of 0.5 percentage points (relative to 1.5 p.p. pre-policy) measures the improvement in diversity from completely eliminating evaluator bias, since now evaluators cannot express disparate treatment, nor rely on statistical discrimination. This result shows that even in a context with likely low levels of evaluator discretion, disparate treatment may still play an important role in determining gender gaps in labor market outcomes.

The next set of comparisons I make starts with a screening process that only used non-written methods — mainly interviews and oral exams — a context with high discretion and potentially subject to a large evaluator bias. Starting from a gender hiring gap of almost 17 percentage points, replacing the non-written exam entirely with a blind written test increases female hiring odds by 7 percentage points relative to men. The change from a very subjective assessment tool to a blind, objective tool yields the most dramatic number of changes in the three factors determining hiring rates. Because the net result from changes in screening precision and tool bias may depress females hiring rates, the large estimated coefficient suggests that either written exams have higher precision or a smaller disparate impact than non-written tests, or that the combined magnitude of these channels is small relative to the size of the disparate treatment in interviews.

A second treatment type of hiring processes relying on interviews before the policy involves keeping the subjective tools, but adding a blind written exam to increase the overall objectivity of the hiring process. Despite the possibility of introducing a disparate impact from a tool such as standardized testing, I estimate an increase in women's hiring rates relative to men's of about 5.9 percentage points, or 35% of the initial gap. By introducing an additional

screening stage, employers increase screening precision, which helps minority candidates both directly and by diluting the contribution from the interview signal, which is still influenced by group-based priors and disparate treatment.

These changes to screening methods are all successful in increasing female hiring rates. More importantly, two of them — blinding a pre-existing written exam and introducing a blind written test in a process with an interview — can only maintain or increase the average productivity of hired employees. The lack of a equity-efficiency trade-off in these cases is intuitive: redesigning these practices implies reducing biased procedures that kept equally qualified minority applicants below the hiring threshold.

The last set of comparisons illustrates the potential role of well-intentioned changes in screening practices that do not affect greater diversity. Employers who employed both written and non-written tests (and hence non-blinded), while (i) both removing interviews from the screening process and (ii) blinding the written stage, saw no changes in female hiring rates relative to men. In this case, even though the employer completely removed evaluator bias from the process, the loss in screening precision depressed female hiring rates. The other counterfactual in this case, which keeps the interview stage and only blinds the written exam also produces no discernible effect on female hiring rates.

There are several lessons from analyzing the different treatment groups. First, gender disparities in hiring come both from screening practices — either by their differences in precision or the existence of disparate impact — and decision makers. Second, decision makers matter even in instances where the tools being employed provide relatively objective signal and limit bias expression. Third, concealing candidate identity in an existing test benefits the less-favored group unambiguously without introducing implying in efficiency loss. However, blinding alone may not be enough to improve gender diversity. Fourth, introducing blind tests helps even further, as long as bias-free screening precision gains offset potential majority-favoring biases in other stages of the job process. In fact, removing subjective tests and blinding exams fails to improve women's outcomes, suggesting that employers should carefully weigh precision loss and net gains from bias reduction.

In the final part of the paper, I study a complementary approach to improving gender equity in hiring: changing decision makers. While changing screening tools to limit discretion is an intuitive idea and, as my results show, can lead to significant advances toward diversity, redesigning the mix of evaluation stages may be complicated in practice if employers have limited information on relative disparate impact and precision between written and non-written exams. Indeed, some changes in screening tools that appear reasonable, might be ineffective, for example, when employers remove interviews without introducing another less biased stage.

Even if employers keep the same screening design, they can still limit bias expression by evaluators. Rather than focusing on de-biasing or other training methods, I expand on the idea that hiring managers face an increasing cost when expressing bias in more objective tools by incorporating a penalty function that depends on the hiring committee composition. The analysis follows the same logic as that of a series of corporate and public policies incentivizing or enforcing more diverse committees.[4]

Exploiting more recent data from Brazil's public sector job processes, I first leverage information on candidate scores by exam type and hiring committee member to study how changes in the gender composition of committees affect female and male candidates and hiring managers. Even though most job processes include a mix of blind-written and non-written tools, women have a slightly lower final score and hiring probability than men. Decomposing the final score into each evaluation round reveals that female candidates receive identical scores on resumes and blind exams, but are scored on average 4 percentage points less than men on non-written exams.

To separate out the confounding effect of individual differences in skills between the two examination types (or disparate impact from the tools) and disparate treatment in interviews and oral tests, I compare the difference between non-written and blind written scores of the same candidate across job processes with different committee gender compositions. Estimated effects show that the non-written penalty for female candidates decreases when there are more women in the committee, as well as their final scores and chances of job offers.

To better understand the forces driving the reduction in biased evaluations of female candidates when the hiring committee has more female members, I analyze how the the same female evaluator scores different female candidates when she participates in hiring committees with different shares of female colleagues. I find little evidence of any change in behavior from female committee members, who only marginally improve non-written scores relative to blind exams that they give to female applicants. In contrast, male evaluators increase their non-written scores given to women relative to men when they have more female colleagues in the hiring committee. As expected, this effect does not appear in blind exam scores, and imply a decrease in evaluator bias of about 1.4 percentage points.

One possible interpretation is that by adding more minority members to a group, two effects contribute to the change in men's behavior. The first is that even though hiring members evaluate each candidate independently, they are allowed (and often do) to share their opinions on candidates' performance, potentially changing their scores before final submission. While this process plays no role in objective measures like written tests, because interviews or oral

[4]For example, Norway passed a law in 2006 that mandated a gender quota of 40% on corporate boards, with other European countries following lead (Bertrand et al. (2019)).

examinations are usually conducted with all hiring members present, this can change behavior and perception during the evaluation and after, when hiring members share opinions about candidates. If members of the majority group believe that other majority members are more likely to hold the same preferences and display similar behavior over the minority group, having members of the minority group observing their decisions may give rise to a bias censoring effect.

This paper makes several contributions to the literature. Most closely related to my work, a body of observational studies examines how screening practices impact equity outcomes. One line of this research has focused on the effects of hiding candidates' identity, starting with the important work by Goldin and Rouse (2000) who show that blind auditions in orchestras increase the likelihood that women musicians are hired. More recent papers include the study of anonymized CVs (Behaghel et al. (2015), Krause et al. (2012), Åslund and Skans (2012)).[5] Another strand has looked at how introducing testing in low-skill jobs helps minorities (Autor and Scarborough (2008) and Hoffman et al. (2018)).[6]

My setting allows me to have a uniquely detailed analysis of screening practices and decisions with plausibly exogenous source of variation in the use of different practices. Screening in Brazil's public sector also involves multiple occupations across all skill levels, most of them with similar counterparties in the private sector, and have job processes structured in a similar way as in other sectors. More importantly, different combinations of tools enable me to separate the effects of screening precision, evaluator and tool bias which require different policy responses to address low diversity.

My paper also adds to the large literature investigating discrimination in hiring using audit and correspondence (AC) studies (Neumark (1996), Bertrand and Mullainathan (2004), Kline et al. (2021)) or natural experiments (Goldin and Rouse (2000)). A common limitation across these papers is the difficulty of observing hiring practices, manager behavior, and the step-by-step results within job processes. Employee selection behavior is effectively a black-box, and the contribution of screening methods or decision makers to observed racial or gender gaps is unknown. By tracking candidate and evaluator results for each screening stage and tool, my paper provides novel evidence that can instruct employers and policymakers on

[5]Several other papers have studied different consequences of partially concealing or including some information about job applicants on labor market outcomes, e.g., age (Neumark (2021)), credit information (Bartik and Nelson (2021)), criminal records and history checks (Holzer et al. (2006), Agan and Starr (2018), Doleac and Hansen (2020)), and drug testing (Wozniak (2015)).

[6]In experimental evidence, Bohnet et al. (2016) examine joint vs. separate evaluation of candidates and find that evaluators are more likely to use gender stereotypes when evaluating one candidate separately, and more likely to base their decisions on individual performance in joint evaluations, as decision makers have more information to update their possibly-biased beliefs.

how to achieve gender equity in hiring. I find that biased hiring managers generate disparate treatment even when the evaluation measure is relatively objective.[7]

A second drawback from experimental studies in discrimination is that they usually can only measure call back rates in the early stage of screening processes instead of the final hiring decision. While audit and correspondence studies have generally found racial gaps at this first screening stage, conclusive evidence on gender disparities is far less apparent (Bertrand and Duflo (2017)), with studies leading to conflicting results (e.g., Kessler et al. (2019), Booth and Leigh (2010)). Even if the range of estimated callback gender differentials provided consensus, the measure may not even predict systematic biases at the hiring decision stage (Cahuc et al. (2019)). My estimates capture gender disparities at the different stages of hiring processes both for performance evaluation and job offers, providing a complete picture of disparities in hiring.[8]

My results also shed light on the role and contribution of decision makers in generating gender disparities. In turn, they provide guidance on how to design hiring committees and implement screening tools that curb evaluators' bias expression. These results contribute to the existing empirical evidence on the importance of evaluator's gender available from specific settings (Broder (1993)) and Card et al. (2019) for grant and journal reviewers, academic promotion committees in Italy and Spain (De Paola and Scoppa (2015), Bagues et al. (2017), public examinations of the Spanish judiciary (Bagues and Esteve-Volart (2010)), and teachers (Lavy (2008), Breda and Ly (2015))), which have offered results ranging from the gender of who evaluates having no effect on candidate results, to female and male evaluators judging women more harshly or less harshly.

My paper finds that hiring manager behavior interacts with the choice of screening tools, as practices with higher degrees of discretion allow for differential degrees of bias expression. When considering hiring committees where evaluation decisions are made individually, but members engage in discussions and their decisions are made publicly, having a similar gender ratio in the committee is likely to balance potential bias in male evaluators.

Fourth, this paper relates to the growing literature on personnel economics of state (Finan et al. (2017)) that has studied how governments can change the applicant pool, and the charac-

[7] This is in line with Sarsons (2019), who finds that referring physicians judge surgeons with the same objective performance record differently depending on their gender.

[8] A more subtle point with respect to why AC-type studies may fail to detect gender discrimination in hiring is that the set of employers available to researchers to send resumes tends to be limited to low-skill, entry level jobs. These jobs are more likely to accept email or online applications and offer less chances of detection of fictitious resumes (Neumark (2018)). However, because these occupations are commonly female-dominated, female candidates might be preferred to otherwise identical men. Moreover, without measurements of higher-level jobs, where women tend to be under-represented compared to entry-level occupations, it is difficult to trace some of these estimates to the economy more generally, or to connect callback gender gaps to hiring or wage gaps.

teristics of individuals that are an important determinant of performance in the public sector (e.g. Dal Bó et al. (2013), Deserranno (2019)). However, given the large public sector premium in many countries and the fact that most government jobs tend to be over-subscribed (Finan et al. (2017)), the type of employees who are hired will ultimately depend on how candidates are chosen, since inadequate screening procedures can undo positive selection.[9]

Fifth, this paper provides a methodological contribution to the growing use of text analysis tools in empirical economics. Researchers have relied mostly on *ad hoc* dictionary methods to parse and interpret information in text form into a predictor of underlying phenomena (e.g., media slant (Gentzkow and Shapiro (2010)), policy uncertainty (Baker et al. (2016))). In many applications, however, researchers are interested in extracting actual structured data from text, a task that is especially challenging when the text is displayed without regular layout and contains confounding information. The natural language processing algorithm I develop leverages semantic patterns of messages containing numeric data, without being constrained by the shape of raw text. This query-based approach offers another text analysis tool to new methods being developed in economics, like that from Shen et al. (2021), which exploits structure patterns to identify text regions in complex layouts.

# 2 Institutional Details & Setting

## 2.1 Overview

Brazil's public sector is a vital part of the country's economy. Federal, state, and local governments employ about 13% of the Brazilian workforce, a similar share to OECD countries, including the US. Brazil's government offers an expansive array of services, from universal healthcare to free pre-K to 12 and college education, controls thousands of state-owned enterprises and agencies from oil exploration to banking services, among many others. The hiring stage of public servant selection in the country is particularly important, as public sector employees receive automatic life-time tenure after being hired and termination is only possible following serious misconduct provisioned in a narrow set of rules, such as peculate or other forms of corruption, lobbying, and post abandonment. Wages are fixed and generally compound on a time-in-office basis and mechanically by inflation.

[9]Some papers have studied how patronage affects allocation of public sector positions (Xu (2018), Colonnelli et al. (2020), Brollo et al. (2017)), and the effects of civil service reforms transitioning from discretionary appointments to meritocratic systems (Estrada (2019), Moreira and Pérez (2021a), Moreira and Pérez (2021b)). This paper examines how changing screening methods within a meritocratic system affects labor market outcomes.

## 2.2 Public Servant Selection

Brazil was the first country in Latin America to establish a formal, merit-based career civil service. It is considered a primary example of a meritocratic and legally professionalized civil service system (see Grindle (2012) and Figure A.3 for a complete history of meritocracy implementation and public servant selection rules). Over 70% of public sector jobs are allocated through a mandatory legal device known as "*Concurso Público*" (Public Tender), a highly competitive and structured process, referred to by Brazilians simply as *Concurso*. The entire *Concurso* must be conducted and reported transparently, with every step of the process recorded and published in a designated daily government gazette (similar to the Federal Register).[10]

Each job selection process follows the same general steps depicted in Figure 1. The first posting regarding a hiring process — the job announcement — is called *Edital de Concurso*. This is a legally-binding set of rules that must describe in detail all pertinent information about the job posting, how the hiring steps are organized and conducted, the composition of the hiring committee, as well as other rules and guidance. Specific job announcement details are job and employer dependent, potentially varying within the same employer. However, every job process must follow the general guidelines prescribed in the Constitution and must integrally respect the rules laid out in the job announcement.[11]

The same *Concurso* may aim to hire multiple applicants for one job title and opening, multiple openings or job titles, for the same or distinct locations. The timing of job announcements and whether an employer conducts multiple separate hiring processes to fill out open positions or only one broad *Concurso* are determined by a complex bureaucratic process. This process requests that the government employer manifest intent in filling out or expanding specific job titles to the appropriate oversight budget and comptroller offices, which then decides whether the job posting should be greenlighted.

The hiring process proceeds as follows. First, candidates apply to the job opening, have their applications screened based on announced requirements (e.g., be a Brazilian citizen, have a valid medical license, attain the education level required), and have their names published on a subsequent journal issue. At this stage, the entire pool of candidates is publicly visible, with information on full names and often some personal identification such as date of birth, individual taxpayer identifier, or identity card number. The authority organizing the hiring process then publishes individual performance/scores on each selection stage as the hiring process un-

[10] Some public sector jobs are exempt from the formal civil service selection procedure, including temporary jobs, positions of trust, and commissioned posts. These jobs are particularly common in occupations closely related to politicians like congressional staff.

[11] Because wages are fixed and determined by law, job announcements always detail the entry wage and benefits, skill required for a candidate's application to be officially accepted in the hiring process, hours worked etc.

folds, including interviews and tests, and identifying the candidates who are ultimately offered jobs, wait-listed, and hired.

## 2.3 External Validity

Brazil's public sector job processes provide a relevant laboratory to study hiring decisions and practices used well beyond the public sector. *Concursos* employ a combination of screening tools that are widely used in the private sector. Exam types can be divided into written or multiple choice tests, resume analysis, interview, oral examinations, and practical evaluations. When job processes include the use of several screening methods, initial stages usually use tests and written exams of general or specific knowledge (e.g., math, language, laws) to filter out candidates, with more subjective methods being applied at later evaluation stages. Practical exams typically test job-related skills, such as typing speed and accuracy for secretaries, foreign language conversations for translators, circuit driving for drivers, teaching presentations for teachers and professors, etc.[12]

Because I observe the universe of public sector hirings, the occupations and skill distribution of these job selection processes also offers a direct comparison to identical or similar occupations outside public administration. In practice, public sector employers compete for candidate pools with the private sector in most occupations: cleaning personnel, janitors, lawyers, accountants, teachers, police or private security services, doctors, secretaries, telephonists.

The structure and characteristics of public servant recruitment in Brazil also shares similarities with public sector hiring of several other countries, including France, Italy, Spain, and India, as well as organizations like the European Central Bank (Hospido et al. (2019)). Generally, applicants to public sector positions in these countries are also subject to competitive exams with rigid rules, and government jobs offer similar amenities (life-time tenure, fixed pay and career progression structure).

More generally, because hiring managers know that the output of their decision-making process is publicly available, one might posit that the expression of intentional bias is less likely relative to settings where hiring practices and decisions are privately observed only by the firm. However, firing or forced transfers in the public sector are also extremely difficult, so hiring managers may especially care about highly subjective traits, such as "culture fit".

In terms of gender attitudes in Brazil around the time the reform took place, the patterns of beliefs about gender roles were broadly in line with countries like Chile, South Korea, but were more egalitarian than India or China. When asked whether they agreed withe the state-

[12]Before that, under-qualified candidates (defined as having less schooling or credentials than indicated in the job announcement) have their application requests immediately rejected.

ment "*when jobs are scarce, men have more of a right to a job than women*", 25% of US men and women would say yes (Figure A.1), compared to about 37% of Brazilians. By 2010, gender attitudes in the South American country had improved considerably, reaching the same numbers as the US in 1990 and several points ahead of South Korea, China, India, and Egypt. The country's female share of the labor force followed a similar convergence path to developed nations during the period, but at the time of impartiality reform stood at two-thirds of the US rate (Figure A.2).

## 2.4 Setting Advantages and Limitations

Several features in the organization and implementation of hiring processes in Brazil's public sector make it an almost ideal setting to study the roles played by different screening tools and the identities of screeners. First, committee members usually have no say in choosing which tools are used in a selection process, since the screening method mix is determined at a higher organizational level or by public technicians and legal specialists, usually in line with strong historical and customary use depending on the occupation.[13] Moreover, because the type, order, and weight of each exam is described in the *Concurso*'s job announcement — the *Edital* —, these are all legally binding rules that if not enforced, result in the invalidation of the entire process or any potentially resulting hires.[14]

Due to the structured and quasi-exogenous nature of selection processes, evaluators cannot ignore or disregard scores: the final job offer decision is entirely determined by candidate ranking (based on exam scores), and the number of job openings.[15] These features provide a clean setting to measure both the effects of screening tools and who screens on applicant outcomes.

Of course, every observational study is subject to a missing data or incomplete information problem. Some historical scanned government gazettes documents were severely damaged and unreadable through any optical character recognition (OCR). In other cases, part of a job processes information was missing due to water marks or stains, in which case I drop the job process altogether. Another inconvenience related to older documents is that most states do not maintain online repositories with long time series of documents, even if these documents are stored in government libraries and official agencies.

[13] For example, oral examinations have been used to select judges, prosecutors, public defendants, and other judiciary members for centuries, class-style presentations are often used to screen teachers and professors as well as written exams for auditors and analysts.

[14] If an announcement contained minor errors or typos, or certain non-fundamental information changes (e.g., the specific room where examination take place), the publishing agency must post a new notice in the government gazette communicating corrections.

[15] I test for the empirical validity of hiring probability conditional on final scores in Figure 5.

# 3 Opening the Hiring Black Box: Data Extraction

## 3.1 Raw Text Sources

The raw data used in this paper come from over 35 million of official journal pages of federal, state, and local governments in Brazil (known as "*Diário Oficial*") from 1970 until 2020. These gazettes are similar to the Federal Register in the US and publish the universe of public notices spanning public procurement processes, executive orders, and information on public servants. Such notices on public sector personnel include the entirety of every public sector employee hiring process (as shown in Figure 1) and professional relevant events of current employees (e.g., promotions, licenses, sanctions). Every government branch maintains its own decentralized repository with daily scanned issues of official journals, which I first scrape and retrieve in order to assemble a dataset with specific government-level journals over time. Table A.1 shows a complete list of the separate government entities used to retrieve the government gazettes, as well as when issues first become available online.

The next — and most challenging — step is to extract the hiring data from these documents. To organize ideas, consider the following sequence of tasks necessary to automate the construction of a comprehensive large-scale applicant-reviewer panel:

**1**. *Filter out all text contained in official government documents unrelated to hiring steps.*

**2**. *Define the boundaries of the relevant text.*

**3**. *Identify the underlying job process of a certain relevant text.*

**4**. *Link different postings belonging to the same process.*

**5**. *Transform text in each posting into data.*

Due to the layout of Brazilian official journals, each step above presents a host of issues. First, there are no boundaries between the text a job posting and other information — say another job posting or a list of government contractors suspended — so that defining the domain of relevant information ex-ante is difficult. Then, because surrounding text may be of a similar nature, filtering out extraneous information that does not belong to a specific job process is also challenging. Further complicating matters, the different stages of the same job process have no exclusive identifier (e.g., a hiring process code) and subsequent postings rarely mention the date that the *Edital* (job announcement) was published. Taken together, these issues underscore the limitations of relying on any text-selection method based on existing content structure to automate steps 1 through 4.

Given that one could identify and link the precise text domain of all stages of a hiring process, extracting data from the raw text presents an even bigger challenge. There is no predetermined layout or set of rules instructing how postings in the *Diários* should display information. Some postings may present candidate results in tables, others in continuous text; scores may be organized by exam type or committee member, or a combination of both; exam types are sometimes informed near candidates and scores and other times at the beginning of the journal posting. While there is certainly some commonality across official postings, after all, these have legal content and enforcement and are often submitted by specialized bureaucrats on behalf of the employer, these similarities are subtle and offer little aid to scrape-like tools that rely on well-defined patterns.[16]

## 3.2 A New Approach to Transform Unstructured Text Into Data

To address all of the challenges, I develop a two-step natural language processing algorithm that allows me to first define the relevant text portions from highly confounding text, attribute a posting to a unique job hiring process and link all different postings related to the process, and finally transform unstructured text into data. This algorithm generalizes a search query with learning and can be applied to a wide variety of empirical settings that follow the same general structure of this paper's data. Here, despite differences in layout and the manner in which information is displayed in the text, all relevant text belong to the same set of temporally ordered documents (i.e., government legal gazettes published daily).

The two steps are broadly defined as follows. First, deciding on how to define the textual matching attributes to link text snippets with highly confounding information and no connecting identifiers. The second step transforms unstructured text into structured data, instead of using the underlying text as a signal of a latent process, which is the goal of most applications in the literature.

**Motivating the approach.** While all steps of hiring processes in the Brazilian public sector are carefully documented and publicly available, there are two major challenges to systematically using these raw data sources. The first is that published notices within the same hiring process are not directly linked. In practice, it is non-trivial to assign a list of candidate scores posted in a certain journal issue to a previously-published job announcement information. Off-the-shelf text analysis tools that connect text bodies based on proportionality and similarity like the term frequency-inverse document frequency (tf-idf) and cosine similarity are not useful in this

[16] See Figure 2 for some examples. I document over 200 different text layouts, with multiple variations within the same broad layout type.

context since information in legal publications is highly confounding. The same page of an official gazette might contain sections with a hiring round of eye surgeons at a certain hospital and a section with another job selection process of brain surgeons at the same hospital. In other cases, the same hospital might be hiring eye surgeons through more than one public notice.

Standard text analysis algorithms that are increasingly popular in economics are poor tools for connecting different text corpus based off *exact* text vectors. Even the sophisticated lexical fingerprinting tools used to detect plagiarism would still rely on the resemblance between text documents that might not be informative for linking purposes. These algorithms require calibration that is context-specific, demanding supervision in a large number of cases, drastically decreasing gains to automation and resulting in a large number of type I and II errors.

Conceptually, the problem boils down to connecting a number of $T$ text snippets by matching on $N$ text attributes. Both $T$ and $N$ are ex-ante unknown. A job selection process might have any number $T$ of published texts and it is unclear which and how many $N$ lexical structures one might need to properly connect such announcements.

**Defining Textual Matching Attributes.** How should $N$ be chosen? Consider that a sequence of $t = 1, ..., T$ connected text documents can be summarized by the set of attributes $A^t$:

$$A^t = \{\text{message keyword, sender, release date, message keyword feature}\}$$

In the case of a specific hiring process, these attributes take the correspondence {job, employer, release date, job feature}, where job feature might refer to the place of work, position title, or any dimension that distinguishes $A^t$ from $A^j$ given $A^t \backslash \{\text{job attribute}\} = A^j \backslash \{\text{job attribute}\}$, $t \neq j$. The motivation for defining $A^i$ stems from its search-query use. For each government gazette issue, I search for a job posting notice, using a combination of words in the same paragraph (formally defined as some text string neighborhood) comprised of "announcement", "job", "hiring", and "posting". When there is one or more hits, I bound the relevant text to each job announcement and extract attributes $A$ (the implementation of relevant text boundaries is detailed below). Only the *release date* is ex-ante known, since I know when each journal issue is published. To correctly identify the terms containing the other attributes in $A$, I rely on ad hoc dictionaries and allow them to expand by "learning" new terms.

More precisely, I construct a list with all public entities from government webpages and a dictionary of occupations that provide a fairly broad library to search for full or partial matches in job announcement texts. After I identify a job (message keyword) and employer (sender) pair, I update these dictionaries used in the search query for the same keyword. For example,

my initial occupation library contains "Professor" and adds terms like "Assistant Professor", "Associate Professor", and so on as I progressively incorporate richer versions of the message keyword "Professor". After building the set of attributes that uniquely identifies a job hiring process, I search in all documents published after the release date for occurrences of $A^t$. The collection of $T$ text excerpts containing $A^t$ thus comprises all published notices of the job selection process.

Note that while still relying on some dictionaries to discipline the domain of the message keyword and sender types, this approach takes an agnostic view with respect to the information derived from the underlying text contents and its potential use to connect text snippets, as well as the need for computationally-intensive updating of the initial search libraries. Indeed, in most applications, researchers may not even need to update their initial search parameters.

Suppose a researcher wants to use the New York Times online archives to collect data on murder rates in major US cities since 1890. In this case, the message keyword could be "murder rate", a list with the desired city names would inform different values for the sender, the release date is the issue's date, and the message keyword feature could be a year matching the release date. Instead of going through multiple manual searches in the archived texts for each combination of city and year, the results to the approach above would give the relevant text snippets for the next stage: transforming the text into data.

**Transforming Unstructured Text into Structured Data.** After linking hiring rounds across government gazette issues, the next challenge to leveraging the richness of the Brazilian public sector hiring information is the lack of structure in the published notices. Hiring rounds might be displayed in tables of varying dimensions, in free text, or in a combination of both. In most text analysis applications as in Atalay et al. (2020), every text snippet has a fairly similar structure, which greatly facilitates mining.

In addition, even in cases with free text as in Bybee et al. (2020), the underlying text structure is relevant only to the extent that it conveys information to identify a predictor based on the message content. That is, researchers map text (raw or represented by a numerical array) onto a discrete set of measures $\mathcal{T} \rightarrow \{M_1(\tau), M_2(\tau), \cdots, M_K(\tau)\}$, where $\tau$ is a transformation of the underlying raw text. Such mappings include sentiment-based approaches as in Gentzkow et al. (2019), where the true sentiment of a message is transformed into a function of a latent quantity.

In many applications, however, researchers might be interested in extracting exact information from text and converting that into a database by distilling $\mathcal{T}$ into a pre-determined list of variables $\{x_1, x_2|x_1, \cdots, x_K|x_1\}$. This is usually an extremely time-intensive task, highly dependent on the particular context that relies heavily on strong prior information about the

potential variations of text structure across $\mathcal{T}$. Often times the implementation of an automated tool to extract data in these cases is so burdensome that researchers end up hand-collecting the desired variables from a feasible subsample of text documents.

It is possible to simplify and create a generalizable procedure by taking into account the relation between several of the desired variables and one fixed variable, which I denote by $x_1$. Let $x_1^i$ correspond to candidate $i$'s exam score, $x_2^i$ her name, $x_3^i$ the exam type, $x_4^i$ the committee member who gave score $x_1^i$ and so forth. In order to deal with the unstructured nature of the text, I start by targeting text tokens containing numbers. Of course, many numbers within the text might be extraneous and not represent scores. The next step searches for tokens in the neighborhood of every number that match the characteristics of each additional variable $x$. This both fully defines the other variables that relate to $x_1$ and filters out numeric elements that are not scores. For instance, numbers without recognizable names in their vicinity are discarded. Further, the same candidate might have several scores for different exams, which will differ along some dimension (Exam I and Exam 2, Written Exam and Oral Exam, etc.). This attribute will be relevant not only for individual $i$'s score, but also for all other candidates who took the same exam type. Thus, it must be that the relation between $x_1^i$ and $x_3^i$ holds for all $i \neq g$.[17]

By choosing one variable to which most or many of the other desired variables relate, I leverage the semantic structure in language that differs across public announcements, but that organizes each candidate's relevant information in the same way within a job notice text. The underlying semantic structure thereby informs the selection model about the location of certain variables rather than feed a label grouping, such as political slant or favorability of a review. This step requires the use of few *ad hoc* dictionaries (a list with Brazilian names in the current application and another with different examination types), which are allowed to learn similarly to before with the lists of occupations and employer names.

Returning to the application example of historical murder rates in major US cities, after defining the relevant NYT articles containing murder rates of a city in a given year (step 1), now the researcher implements step 2 to extract the actual number from the text ($x_1$), which is the murder rate. The process here is simple since *i)* the murder rate number only has two relevant attributes — city and period or year. Of course, numeric values of $x_1$ may give different scales or measurements of murder figures, for which the researcher will need to implement some form of ex-post harmonization.

[17] For example, if the data is organized in a table where a certain column contains each exam type and rows display candidate names and scores, each candidate's score in a given exam will be aligned with the column's name. Another example: if the beginning of recorded scores displays a legend that gives an ordering such as "*Name - ID # - Written Exam - Interview - Final Score - Rank*", every candidate will have scores displayed in the same order.

The procedure above retrieves immense amounts of text snippets and data from the raw PDF files. There are over 900,000 unique texts identified by my matching attribute search $A^t$, of which about 110,000 were unique job processes. From these, I successfully link processes with enough information to match on and that start and end (some processes are cancelled or interrupted due to candidates' legal actions). Some job processes publish the same post more than one time to give enough visibility to the public, which I further filter out. At the end, I identify 89,000 unique job processes from 1970 to 2020.

# 4 Impact of Increasing Hiring Impartiality on Gender Equity

This section introduces my first set of results, focusing on how greater impartiality in hiring practices impacted hiring odds and application behavior of male and female candidates. I begin by discussing a 1988 reform in Brazil's Federal Constitution that introduced an impersonality requirement in public sector hiring as the main source of variation to the mix of hiring methods used by employers. The impartiality requirement was immediately adopted at the federal government level, but states only began passing the legal framework to equate their public servant selection processes to the new federal norms years later.

By comparing hiring processes for the same occupation at the federal and state levels, I can analyze how the hiring impartiality requirement affected candidates along the following dimensions: scores for different screening stages, gender hiring gap, and applicant behavior, distinguishing both supply and demand channels. To study the first-order reduced-form effects from the policy, I consider the treatment to be binary, in the sense that federal government employers had to promote changes to their mix of tools by replacing some tests and features with others. Later in the paper I consider all different treatment types induced by the policy.

## 4.1 The Impartiality Reform: Description

In October 1988, Brazil passed a new Federal Constitution in the wake of the end of several decades under military regimes. Policymakers sought an overhaul of civic and legal legislation previously enacted during dictatorship. The new Constitution also modified its provisions instructing how the selection of public servants via *Concurso* should occur. The new text kept all requirements introduced by the previous Constitution in the 1960s, which mandated that "*Public sector positions are accessible to all Brazilians* [...] *and hiring must be conducted through formal process (concurso) using exams or exams and candidate qualifications*" (1967 Constitution of Brazil, Section 7, Article 95), that is, meritocratic hiring. In addition, it added the following

amendment: "*hiring must obey the principles of legality, impersonality, morality, transparency, and efficiency*" (1988 Constitution of Brazil, Section 3, Ch. 7, Section 1, Article 37).

These principles are poorly-defined legal terms not explicitly laid out in the Constitution's text, although Brazilian jurisprudence at the time already offered interpretations for *legality* — following the letter of the law by not adopting practices explicitly stated as illegal — *efficiency*, which meant that in order to begin a *Concurso* there should be a clear need for the hire and that the screening cost should be adequate, and *transparency*, which made it official that both job postings, screening stages, and results should be made public, a practice already in place for decades. Note that these requirements introduced by the 1988 Constitution are either maintaining previous practices or of little consequence to the screening process. With respect to *morality*, the principle has been broadly interpreted by courts and legal analysts to make it illegal for candidates or evaluators to display unethical or disloyal behavior, such as cheating on screening tests, another practice previously deemed illegal according to job announcement rules.

The most important principle in the 1988 Constitution, *impersonality*, disallowed any practices in public servant hiring that would allow a specific candidate, or someone from a specific identifiable group, to gain improper advantage. In the case of written exams or multiple-choice tests, identifying a candidate's name would be a clear violation of the rule, resulting in the blinding of these exams. However, determining how to appropriately handle other screening tools was less straightforward.

Despite the apparent contradiction between conducting interviews and having a hiring process that is impersonal, non-written tests that allowed evaluators to observe and interact with candidates continued to be used in several occupations. Policymakers and government lawyers considered that some common practices were important screening tools for several public servant careers, and that as long as their use was combined with purely impartial tools, such as blind tests, they could still be used as long as they observed the other principles (e.g., interviews had to be open to the public and not closed-door). For example, it was common practice to perform oral exams in the judicial system, a practice that remained after 1988. Nonetheless, as I show later, on average, federal sector employers decreased their reliance on non-written stages, either by reducing their relative number with respect to blind practices or by removing them completely.

In principle, the provisions in the new Constitution applied to public sector hiring at all government levels. However, because public servant selection is conducted by states and municipalities independently of the central authority, states had to pass the appropriate legal frameworks to comply with the new federal government rules. Compliance could be enforced either by passing specific public sector legislation or by passing a new Constitution, similar to

the federal government's decision in 1988. In reality, the same reason that prompted Brazil's federal government to pass a new Constitution — the exit from a military regime and return to democracy — also imposed the need on other federation entities to also introduce their own updated constitutions. As a result, it took several years for the sharp shift in federal employer behavior with respect to hiring to trickle down to state agencies and governments.[18]

Among many other changes, the 1988 Constitution re-organized political constituencies, reinstated popular vote for the executive branch, and ended media censorship that was instated during military regime. The Constitution also expanded the bill of rights and public services, most of which took several years before being offered to the population. Although these changes affected civil society and the political landscape, the only changes to public sector hiring were the new principles that I discussed.

## 4.2 Sample Selection and Data Patterns

For the analysis centering on the impartiality reform policy, I restrict my estimating sample to the years 1986 through 1991 to the federal government level and to the states with official gazette issues available online for the period. These states were Amazonas in the country's north region, Pernambuco in the northeast, Distrito Federal, Mato Grosso, and Mato Grosso do Sul in the central region, São Paulo — the largest and richest state — in the southeast, and Rio Grande do Sul in the south. I use all job processes with complete information on job requirements, screening steps, as well as candidate scores, final ranks, and job offers, if any.

I focus the analysis on the 1986-1991 period since states began jointly passing new state-level Constitutions with similar guidelines to the Federal rules at the end of 1990. In the case of states, however, the enforcement of impartiality rules was much less organized, with some state employers changing hiring methods in the 1990s and others still hiring solely based on interviews, for example. Figure 3 shows the gender distribution of applicants by occupation and skill level.

## 4.3 First Stage: Did the Reform Change Screening Practices?

Due to the nature of the shock to hiring practices that I study, it is crucial to begin the main empirical analysis by evaluating the extent to which the introduction of the impartiality re-

[18]It was common to observe states hiring for several occupations only using interviews (which complied with the previous constitution requirements as these were personality and character "exams", while out of thousands of job processes at the federal level post policy, I found no occurrences of hiring based solely on interviews. More importantly, it was common to find lengthy discussion pieces in federal gazettes on how federal agencies were adjusting their hiring processes and other practices to comply with the impartiality and other guidelines in the Constitution.

quirement led to a reaction from federal employers relative to untreated hiring processes. I test for a series of different take-up or compliance measures in federal jobs relative to states by estimating regressions of the form:

$$y_{ct} = \delta_{o(c)} + \alpha \text{Post}_{o(c),t} + \gamma_t + u_{ct} \tag{1}$$

where outcomes $y_{ct}$ for a job process $c$ of occupation $o$ are regressed the on variable of interest, $\text{Post}_{o(c),t}$, which takes the value of one if the job process is conducted by the federal government after 1988. Comparing similar occupations between treated and control groups is important to net out composition differences between aggregate jobs posted at different government levels. In Brazil, healthcare services are usually provisioned at the local and state levels, while bureaucracy tends to be concentrated in the federal sector (e.g., tax compliance offices).

Table 1 shows "first-stage" results given by equation (1). Columns (1) and (2) test how likely treated job processes are of having at least one written round after the policy (which then become blind exams) relative to state job processes used as control. To gauge the importance of the composition effects, the first column only uses year fixed effects and compares all occupations, with a precisely-estimated coefficient of zero. After controlling for occupation in column (2), the coefficient becomes large and statistically significant, indicating that treated jobs become 25 percentage points more likely to have at least one written stage as part of the screening process.

Columns (3) through (4) reflect similar exercises, but now testing whether the impartiality reform induced treated employers to reduce the probability of having at least one *non*-written exam. Conditional on occupation, column (4) shows a negative but imprecisely estimated effect. Finally, column (5) finds that treated job processes were 48 percentage points more likely to use a unique screening tool, comprised by a written (blind) exam, and column (6) shows a 25 percentage-point decrease in the probability that a job process uses only non-written screening methods.

First-stage estimates indicate sharp changes in the mix of screening tools used in federal sector hiring processes relative to those in the control group. Although I discuss in detail the different treatment groups giving rise to each estimated effect in Table 1 later in the paper, it is useful to shed some light on what underlying responses each of these estimates capture. For example, maintaining all rounds as non-written in a federal job process would be a direct violation to the principle of impersonality (column (6)). Similarly, to increase impartiality, employers previously using a mix of written and non-written tools might remove subjective stages and blind the written stage (columns (4) and (5)). Instead of removing non-written stages, employers could add a written blind round (column (1)).

These different combinations of changes in screening methods toward more impartiality contribute to non-perfect compliance rates in each individual regression. Taken together, these estimated effects all represent policy-compliant changes, and, as I later show are largely determined by occupation.[19]

## 4.4 Binary Difference-in-Differences Design

My empirical strategy exploits the immediate compliance with the introduction of impartiality in public servant selection by federal government employers, together with a lagged and slow adoption by state-level employers. To assess the effects of the policy on gender gaps in several labor market outcomes:

$$y_{it} = \delta_{o(i)} + \beta \left( \text{Post}_{o(i),t} \times \text{Female}_i \right) + \gamma_t + u_{it} \tag{2}$$

where $y_{it}$ represents candidate $i$'s job process outcomes, $\text{Post}_{o(i),t} \times \text{Female}_i$ measures the differential effect of greater hiring impartiality on women relative to men, while controlling for year and job announcement's occupation fixed effects. In all specifications, standard errors are clustered at the job process level. I only consider job processes with at least one male and one female applicant, with known job offers, and that consistently appear before and after the policy in both groups. I assign candidates' gender using Brazil's Census Bureau Gender of Names database, which contains nearly 200,000 unique first names and their corresponding gender. The match precision is above 98%.

## 4.5 Effect on Candidate Scores

An advantage of having Brazil's public sector as a setting is that, in addition to observing job offers to candidates, I have detailed performance scores from each screening stage. Assuming more impartiality is women-favoring, depending on the magnitude of the effect, women's final scores may increase and yet hiring gaps remain unchanged if the marginally not hired female candidate was too far behind the marginal hired man in measured performance. Therefore, with a less coarse outcome such as scores, more subtle effects of the policy can be measured.

Before using final scores, however, I check whether they actually determine job offers. Figure 5 compares hiring odds across the distribution of final score results within a job process (i.e., the final ranking of candidates determined by sorting highest to lowest final scores). Only

[19] Figure A.4 shows an enforcement example of blind exams in a selection process for federal judges published on September 4, 1989 in the job announcement rules (*Edital*). The rule states that candidates identifying themselves in any exam (written or multiple-choice) will be excluded from the hiring process.

candidates in the highest score decile in each job process have a non-zero probability of being hired, with top scorers having about a 60% chance of receiving an offer. This is not surprising — according to the rules, hiring decisions are made exclusively in accordance with the ordering of candidates' final results, and even top scorers are not guaranteed job offers since job openings are generally fixed.

Table 2 begins by comparing final scores in the hiring process received by female and male candidates. Scores are standardized within each hiring committee, so that they are comparable across different job processes. The final scores of women increase by 0.07 standard deviations after the policy, with the final scores of men decrease by slightly more. Combined, these effects imply a 0.14 standard deviation narrowing of the gender score gap. These separate effects by treatment and control are shown in Figure 4, where final score gender gaps remain unchanged for candidates in state job processes and the gap significantly narrows for federal jobs. Figure 6 displays dynamic effect versions of the pooled estimate in column (3), first comparing the evolution of the evaluation score gender gap in federal and state hiring processes and then plotting the difference-in-differences estimate of the two series.

To lend further credibility to the change in final scores as a consequence of the reform, note that depending on the mix of screening methods used in each job process, the final score is determined by some weighted average of these tools. As Table 1 shows, albeit occupations complied with the impartiality requirement in different ways, one would expect the increase in the final score to be driven on the intensive margin by an increase in the score of written exams of women relative to men. As I show in my conceptual framework in Section 5, this expected increase in relative score is attributed to the elimination of evaluator bias, or disparate treatment, after blinding written exams.

Column (4) of Table 2 shows that the written scores of men decrease by about 0.10 standard deviations, contributing to an overall increase in women's written scores once exams are blinded relative to men of 0.13, almost the entire magnitude of the improvement in final scores. One interpretation of the decrease in men's scores is that prior to concealing candidates' identity in written exams, men were being over-scored. Next, absent substitution effects — i.e., evaluators strategically adjusting scores in non-written exams as a response to the addition of blind written tests — changes in relative scores of non-written should be close to zero or at least small in magnitude. This is confirmed in columns (7) through (9).

## 4.6 Effect on Hiring

Having determined that the impartiality reform increased women's final scores relative to men, combined with the fact from Figure 5 that final scores determine hiring offers, my next analy-

sis answers whether the performance improvement was sufficient to increase women's hiring rates. I run regression (2) with a dummy for whether the candidate received a job offer as the outcome. Recall that these job offers represent the official conclusion of the *Concurso*, in which part of the candidate pool that ranks above a final score threshold (when it exists) is considered "adept", that is, could legally be hired, and the number of top candidates matching the number of job openings is offered the job offer, known as *convocação*. When candidates decline or cannot accept the job offer (e.g., because of death), the next enabled candidate outside the initial list receives the offer.

Columns (1) and (2) in Table 3 show that the probability of being hired for women and men, respectively, go in opposite directions after the impartiality policy takes effect. Women become 0.3 percentage points more likely to be hired and men's hiring rates decrease by 0.4 p.p. Interpreting these coefficients in light of the variation used in the empirical strategy, consider the following example. A woman (man) applying to an accountant job in the federal government is more (less) likely to be hired after the policy compared to a woman (man) who applied to another accountant job in a state. Taken together, these hiring probability estimates imply a 0.7 percentage-point decrease in the gender hiring gap on average. Thus, the policy made women more competitive candidates because of higher final scores, and the improvement in performance was sufficient to result in higher hiring rates.

## 4.7 Gender Hiring Gap: Disentangling Supply and Demand

The gender hiring gap is determined by a sequence of decisions of both job seekers and employers. First, potential candidates decide whether to apply, and second, conditional on being an applicant, there is some probability of getting a job offer and being hired. Systematic differences at these stages between genders in turn determine the broader hiring gap. My previous estimates focused on the second factor, which is typically unobservable in other settings, since calculating the conditional hiring probability requires observing *all* applicants, not only the hired pool.

Knowledge of the applicant pool is important for a complementary reason. Employers and policymakers may also be interested in the initial individual decision of whether to apply to a job or not. Intuitively, drawing more candidates from a minority pool should increase the overall hiring rate of that group if qualified individuals refrain from applying. With respect to gender, previous studies have presented evidence on several fronts suggesting that women may be less likely to apply for promotions and less likely to enter tournaments than men due to a lower willingness to compete or self-stereotyping (Niederle and Vesterlund (2007)), Hospido et al. (2019), Bosquet et al. (2019), Coffman et al. (2021)), sort into female environments to avoid

competing against men (Gneezy et al. (2003)), or even being nudged to apply when job ads indicate a preference for diversity (Flory et al. (2021)).

Employers' use of biased screening tools is likely to interact with these factors, further magnifying barriers to extensive-margin responses of female candidates. Implementing more impartial screening may motivate more female candidates to apply, as even the perception of fairer treatment could be consequential in shaping minorities' behavior (Small and Pager (2020)).[20] Supply-side factors are important because suboptimal entry by high-performing women is costly to firms and without observing application rates, the effectiveness of enforcement of anti-discrimination laws cannot be fully assessed.

To understand this point more formally, let the share of women hired by an employer or from a job selection process be defined as $\mathbf{Pr}(\text{Female}|Hired = 1)$, in which researchers observe the pool of hired candidates and then calculate the makeup of female hires. Observing low hiring rates for women in this case masks two different effects: (i) the potential propensity of the employer or hiring committee to discriminate (under a set of assumptions), against women, and (ii) lower quality or fewer women applying for the job. Because researchers can usually only observe the rate at which women are hired, measured hiring rates are conditioned on an endogenous variable — the hiring decision based on the available candidate pool — and therefore cannot distinguish between employer and applicant behavior.

When policymakers enforcing gender discrimination laws rely on observed hiring rates, non-discriminatory employers may be inadvertently punished when observed gaps are driven by differential gender sorting across employers or other supply-side factors. Brazil's public sector hiring processes enable me to decompose the female hiring rate into two components: a demand channel, capturing differences in the odds of female candidates winning and a supply channel, which measures differences in application rates as:

$$\mathbf{Pr}(\text{Female}|Hired = 1) = \underbrace{\mathbf{Pr}(Hired|\text{Female} = 1)}_{\text{Demand}} \times \underbrace{\mathbf{Pr}(\text{Female} = 1)}_{\text{Supply}}$$

The demand component, $\mathbf{Pr}(Hired|\text{Female} = 1)$, which was estimated in column (1) of Table 3, indicates an employer-driven response improvement in female candidates being hired of 0.3 percentage points, which coupled with a similar-sized decrease in hiring odds to men represents a 0.7 percentage-point decrease in the overall hiring gap. Column (4) in Table 3 estimates the supply response of women applying to jobs after the impartiality policy ($\mathbf{Pr}(\text{Female} = 1)$). The estimate shows that women application rates grew by 1 percentage

[20] Women are more likely to place greater weight than men on fair treatment, and the perception of fair treatment is more strongly linked to womens than to men's willingness to apply at a previously rejecting firm (Brands and Fernandez-Mateo (2017)).

point. To benchmark these magnitudes, consider that the hiring gap for federal jobs pre-policy was about 1.6 percentage points (net of occupation effects), the drop in the hiring gap implied by the demand channel corresponds to about 44% of the pre-treatment level, while the supply effect measured as gender application gap amounts to around 62%.

To gain further insight into the general forces behind supply movements, in the results in Table 4, I run job process level versions of the binary DiD model, first confirming that the share of women hired grows after the impartiality requirement, as well as the share of female candidates in the applicant pool. Note that these specifications are equivalent to observing aggregate data on $\mathbf{Pr}(\text{Female}|\textit{Hired} = 1)$ in column (1) and $\mathbf{Pr}(\text{Female} = 1)$ in column (2). Moreover, note that latter effect — about a 6% growth in the female application rate — is statistically indistinguishable from the estimated in column (4) of Table 3, of around 7% of the pre-treatment level.

Next, column (3) shows that the number of applicants decreases after the policy — a movement that may be driven both by candidates' perception of an increase in the cost of the job process may be, for example because of more screening rounds, or based on the job openings, which positively correlate with candidate pool and are determined by budgetary and personnel management — but the reduction in the number of men applying is 10 percentage points larger than that for women, accounting for the increase in the probability of a candidate being female.

# 5 Conceptual Framework

This section sets up a theoretical framework to explore the impact of introducing and removing different screening practices on hiring rates. The framework builds on the canonical models of statistical discrimination by Phelps (1972), Aigner and Cain (1977), with important modifications by Autor and Scarborough (2008). I model managers and screening practices allowing for them to manifest several dimensions that employers may face in practice when designing selection processes.

The first ingredient in the model is hiring manager bias. Managers have the task of selecting employees with a mix of screening tools delegated to them by the employer. I allow managers to have a systematic bias for a certain demographic group. The term could be interpreted as taste-based discrimination, as it effectively captures a utility disamenity from hiring some group, as well as implicit or any other source of unintentional bias. However, this bias can only be expressed to the extent that the screening tool used enables discretion. The intuition is simple: expressing bias vis-à-vis an objective test is more costly than when evaluating a candidate in an interview because detection is easier. In contrast, statistical discrimination in

the model does not depend on the objectivity of screening practices used, but it does depend on whether the tool conceals the candidate identity. That is because managers base their prior of candidates' productivity on their group membership, resorting to the population mean instead when group membership cannot be assigned.

The second addition I make is to model the possibility of screening practices themselves to be biased. Independently of the behavior of a hiring manager, certain screening tools may disadvantage a particular group. For example, if written tests reward risky behavior by penalizing wrong answers without measuring productivity, women may be disadvantaged and the screening practice would lead to a disparate impact.[21] Finally, by maintaining screening precision in the model, the role of tool bias is equivalent to adding systematic noise to the productivity signal provided to managers, favoring less productive applicants of the favored group.

With these basic forces — manager bias, tool bias, and precision — interacting, my goal is to derive reduced-form predictions of gender hiring gaps for five types of changes in screening tools I empirically observe. In addition to being interesting in their own right, these cases will reveal the relative importance of tools and managers for gender equity.

## 5.1 Environment

An employer (the principal) delegates the screening of a pool of job applicants to some number of hiring managers or evaluators. The candidate pool comprises individuals from two demographic groups, $x = \{m, f\}$, corresponding to a minority and majority group, female and male, respectively. As usual, I use the term minority in its socio-economic dimension, so that for now the gender make-up of the candidate pool is unrestricted. The employer bases the hiring decision on some indicators of productivity $\theta = \{s, \eta\}$, observable only by hiring evaluators, which coarsely measure a candidate's true productivity level, $y$. The productivity of job candidates is distributed as:

$$Y \sim N\left(\mu_0\left(x\right), 1/h_0\right)$$

where the mean $\mu_0(x)$ is allowed to depend on group membership, and $h_0$ is assumed to be independent of $x$. Given that I consider women to be the minority group, women's average productivity is lower than men's, $\mu_0(f) < \mu_0(m)$.[22]

[21] Baldiga (2014) shows that women are more likely to skip than to guess on SAT questions that penalize a wrong answer, which decreases their test scores. Importantly, the pattern is not explained by gender differences in knowledge or confidence.

[22] Aigner and Cain (1977), Lundberg and Startz (1983), Cornell and Welch (1996), and Bartik and Nelson (2021) model signal precision depending on group membership. Similar to Autor and Scarborough (2008), I assume it to be independent of group membership to focus the analysis on the new features that I introduce in the model.

The employer's objective is to hire a proportion $K$ of workers that maximize expected productivity. But evaluators' objectives are imperfectly aligned with those of the firm. Evaluators care both about productivity and their bias toward a group, which must be jointly maximized when hiring job applicants by

$$u_j\left(y, \pi\left(x\right)\right) = y + (1 - c_\theta)\pi_j(x) \equiv y + d_\theta \pi_j(x)$$

where $\pi_j$ is evaluator $j$'s bias, $c_\theta$ is a cost function disciplined by the usual properties and defined over $c \in [0, 1]$. This component captures the cost that evaluators face by expressing bias, i.e., reporting to the employer a value of a candidate's measured performance that differs from the signal provided by the screening tool. Intuitively, this cost increases in the objectivity of the screening signal. Scoring a candidate's written test differently than the publicly-observable signal poses a much higher threat of detection than underscoring someone after an interview because the person did not appear to be friendly or an "appropriate fit".

The cost of expressing bias plays a central role in the model. The term connects an intrinsic property of a screening tool — which I call $d_\theta$ — to represent a screening practice's degree of discretion (or subjectivity), which loads on the bias term and determines its relative role in the manager's utility. Later, I impose additional structure on $c_\theta$ where the cost of behaving in biased ways will depend not only on how much discretion is granted to the manager by the tool, but also on the composition of the hiring committee along $x$.

To keep the notation tractable and match the model predictions to the empirical setting, consider the screening tool choices available to employers before and after the impartiality reform in Brazil's public sector. The full choice set and why the following are the relevant cases are discussed in Section 6. Before the reform, employers could use *i)* a written test, which generates a signal $s$; *ii)* a non-written test with signal $\eta$; or *iii)* a combination of both written and non-written tests.[23] After the policy, employers in the federal government are constrained to screen candidates using only a blind written test or a combination of non-written and blind written tools.

Before I begin to evaluate hiring rates under these different choices of screening practices, a final ingredient in the model is the ability of any screening tool to favor a group from $x$. This screening tool bias, which generates a disparate impact, mean-shifts a candidate's true productivity based on that individual's group membership.[24]

[23] Within the model, I do not distinguish whether employers use one or multiple tests of the same type. A richer formulation that would incorporate the supply of candidates could take into account the number of exams and therefore the length of the screening process as an application deterrent.

[24] I consider that the different screening tools provide signals of productivity determined by one factor, which is to say they measure the same skill. A two-factor model would reformulate the productivity as $y = y_1 + y_2$, where, in the spirit of Frankel (2021), $y_1$ could represent soft skills and $y_2$ hard skills.

### 5.1.1 *Before Policy: Hiring Rates With Written Exam*

When the hiring technology only includes written tests, the distribution of written signals, $s^* = y + \nu_s(x) + \varepsilon_s$, $\varepsilon_s \sim N(0, 1/h_s)$, is given by:

$$s^* \sim N\left(y + \nu_s\left(x\right), 1/h_s\right)$$

where $s$ represents the unbiased signal $s = y + \varepsilon_s$, $h_s$ is the inverse of the variance of the written signal, measuring the precision of written testing and independent of group membership $x$. $\nu_s(x)$ represents the disparate impact of the screening tool, which favors men when $\nu_s(m) > \nu_s(w)$. After observing $s^*$, the hiring manager updates her assessment of expected productivity of candidates, initially based on group productivity, $\mu_0(x)$, forming the posterior:

$$\mu(x, s) = s\frac{h_s}{h_0 + h_s} + \mu_0(x)\frac{h_0}{h_0 + h_s} + \nu_s(x).$$

The expression above represents a weighted average of perceived productivity of group $x$ and written signal provided by the written test, with weights determined by the relative precision of the signal with respect to productivity dispersion. A direct implication from the updated group mean $\mu(x, s)$ is that when written tests are less informative, hiring evaluatores rely more on her group prior.

The hiring decision that maximizes the evaluator's objective function satisfies the rule Hire $= I\{\mu(x, s) > k_s\}$, where $k_s$ is the threshold that yields a hiring rate of $K$. For the detailed solution to the hiring threshold, $z^*_\theta(x)$, as well as all the detailed solution for all cases below, please see Appendix B. Due to the linear form of the signal expression, the hiring threshold for group $x$ decreases when the group mean productivity is higher, the tool's bias favors the group, or when evaluators are biased toward $x$ (given the discretion in written exams).

### 5.1.2 *Before Policy: Hiring Rates With Non-Written Exam*

When the employer screens job applicants solely based on non-written tests, the intuition for the effect of evaluator bias, precision, and tool bias is similar to the case of written tests. However, an important distinction arises as a consequence of different subjectivity degrees between the two practices. Formally, let the distribution of non-written signals be

$\eta^* = y + \nu_\eta(x) + \varepsilon_\eta,\ \varepsilon \sim N(0, 1/h_\eta)$, where $\nu_\eta(x)$ represents the possible disparate impact of non-written tests and $\eta$ is the unbiased non-written signal, $\eta = y + \varepsilon_\eta$. Non-written exams allow discretion $d_\eta$ to evaluators. Given that interviews or oral exams are more subjective than written tests, the discretion given to managers is higher with non-written than written tests: $d_\eta > d_s$.

### *5.1.3 Before Policy: Hiring Rates With Written and Non-Written Exams*

The third pre-reform possibility of screening practices is a combination of written and non-written exams. Given the two signals previously described, $\eta^*$ and $s^*$, and the perceived group productivity, $\mu_0(x)$, the hiring manager updates her assessment of expected productivity taking into account both exam signals:

$$\mu(x, \eta^*, s^*) = s\frac{h_s}{h_T} + \eta\frac{h_\eta}{h_T} + \mu_0(x)\frac{h_0}{h_T} + \nu_s(x)\frac{h_s}{h_T} + \nu_\eta(x)\frac{h_0 + h_\eta}{h_T}$$

where the overall screening precision is $h_T \equiv h_0 + h_\eta + h_s$. With two screening tools, evaluators place less weight on their group priors, which favors the hiring threshold of the minority group if $\mu_0(m) > \mu_0(f)$. Moreover, the overall bias now captures bias from both tools.

I now evaluate how the relevant expressions in the three cases above change when blinding written exams.

### *5.1.4 After Policy: Hiring Rates With Blind Written Exam*

After the impartiality reform of 1988, federal employers using written exams as screening tools had to conceal candidates' identity. Within the model, blinding makes it impossible to assign individual candidates to a group, since hiring evaluators cannot observe whether a certain signal is generated by a male or female candidate. Let the blind written signal be defined as $b^* = y + \nu_s(x) + \varepsilon_s$, with $\varepsilon \sim N(0, 1/h_s)$. Note that the blind written exam contains the same screening precision $h_s$ and the same disparate impact $\nu_s(x)$ as the written test previously modeled.

When the screening technology includes blind written tests, the evaluator's objective becomes:

$$u_j\left(y, \pi\left(x\right)\right) = y + \underbrace{\left(1 - c_b\right)}_{=0}\pi_j(x) \equiv y + \underbrace{d_b}_{=0}\pi_j(x),$$

as discretion is entirely removed from the screening tool. Additionally, blinding the written test affects how the evaluator updates perceived candidate productivity, using the written signal, $s$, and the perceived *population* productivity, $\mu_0 = \frac{\mu_0(x)+\mu_0(y)}{2}$, since group membership is not identifiable:[25]

$$\mu(x, b^*) = s\frac{h_s}{h_0 + h_s} + \frac{\mu_0(x) + \mu_0(y)}{2}\frac{h_0}{h_0 + h_s} + \nu_s(x).$$

The hiring threshold for group $x$ determined by $b^*$ is similar to the expression obtained for written test screening, $s^*$, with an important distinction. While the signal given by the blind written test is just as informative as in the non-blind case, now evaluators update a group-neutral prior.

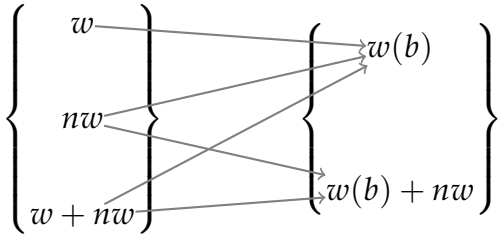
### *5.1.5 After Policy: Hiring Rates With Blind Written and Non-Written Exams*

Lastly, consider blinding a written exam when the screening process also includes a non-written test. This is similar to the previous case of combining screening signals from both exams, except for the blind written exam having no disparate treatment. However, evaluators still rely on group means and express bias in the overall posterior because of the non-written signal.

[25] For simplicity and without loss of generality, I assume that each group comprises half of the candidate pool. Another reason for using identical gender distributions is to keep application behavior from the pool of qualified workers outside the model.

## 5.2 Empirical Predictions

Each of the previous five combinations of screening methods, pre and post the impartiality reform, determines hiring rates for each group and thus the hiring gap. The screening thresholds, $z^*_\theta(x)$, and resulting hiring rates are in turn determined by functions of tool bias — disparate impact — screening precision, and evaluator bias, as governed by discretion. Formally, denote a written exam by $w$, non-written as $nw$, and written-blind $w(b)$, the reform induced employers to change screening tools in the following ways:




Before Policy After Policy

We are interested in two sets of predictions for each of the transitions above. First, how hiring rates for men and women and the hiring gap, determined by hiring thresholds, change. Second, the impact of the different changes in screening on the average productivity of hired employees. In general terms, bias of any kind in a job process reduces selectivity of the favored group (lowers hiring threshold). Removing or attenuating the expression of bias therefore raises the expected productivity of hired job applicants from that group. For that reason, in several of the cases discussed below, increases in women's hiring rates do not impose an equity-efficiency tradeoff, as aggregate employee productivity remains the same or increases. Detailed derivations can be found in Appendix B.

### *5.2.1 From Written to Blind-Written Exams ($w \longrightarrow w(b)$)*

Blinding a preexisting screening practice removes disparate treatment by those who conduct the screening. By concealing candidates' identity which eliminates discretion, evaluator bias ($\pi_j(x)$) cannot be expressed and therefore taste-based discrimination, as well as other bias stemming from decision makers, is eliminated, if they existed. More subtly, statistical discrimination is also removed since now candidates' identity and thereby group membership are concealed. Also note that in absolute terms, a screening process comprising of only written tools is already likely to allow for low degrees of evaluator bias expression, as these practices

offer less discretion due to being more objective. However, the bias of a written exam that is independent of the evaluator, $\nu_s(x)$, remains and can still have a disparate impact on the group that it disadvantages.

Written tests — whether blind or not — have the same screening precision and disparate impact, since they are otherwise identical save for hiding a candidate's identity. In this case, the only change to the three driving forces of hiring rates resulting from blinding involves removal of evaluator bias. Intuitively, if evaluators favor men, either through statistical discrimination or bias, blinding increasing hiring rates for women. Alternatively, if an evaluator is biased in favor of women, blinding the exam curbs the evaluator's ability to balance women's penalty from statistical discrimination with personal bias, potentially decreasing the female hiring rate.[26]

Because blinding the written test is bias-reducing, without other changes to the screening technology (i.e., screening precision and disparate impact), this change toward impartiality is productivity-enhancing. The intuition is as follows: because bias reduces selectivity of the favored group, it follows that removing bias raises selectivity for that group by raising the expected productivity of hires.

### *5.2.2 From Written, Non-Written to Blind-Written, Non-Written Exams* $(w + nw \longrightarrow w(b) + nw)$

Even though this change also blinds a preexisting written exam, here a candidate's identity is still known during the non-written screening stages, so that group membership information is used in the employer's posterior. As a consequence, only disparate treatment from the written exam is removed, while some statistical discrimination remains. If evaluators favor male candidates, blinding the written stage in a mix of non-written tools also increases women's hiring rate, but by less than in the $w \rightarrow w(b)$ transition.

Exams in Brazil's public sector job selection processes have pre-determined non-stochastic weights, for which I implicitly assume an equally-weighted screening tool mix. In practice, if blind written exams receive less weight by principals than prior to the policy, blinding may have limited or undetectable effects on the gender hiring gap. The same attenuation

[26] This implicitly assumes that an individual's measured performance (without bias) remains the same regardless of the conditions of the examination. While testing this assumption is difficult, the following exercise helps understand how it could be factored into the model. Suppose women actually perform better when a written test is blind relative to non-blind, perhaps because they feel less stereotype pressure. This would imply a different disparate impact for the blind exam — in this case, $\nu_{sb}(w) > \nu_s(x)$, which would only reinforce the effect of removing disparate treatment. A more subtle point here is how the possibility of differential performance affects whether one interprets what loads on the evaluator bias term as "discrimination". While this confounds the source of the issue — whether evaluator bias or blinding the exam — a broader view of discriminatory practices that also encompasses practices that unintentionally generate disparate impact would still prescribe blinding.

effect would happen if written exam weights were to remain the same on average before and after the policy, but contribute little to the final score.

Implications to average productivity of hired employees in this case are the same as in the $w \longrightarrow w(b)$ case. Blinding limits evaluator bias which raises selectivity, and in turn, raises the expected productivity of hired workers.

### *5.2.3 From Non-Written to Blind-Written Exams ($nw \longrightarrow w(b)$)*

I now analyze the potential change induced by the policy that most dramatically alters the mix of screening tools. To build intuition, consider an employer that solely relies on interviews to screen candidates. The disparate impact of interviews, their precision, and how much they enable evaluator bias to be expressed all determine an applicant's hiring odds. Only in terms of evaluator bias, under the assumption that interviews offer more discretion than written exams, this pre-policy state contains the highest expression of evaluator bias. In contrast, as discussed before, screening solely based on written exams is likely to provide a setting with low disparate treatment.

Because the two screening tools involve various different parameter values, I first make the following assumptions to focus on the effects of decreasing discretion and removing group-based priors. Let written and non-written signals have the same screening precision, $h_s = h_\eta$, and the same disparate impact, $\nu_s = \nu_\eta$. It follows that the gender hiring gap decreases with the blind-written signal relative to the non-written signal as long as evaluators favor men or, alternatively, if bias toward women is sufficiently small.

Relaxing the assumption of identical disparate impact implies that now both relative changes in evaluator and tool bias affect hiring rates. If evaluators are men-favoring and interviews have a larger disparate impact than written exams, moving from an interview to a blind-written test increases female hiring rates. While removing evaluator's majority group bias directly raises the selectivity of that group, any tool with less systematic bias helps the minority group.

Finally, we can relax the assumption that the screening precision of written and non-written exams are identical. If written tests have a higher precision, $h_s > h_\eta$, switching from non-written screening to written testing raises the hiring rate of the group with lower perceived productivity, observed the conditions discussed above. However, if interviews have higher precision, $h_s < h_\eta$, the transition from interviews to written test leads to higher hiring rates of the favored group, men.

In this case, the net effect on productivity depends on the direction of changes in evaluator and tool bias, and screening precision. Replacing an interview with a blind written test only imposes an equity-efficiency tradeoff if written tests either have much lower precision or impose a much higher disparate impact than interviews, since evaluator bias is eliminated by the screening practice change.

#### *5.2.4 From Non-Written to Blind-Written, Non-Written Exams* $(nw \longrightarrow w(b) + nw)$

This case maintains the use of non-written exams but, to comply with the impartiality policy, the employer adds a blind-written test to the hiring process. This addition increases screening precision which has a positive effect on female hiring since evaluators now place less weight on group means, as the additional signal increases the weight placed on individual performance in the hiring process. With better screening precision the gender hiring gap narrows even if women perform worse on the written test. Additionally, adding a screening tool without introducing evaluator bias (since $d_b = 0$) reduces the weight of the discretion in non-written stages in determining hiring decisions. However, introducing an additional screening method can generate additional disparate impact, $\Delta\nu_s$, due to the bias originiating from the practice itself regardless of evaluator bias. This can have a negative effect on hiring rates of women, depending on which group it favors and how it compares to the disparate impact that pre-existing non-written methods generate.

Adding a blind written test affects productivity through two channels: screening precision and selectivity. First, the rise in screening precision due to an additional productivity signal improves the accuracy of the manager's assessment of applicant productivity, which raises the expected productivity of hires from each group. Second, selectivity can either rise or decline depending on whether written tests are relatively more biased than non-written ones in terms of disparate impact. If the added written signal is less biased relative to the non-written, it increases selectivity for the group favored by the bias, raising quality of hires. However, if the written exam is bias-increasing, it increases excess hiring of the favored group which is productivity-reducing. Overall, the change does not impose an equity-efficiency trade-off, unless written tests are relatively more biased than interviews in terms of disparate impacts.

#### *5.2.5 From Written, Non-Written to Blind-Written* $(w + nw \longrightarrow w(b))$

Removing the non-written signal from a screening mix of written and non-written tests involves changes to all determinants of hiring rates. First, it decreases total screening precision. The loss in the number of productivity signals necessarily decreases the female hiring rate. Second, the removal of interviews also eliminates its disparate treatment in the job process, which increases women's hiring rates if evaluators favor men. Similarly, blinding the written exam removes evaluator's bias associated with the tool, which again decreases female selectivity. While now evaluators rely on the population productivity mean instead of group averages – favoring the minority group – the removal of a screening signal induces a greater weight placed on the population prior, helping women even further.

The third effect on hiring rates is determined by eliminating non-written exam bias from the job process. To understand whether women are favored along this channel, define $\mu_0(x) + \nu_s(x)$ as group $x$'s perceived productivity, so that if $\mu_0(f) + \nu_s(f) < \mu_0(m) + \nu_s(m)$ holds, men are the favored group when screening involves the written exam. First consider the case when written and non-written exams have the same disparate impact. In this case, removing the interview bias favors women if they have lower perceived productivity. Second, if the disparate impact between the two exams are different, then removing interviews increases (decreases) female hiring if at least one of the exams favors women (men).

The effect of removing the non-written screening practice on productivity of hired workers depends on the net effect from reduced screening precision and increased selectivity due to lower bias. One one hand, the decrease in total screening precision decreases the accuracy of assessment of applicant productivity and thus decreases the quality of hires from each group. On the other hand, this transition is bias-reducing by removing disparate treatment from both written and non-written, as well as bias of the non-written tool, which is a productivity-enhancing effect.

# 6 Impact of Changes in Screening Tools on Gender Equity

While so far I have studied the introduction of the impartiality requirement under the canonical, binary difference-in-differences research design, I can leverage the fact that the policy generated multiple treatments to gain further insight into how different screening tools change women's labor market outcomes. In the previous section, I showed that different combinations of screening tools capture various levels of precision, manager bias, and tool bias, and that depending on the compounded effect of changes in the mix of practices, gender hiring gaps may either diminish or increase.

In this section, I take these multiple types of screening tool combinations and changes to the data to analyze how five different changes in screening methods affected hiring rates.

I first formalize the treatment space generated by the policy, and the assumptions necessary for identification. I then estimate the effects of counterfactual changes in screening methods on final scores, hiring rates, and female participation in job processes. I conduct an analysis of the differential impact by the setting in which hiring processes would otherwise be conducted. Second, there are two possible ending points: (i) blind written, and (ii) a blind written and non-written combination. Altogether, this creates six possible treatments or transitions/changes in the screening methods used.

## 6.1 Treatment Space

I group examination types used in job selection processes into two broad categories: *written* and *non-written*. The *written* group encompasses both actual written and multiple-choice exams. Non-written exams include oral and practical examinations, and interviews. I omit resume analysis stages. Job processes may use any number of written or non-written exams, including combining screening tools from both groups, resulting in varying degrees of discretion. This broad grouping is useful as the impartiality reform should affect written exams by making their implementation blind and potentially curb the use of non-written exams, which could be considered intrinsically not impersonal.

In the previous section, I studied the effects on the gender hiring gap by five different changes induced by the impartiality policy. To understand why these are the empirically relevant cases in our context, I trace out in Figure 8 the potential treatment space for job processes under the following combinations of screening tools: written ($w$), non-written ($nw$), written and non-written ($w + nw$), blind written ($w(b)$), and blind written and non-written ($w(b) + nw$). Cases shaded in gray are ruled out by assumption in a sharp DiD design (perfect compliance). Subgroups of these options would be subject to the monotonicity and exclusion restriction assumptions in the standard IV case (e.g., Kline and Walters (2016), Feller et al. (2016)). Of the six remaining transition cases, $w \longrightarrow w(b) + nw$ accounts for less than 1% of transitions in the data.[27]

We are thus left with five possible treatments, capturing the following general changes in screening practices:

1. *Only Blinding (No Change in Screening Tools):* $w \longrightarrow w(b)$ and $w + nw \longrightarrow w(b) + nw$

2. *Blinding and Replacing Screening Tools:* $nw \longrightarrow w(b)$

[27] For this possible transition, on one hand, blinding the preexisting written exam represents a reduction in partiality. On the other hand, the introduction of the non-written screening tool increases the overall level of discretion in the mix, thus, increasing partiality. The overall change in partiality depends on the respective weights of the written and non-written stages.

**3**. *Blinding and Adding Screening Tools:* $nw \longrightarrow w(b) + nw$

**4**. *Blinding and Removing Screening Tools:* $w + nw \longrightarrow w(b)$

What does each of these treatments measure? Informed by the conceptual framework of the previous section, blinding or modifying screening tools implies changes in evaluator bias (disparate treatment), screening precision, and disparate impact from different tools. For $w \longrightarrow w(b)$ and $w + nw \longrightarrow w(b) + nw$, the only change to the design of the screening process is blinding the written exam, which completely eliminates discrimination associated with evaluators for $w \longrightarrow w(b)$. It also decreases disparate treatment in $w + nw \longrightarrow w(b) + nw$, but this effect may be modest toward a candidate's final score if the weight on the written exam is small. In both treatment types, screening precision and disparate impact remain the same, although the disparate impact in $w + nw \longrightarrow w(b) + nw$ combines the tool bias from both exam types.

Replacing a non-written exam with a blind-written test ($nw \longrightarrow w(b)$) involves the most dramatic number of changes to the forces determining hiring gap rates. First, disparate treatment of all sources is not only eliminated, but its absolute change could be sizable since one moves away from the tool with highest discretion to a setting with no discretion. Second, if both tools provide equally accurate productivity signals, screening precision does not change as a result of the transition, and therefore has no impact on the hiring gap. In contrast, if written tests have higher precision than interviews and female candidates have lower perceived productivity, the increase in screening precision helps women. Third, as long as the disparate treatment from interviews favored men more than the change in disparate impact from switching the tools, the hiring gap also decreases.

Adding a blind-written exam to an interview stage ($nw \longrightarrow w(b) + nw$) improves screening precision, which raises women's hiring rates if they are the group with less perceived productivity. Adding a blind exam does not introduce a disparate impact, and employers also reduce the reliance on evaluator bias in the interview stage since now the additional tool dilutes evaluation weight from the non-written tool. However, with a new tool, an additional disparate impact source is introduced, either increasing the potential group-favoring property of the non-written exam or attenuating it.

Finally, $w + nw \longrightarrow w(b)$ removes disparate treatment from the interview and non-written test, resulting in a hiring process free from evaluator bias other than the disparate impact from written tests, which remains the same. However, by eliminating the non-written stage, its disparate impact is also removed from the process and the total precision in the hiring tool mix decreases, which adversely impacts women (or the minority group more generally).

The exposition above reveals important sources of variation in the use of different combinations of screening tools — the strata in Figure 8 — induced by the policy's increase in hiring impartiality. Coupled with the reduced-form predictions in the previous section, this framework will inform the interpretation and unveil the forces driving the effects of changes in screening tools that I estimate next.

## 6.2 Assumptions and Identification

Let any treatment type $D$ be defined over the support $\mathbb{D} = \mathbb{D}_+ \cup \{0\}$, where job process $i$ receives treatment (dose) $D_i$, with potential outcomes in period $s = \{t-1, t\}$ given by $Y_{is}(d)$. Assume further no anticipation, so that $Y_{it-1} = Y_{it-1}(0)$ and $Y_{it} = Y_{it}(D_i)$. By relaxing the binary treatment assumption, i.e., $D = 0$ or $D = 1$, we allow for any dose or treatment level $d \in \mathbb{D}_+$. Using the results from Callaway et al. (2021), the standard parallel trends assumption is sufficient to identify the average effect of treatment $d$ among job processes experiencing the treatment with

$$\underbrace{\mathbb{E}[Y_t(d) - Y_t(0)|D = d]}_{ATT(d|d)} = \mathbb{E}[\Delta Y_t|D = d] - \mathbb{E}[\Delta Y_t|D = 0]$$

The possible treatment types induced by the impartiality reform are given by

$$\mathbf{g} = \left\{ \begin{array}{c} w \longrightarrow w(b) \\ w + nw \longrightarrow w(b) \\ w + nw \longrightarrow w(b) + nw \\ nw \longrightarrow w(b) \\ nw \longrightarrow w(b) + nw \end{array} \right\},$$

For each $g$ I estimate the following versions of the baseline DiD model,

$$y_{git} = \delta_{o(g,i)} + \beta_g \left( \text{Post}_{o(g,i),t} \times \text{Female}_i \right) + \gamma_t + u_{git} \tag{3}$$

comparing outcomes ($y_{git}$) for female candidates relative to men ($\text{Female}_i$), participating in job processes for the same occupation ($\delta_{o(g,i)}$) that had screening practices changed ($g$) in federal jobs but not in state-level processes $\left(\text{Post}_{o(g,i),t}\right)$.

The following example illustrates the variation used to identify $\widehat{\beta}_{g=w \longrightarrow w(b)}$. The regression in (3) compares a job selection process for, say secretaries, in the federal government that used a written exam before the policy to screen candidates, to another process selecting secre-

taries to state governments also only using a written test. Under the previous DiD assumptions, the parameter of interest in the example measures the causal effect of blinding the written exam in the selection of secretaries for federal jobs, using the fact that state-level job processes continued to use a non-blind written exam. The interpretation of $\widehat{\beta}_{b=w\longrightarrow w(b)}$ is informed by the reduced-form predictions from Section 5: the effect represents the change in the outcome due to eliminating discrimination of any source in the hiring process.

As in the binary DiD in Section 4, it is important to include occupation fixed effects in order to net out composition effects from supply fluctuations in public sector postings, whether determined by political cycles, economic growth, personnel retirements, etc. However, when taking into account the multi-valued nature of treatment, comparing job processes with a given treatment type within the same occupation becomes even more important.

However, when conditioning on occupations, there is almost no variation in treatment types. That is, job processes for doctors used to employ a mix of written and non-written exams before the policy and almost every *Concurso* for medical doctors after the policy in the federal government uses a combination of blind-written and non-written exams, so that within the occupation, 92% of job processes are treated by $w + nw \longrightarrow w(b) + nw$. While there are no technical reasons for this robust pattern in the data, customary norms and centralized decisions in the federal government may explain why different employers hiring for the same occupation both employ similar screening methods and respond to the policy by adopting similar changes to the screening mix.

The strategy above identifies effects off changes in screening methods given each treatment type — it measures how increasing or decreasing levels of disparate treatment, tool bias, and screening precision given each screening mix changes the outcome. Following Callaway et al. (2021), to identify non-local treatment effects, $ATE(d) = \mathbb{E}[Y_t(d) - Y_t(0)]$, a stronger version of parallel trends is necessary. The assumption involves not only untreated potential outcomes, but also all potential outcomes under all the different treatments. That is, for all treatments $d$, the change in outcomes over time across all units if they had been assigned that treatment is the same as the change for all units that experienced that dose:[28]

$$\mathbb{E}[Y_t(d) - Y_{t-1}(0)] = \mathbb{E}[Y_t(d) - Y_{t-1}(0)|D = d]$$

In my setting, most comparisons between estimated effects across $\mathbf{g}$ are not informative or even ill-determined (e.g., there is no plausible "counterfactual" to compare $w + nw \longrightarrow w(b) + nw$ to $nw \longrightarrow w(b)$ because the two treatments have different starting points). How-

[28]The assumption is still weaker than assuming that all treatment groups would have experienced the same path of outcomes if they were assigned the same dose, which would imply that $ATE(d) = ATT(d|d)$. In contrast, the strong parallel trends assumption allows for some selection into a particular treatment.

ever, in some cases they might be useful. For example, had treatment $w + nw \longrightarrow w(b)$ been $w + nw \longrightarrow w(b) + nw$ instead, would there be a different effect on the gender hiring gap? This comparison, expressed as $ATT(d|d) - ATT(d'|d') = (ATT(d|d) - ATT(d'|d)) + (ATT(d'|d) - ATT(d'|d'))$, requires the strong parallel trend assumption to warrant causal interpretation, otherwise the selection bias of some occupations removing the non-written exam and others keeping it implies $ATE(d) - ATE(d') \neq \mathbb{E}[Y_t(d) - Y_t(d')]$. In those pairwise comparisons, while I cannot test directly for strong parallel trends, I am assuming the following set of assumptions, shown by Callaway et al. (2021) to be equivalent to assuming strong parallel trends:

$$\begin{aligned}
\mathbb{E}\left[Y_t(0) - Y_{t-1}(0)\right] &= \mathbb{E}\left[Y_t(0) - Y_{t-1}(0)|D = 0\right] \\
\mathbb{E}\left[Y_t(d) - Y_{t-1}(0)\right] &= \mathbb{E}\left[Y_t(d) - Y_{t-1}(0)|D = d\right] \\
\mathbb{E}\left[Y_t(d') - Y_{t-1}(0)\right] &= \mathbb{E}\left[Y_t(d') - Y_{t-1}(0)|D = d'\right]
\end{aligned}$$

Violations to the first assumption can be tested by conducting a standard parallel trends "inspection", where each pairwise comparison from $\mathbf{g}$ should have pre-trends with similar behavior (note that these comparisons have the same pre-treatment mix of screening tools).[29]

## 6.3 Estimation and Results

I now proceed to estimate model (3) in five separate regressions for gender final score gaps and gender hiring gaps. Figure 9 shows treatment effects of final scores. For conciseness, I center my discussion in Figure 11, which conducts the same analysis using the gender hiring gap as outcome. Since job offers are solely based on final scores and job openings, any improvement in women's hiring rates relative to men's implies a decrease in the gender final score gap.

Figure 11 analyzes in three groups the five treatment types induced by the policy. Each group has the same baseline or pre-policy screening tool mix — $w$, $nw$, or $w + nw$ — for which I then estimate treatment effects depending on each complier type. To benchmark the following coefficient magnitudes, the initial hiring gap in the federal sector for each case is 1.5%, 17%, and a slight gender hiring advantage in the $w + nw$ case of 0.5% (although the sample average

[29] If the standard parallel trends assumption holds, then

$$\mathbb{E}\left[Y_t(0) - Y_{t-1}(0)|D = 0\right] = \mathbb{E}\left[Y_t(0) - Y_{t-1}(0)|D = d\right] = \mathbb{E}\left[Y_t(0) - Y_{t-1}(0)|D = d'\right]$$

and thus the first condition of strong parallel assumptions follows, since it is equivalent to the decomposition

$$\mathbb{E}\left[\Delta Y_t(0)|D = 0\right] = \mathbb{E}\left[\Delta Y_t(0)|D = d\right] \frac{P(D = d)}{P(D = d) + P(D = d')} + \mathbb{E}\left[\Delta Y_t(0)|D = d'\right] \frac{P(D = d')}{P(D = d) + P(D = d')}.$$

statistic is non-significant). Note that, at least observationally, the hiring gap is much larger in job processes relying solely on non-written stages.

Starting with how the gender hiring gap changed when job processes within the same occupation switched from a written test to a blind-written, the estimated decrease in the gender gap of 0.5 percentage point cleanly measures disparate treatment, or the impact of complete removal of all evaluator bias sources, for a given level of disparate impact and screening precision of written tests. The pre-policy use of written tests provided the smallest (significant) effect on gender hiring gap, likely due to the low discretion from the screening type. The estimated impact corresponds to a decrease in the gender gap of about 33%.[30]

Next, I analyze how two different treatments to a screening strategy using non-written interviews affected the gender gap. In this case, because interviews are are high-discretion tools and leave employers susceptible to bias, they may be interested in removing or replacing non-written stages. However, they may also recognize that interviews could have higher screening precision if managers are better informed than they are biased. Carefully weighing of these considerations is important to ensure a hiring system that is both more equitable and selects the most productive candidates.

Under the assumptions stated before, the two treatment types provide counterfactuals in a similar sense as Mountjoy (2021) in the IV case. When job processes switch from $nw$ to a written-blind, the gender hiring gap decreases by almost 7 percentage points. When benchmarked against the initial gap level, the estimated magnitude implies a decrease in the gender gap of 41%, a larger relative response than $w \longrightarrow w(b)$. In light of the model in the previous section, this treatment type involves the most dramatic changes to all forces determining hiring rates. If evaluators favor men (which is the case in the first estimated effect), then the pure disparate treatment channel will help women. Because the net result from changes in screening precision and tool bias may depress female's hiring rates, the large estimated result suggests that either written-exams have higher precision or smaller disparate impact than non-written, or that the combined magnitude of these channels is small relative to the size of disparate treatment in interviews.

The next treatment type and alternative "counterfactual" to the previous case involves adding a blind-written exam to a pre-existing non-written stage. This is an interesting case in light of growing criticism over requiring standardized or written tests that could disadvantage women or minorities (introduce disparate impact). My estimates suggest that the potential negative effects from these evaluation methods is more than compensated by gains in screening

[30] Under the additional assumption that the contents of written tests are exactly the same before and after the policy (I find no evidence to the contrary), blinding the test measures discrimination, particularly, in terms of disparate treatment.

precision, which helps the minority group. The estimated effect is about 5.9 percentage points, or 35% of the initial hiring gap from using $nw$. From the reduced-form predictions given by the theoretical framework in section 5, this treatment improves screening precision without introducing additional disparate treatment by adding $w(b)$, which favors women. Moreover, with another productivity signal, the final score and therefore hiring threshold relies less on the unconditional group mean (in effect, reducing statistical discrimination). In addition, less weight is given to evaluator bias still remaining in $nw$, further helping women. However, the introduction of $w(b)$ adds a tool with potentially different screening precision and bias. If the written exam has no disparate impact or favors female candidates, it will also have a positive impact on women's hiring rate.

The next two estimates compare alternative treatments of a screening process containing a mix of written and non-written tools, $w + nw$. First, removing the non-written while making the written blind is particularly interesting since it could be interpreted as an employer induced to drop a high-discretion screening tool ($nw$) and altering the other to ensure no disparate treatment. This can appeal as an approach to employers interested in reforming hiring practices by removing stages seen as potential barriers to increasing diversity, fine-tuning the remaining practices, but not replacing the removed tool with any other signal.

The lack of a statistically significant effect — despite being precisely estimated — indicates that potential gains from removing disparate treatment and potential disparate impact from interviews and evaluator bias from the written test are offset by loss in precision from dropping $nw$. How much does this precision loss matter? Assuming that evaluators favor men, so that removal of disparate treatment helps women (both via blinding and removing $nw$), either the precision loss of non-written exams is large enough to offset the complete elimination of evaluator bias and disparate impact of interviews (if they favor women), or interview signal precision matters less because interviews favor women (on the disparate impact margin).

Now consider a treatment that starts with the same screening mix $w + nw$, but only blinds the written stage. This captures an employer who only fine-tuned some existing practices (by blinding), without removing any stages. I estimate another null effect, although estimates are not precise. Note that the reduced form prediction for $w + nw \longrightarrow w(b) + nw$ is that the female hiring gap increases relative to men due to partial removal of evaluator bias. However, as the implementation of this type of screening involves different weights on the different stages, I further investigate how the effect varies with the weight on the written test.

To conclude this section, Table 8 compares female participation shares in applicant pools for each treatment type. Consistent with the idea from section 4.7 that perceived discrimination or unfair treatment during hiring may discourage minorities from applying in the first place, from section 4.7, column (1) finds that by blinding the written stage, the participation of women

in the applicant pool increases by 2%. Column (2) shows that switching from a non-written exam to a written stage women did not increase application rates relative to men. With a completely different screening method, women may think that the process is fairer, but may be uncertain about potentially allocating more time to prepare for the test. Alternatively, men could interpret the new testing method as a more competition-driven environment, eliciting more male candidates to apply, evidence of which I do not find.

In line with the cost of application explanation, column (3) shows that when employers introduced an additional screening requirement, the estimated effect is zero, so that it does not incentivize or disincentivize applications from women relative to men. This suggests that even if adding a blind objective stage could differentially appeal to female candidates due to a less biased process, the higher application cost might be the dominant force. In contrast, column (4) shows that removing an interview from $w + nw$ leads to an increase in the female share of applicants of 5%. Similar to column (1), blinding a written exam in the treatment type $nw + w \longrightarrow w(b) + nw$ improves female application rates relative to men. Finally, keeping all screening tools but fine-tuning written exams by making them blind increases female participation.

# 7 Impact of Who Hires

In the previous sections, I have focused on the redesign of screening tools (blinding written exams, removing subjective stages, among others) to improve gender equality in labor markets. This approach recognizes that certain hiring practices may leave firms more susceptible to biased decisions and may impact minority groups differently. I now turn my analysis to another determinant of unequal treatment: hiring managers. There are two main reasons to study how the features of evaluators affect minority hiring outcomes.

First, the changes in screening tools I studied involve limiting evaluator bias expression via more or less discretion, taking managers' potentially biased behavior as given. Blinding exams removes any manager-related bias expression, and altering the screening tool mix to make it less discretionary removes part of the weight from evaluators' private signal or bias. However, some of these choices come with consequences. My estimates indicate that the removal of interviews or other non-written screening tools from a mix containing written and non-written methods does not appear to improve women's final scores or hiring rates. This suggests that removing discretionary screening tools may not help improve gender equity, as loss in precision may offset potential relative gains from removing disparate treatment and impact.

Second, employers and policymakers may have an intrinsic preference for promoting less biased decisions, but may still not want to modify screening tools. Evaluator discretion is

likely to provide important private information that could both increase efficiency of hires and even help minorities if the disparate impact of standardized or written tests is relatively larger. Intuitively, the usefulness of standardized or blind tests depends on the context: if an employer is hiring a driver, it is unlikely that a math test will select the best candidates at driving. If a short driving test of each candidate seems to be a reasonable screening option, implementing the stage in a "blind" manner seems challenging. However, my previous estimates show that women face disparate treatment from evaluators even in screening stages with little discretion.

To investigate how committee composition contributes to biased hiring decisions, I take advantage of several features of the constructed data set. First, observing individual scores for each hiring stage within a job process, committee member and applicant identities, allows me to compare blind and non-blind scores received by the same candidate when facing different committees. Second, exploiting information on individual scores given by each committee member, I analyze within-committee dynamics and how the same hiring manager changes their behavior when serving with different colleagues on committees.

## 7.1 The Role of Diverse Committees

A common strategy to improve diversity of employees being hired is to make the pool of evaluators more diverse. A diverse hiring committee may bring various viewpoints into the search process, thereby providing more nuanced evaluations of applicants with a different sets of characteristics. In the case of gender, a "critical mass of women" in a team (Kanter (1977)) may correlate with group performance (Woolley et al. (2010)) and influence behavioral changes in male colleagues (Adams and Ferreira (2009)).

This section uses more recent data, spanning 1999-2019, pooled from Brazil's federal and state governments and broadly corresponds to the setting of the impartiality reform. The sole difference, of course, is that written exams in the sample are necessarily blind, which allows for comparison of blind to non-blind scores within the same job process. This enables me to compare how candidates are scored in hiring stages where their identity is concealed compared to when it is not. As presented formally in Section 5, the blind score is free of any bias on the part of the hiring manager, including statistical and taste-based discrimination. In order to use the blind score as a counterfactual measure of applicant ability, I account for the fact that the two scores may not be comparable as written and non-written tests might measure different skills. I do so by investigating how the bonus a given candidate received at non-blind non-written tests, compared to blind written tests, varies across committees with different gender composition, depending on her gender. Importantly, this comparison controls for candidates' potential differences in abilities between written and non-written tests.

Table 10 shows descriptive statistics by candidate gender for various job process evaluation scores. Women receive slightly lower scores for resumes than men and have slightly higher scores in blind written exams, although neither of these differences is statistically significant. However, female candidates receive 4 percentage points less in non-written exams than men, which results in a non-written blind-written score gap for women, while men have virtually the same performance in both exam types on average. Finally, despite having higher resume and blind written scores, women's final scores are 2 percentage points lower than men's due to the penalties in non-written stages.

While these raw differences do not necessarily reflect evaluator bias in non-written exams, Table 11 reveals an interesting pattern related to the gender composition of hiring committees. Hiring odds of female candidates in committees with less than 30% of women are much lower than men's. As the gender ratio of the committee starts to balance, hiring changes of both groups begin to align, until female candidates become slightly favored when the committee is female-majority.

These patterns indicate that, either due to lower skill or actual performance in non-written exams, or due to some factor related to the higher degree of discretion in these stages, women's scores are lower in interviews, practical exams, and oral presentations. This harms their cumulative score, despite doing at least as well on blind exams as men. Moreover, when committees are more female-dominated, men's hiring rates decline, and women's increase.

To tease out the effect of committee gender composition on gender gaps, I implement a difference-in-difference-in-differences approach where I net out differences in individuals' skills between written and non-written exams:

$$\underbrace{\text{Score}_{icj}^{nw} - \text{Score}_{icj}^{w(b)}}_{\Delta S_{icj}} = \beta \left(\text{Female}_i \times \%\text{Female Evaluator}_c\right) + \gamma_c + \mu_i + \varepsilon_{icj}$$

where the difference between a candidate's blind written and non-written exam scores is regressed on a female indicator interacted with the share of female committee members. The regression also controls for candidate and committee fixed effects, so that $\beta$ is identified off of candidates who were evaluated by committees with different gender compositions (i.e., individuals who applied to more than one job), and measures how evaluation committee's gender bias at non-blind non-written tests varies depending on the committee's gender composition. Combining the comparison of blind and non-blind assessments with contemporaneous within-individual comparisons across hiring processes deals with the concern that blind and non-blind

examinations may not necessarily measure the same skills.[31] The identification assumption in this case is that candidates differences in abilities between blind written and non-written tests is constant across hiring processes.[32]

Table 12 shows the results. The first column presents the gender differences in the raw blind gap ($\Delta S_{icj}$). Women have a slightly lower non-written premium than men, consistent with the previous discussion. When analyzing this subjectivity premium by committee composition, female candidates evaluated by committees with less than 50% women receive an even lower non-written premium relative to men. Strikingly, this pattern reverses when the committee is female dominated: women receive the same non-written premium as men or even higher. When controlling for individual differences in skills between the two exam types and directly assessing the effect of higher shares of female committee members, columns (4) through (6) show that the non-written premium for female candidates grows with more women on the committee. Thus, the non-written premium, the final score received and the probability that a woman receives an offer all rise relative to men when there are relatively more evaluators.

## 7.2 Differences in Scores Between Male and Female Evaluators

Why do female candidates receive higher scores when there are more women in the hiring committee? First, female evaluators may score female candidates more favorably than male evaluators. It is not a priori clear whether female evaluators would benefit female candidates, as they may share the same stereotypes as men on the committee. Moreover, homophilic competition may emerge, in a similar sense to Beaman et al. (2012) who argues about members of an ethnic network facing a trade-off in the context of job referrals due to competition over employment in an occupational niche.

Second, the presence of female colleagues may affect male evaluators' scoring behavior – perhaps by inducing norms-based costs due to different group gender norms (Akerlof and Kranton (2000)), inducing a censoring effect, or increasing awareness over unconscious biases. This is consistent with evidence that shows that women change male behavior in other settings, such as male board member attendance improving when there are more women on the board (Adams and Ferreira (2009)). Similarly, Boyd et al. (2010) find that male judges are more likely

[31] Note that in light of my conceptual framework, the within-individual comparison also accounts for differences between examinations types in terms of their potential disparate impact, namely, whether one type of examination favors one group more than other in a way that is not related to productivity.

[32] Studies have used differences between men and women's gaps in blind and non-blind tests to identify discrimination (Blank (1991), Goldin and Rouse (2000), as well as teacher bias in grading (Lavy (2008)). However, double (or "simple") differences identified off comparisons between individuals and may be biased by gender-specific differences in individual ability between blind and non-blind examinations. Breda and Ly (2015) compare the same student's blind and non-blind scores across different subjects to examine how evaluation bias changes with the feminization of a subject.

to hand down favorable decisions to plaintiffs alleging gender discrimination when they serve on panels with a female judge. [33]

To understand how different committee members react to the committee gender make-up, I first analyze how female evaluators score female candidates relative to men. Figure 12 shows that female judges give women better scores on non-written exams, and surprisingly give lower scores than male evaluators for blind written exams. As a result, female committee members give a 4 percentage-point non-written bonus to women. In Table 13, I analyze how women's scores given by female and male committee members change relative to men's when the hiring committee has a greater share of women. The specification also controls for evaluator fixed effects, so that each estimate captures how the same evaluator of a given gender scores women relative to men when there are different proportions of female colleagues on the hiring committee.

Columns (1) through (4) show that female committee members do not particularly change their scoring behavior when there are more female colleagues. The estimated effect on the non-blind premium is small and almost statistically non-significant. Columns (5) through (8) show a different response by male committee members. The same male evaluator scores female candidates 0.7 percentage points higher when there are more female colleagues on the committee. As expected, scoring on blind tests remains the same regardless of committee composition, which then implies a 1.4 percentage-point non-blind bonus to women. Overall, the change in men's behavior from the presence of more female colleagues accounts for the equity benefit toward female candidates.

## 7.3 Raising the Costs to Evaluator Bias Expression

Why would the presence of female colleagues reduce bias from male evaluators toward female applicants? To gain insights, I re-examine the hiring manager's payoffs from Section 5:

$$u_j\left(y, \pi\left(x\right)\right) = y + (1 - c_\theta)\pi_j(x) \equiv y + d_\theta \pi_j(x)$$

[33] In contrast, in the context of academic promotion competitions in Italy and Spain, Bagues et al. (2017) find that male evaluators become less favorable toward female candidates with women on the committee. However, under which hiring practices evaluators interact with applicants is not not taken into account. This relationship is important since hiring practices determine the discretion level to evaluators and consequently the extent that human bias can be expressed. Zinovyeva and Bagues (2011) document either a positive or non-significant relationship between the proportion of female evaluators and success of female applicants, depending on the position, in academic promotions in Spain. On the other hand, for similar academic promotions in Italy, Bagues et al. (2014) find a negative effect of higher share of women on committees and success rate of female candidates. The difference could lie in the hiring practices used in these different settings, as in Spain evaluations involved a research seminar given by the candidate, while in Italy the evaluation relied completely on CVs and publications and it did not require any personal interaction between evaluators and candidates.

when the cost of expressing bias, previously fixed, might be determined endogenously based on some underlying characteristic of the job hiring process: $c_\theta(\phi)$. Therefore, by finding an appropriate $\phi$ such that $c'(\phi) > 0$, employers could successfully reduce the disparate treatment of non-written exams.

In practical terms, $c_\theta(\phi)$ represents the cost of detection that an evaluator faces when giving a score that deviates from the signal provided by the screening tool. The higher the discretion degree of a tool (e.g., interview), the harder it is for an employer to observe a deviation from the observed signal. One choice of variable for $\phi$ could include the transparency level in the screening practice, either by conducting interviews in front of impartial observers (say, compliance officers), or recording the process (which would potentially expose firms to litigation risk). Job processes in Brazil's public sector implement these measures to some extent, since hiring must be transparent. Yet, even in settings with little discretion, I find evidence of disparate treatment.

To inform on the choice for $\phi$, I drew from a large set of existing literature that shows the effects of conditioning evaluator's behavior along some demographic variable, particularly matching the demographic groups being studied. Formally, I let

$$c_\theta\left(\frac{\sum_j f_j}{\sum_j f_j + \sum_j (1 - f_j)}\right), \qquad c'_\theta > 0$$

with higher-order derivatives unsigned for now and $f_j$ representing the number of female committee members. The expression above considers that bias expression of an evaluator toward a minority group increases in the share of committee members of that minority group. Put simply, expressing bias against female candidates becomes increasingly more costly when more women are on the hiring committee, perhaps due to increasing norms costs associated with group gender norms, which captures the effects I estimate in Table 13.

# 8 Conclusion

Hiring decisions shape firm outcomes and determine individuals' access to labor market opportunities. To ensure fair recruiting processes and increase employee diversity, organizations face several challenging questions. Does replacing interviews with objective or standardized tests help or hurt female candidates? Should an employer remove screening practices with high discretion even if they may provide important information about applicants? Do different choices of individuals conducting the screening lead to more diverse hires? Causally linking the design of hiring practices to gender equity in hiring requires overcoming lack of data on

recruiters' decision making process and generating appropriate variation in the choice and implementation of screening methods.

This paper studies how the design and implementation of firm hiring practices affect gender equity. I open the black-box of hiring decisions by developing a natural language processing algorithm that distills high-dimensional, unstructured text records into a uniquely-detailed dataset on the universe of hiring processes in Brazil's public sector. The data contain complete information on candidate performance, including job offers and individual scores, screening methods, and committee hiring members' evaluations to each job applicant in all hiring stages. This setting provides valuable lessons for private sector firms and professionalized bureaucracies around the world, as public employee selection in Brazil uses screening practices widely adopted in competitive job processes.

Several implications emerge from my analysis. First, gender disparities in hiring come both from screening practices – either by their differences in precision or the existence of bias toward a group – and decision makers. Second, hiring managers matter even in instances when the screening tool provides a relatively objective signal and already limits bias expression. Third, concealing candidate identity in an existing test benefits the less-favored group unambiguously, without leading to an equity-efficiency tradeoff. However, blinding alone may not be enough to improve gender diversity, as evaluator bias allowed by discretion in other hiring stages may be the dominant force.

Further, introducing an additional hiring step comprising blind tests helps female candidates the most. This indicates that improving the accuracy firms' assessment of applicant productivity offsets evaluator bias in other stages of the job process. In fact, removing subjective tests and blinding exams fails to improve women's outcomes, suggesting that employers should carefully weigh precision loss and net gains from bias reduction. Finally, my results shed light on how to optimally design the composition of hiring committees to minimize gender disparities. Increasing the presence of female evaluators in hiring committees raises scores given by male colleagues to female applicants in subjective stages. More gender-balanced decision makers improve diversity even when the firm does not promote any changes to screening tools.

Remaining questions to be addressed in future research include the implications of different screening practices for the quality of candidates. In separate work, I refine the natural language processing algorithm developed in this paper to extract long-term career progression records of hired job applicants. This allows for empirically testing the theoretical predictions that most changes in screening practices that increase gender equity involve no efficiency losses. More generally, these output and performance measures provide the necessary information to study efficiency concerns in the design of more equitable recruiting practices.

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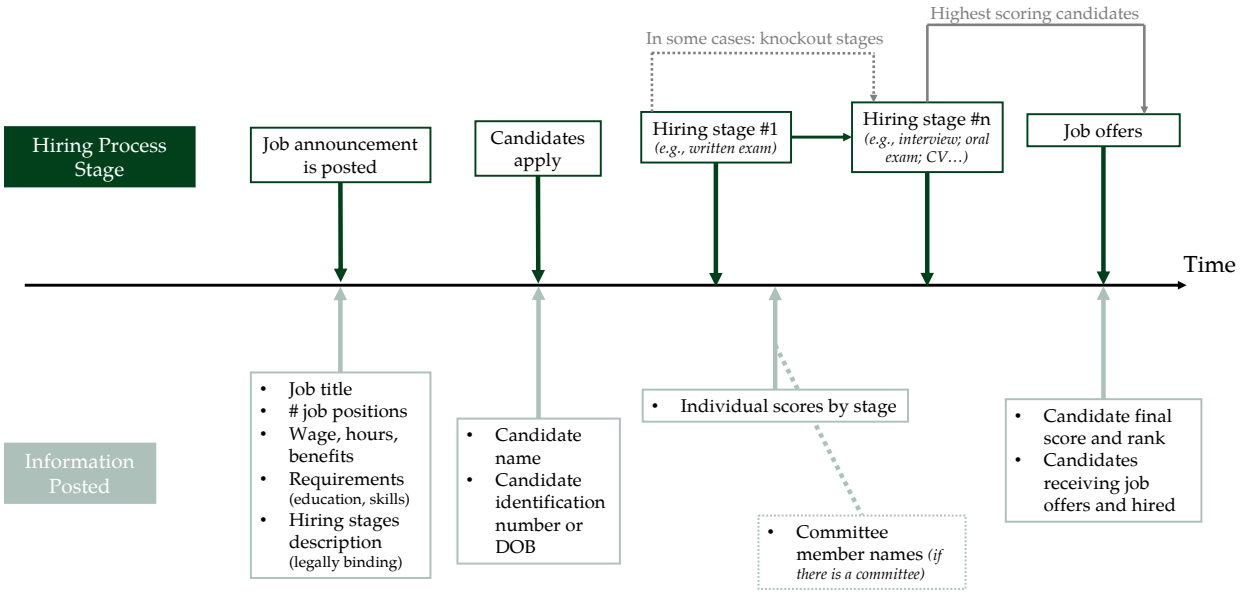
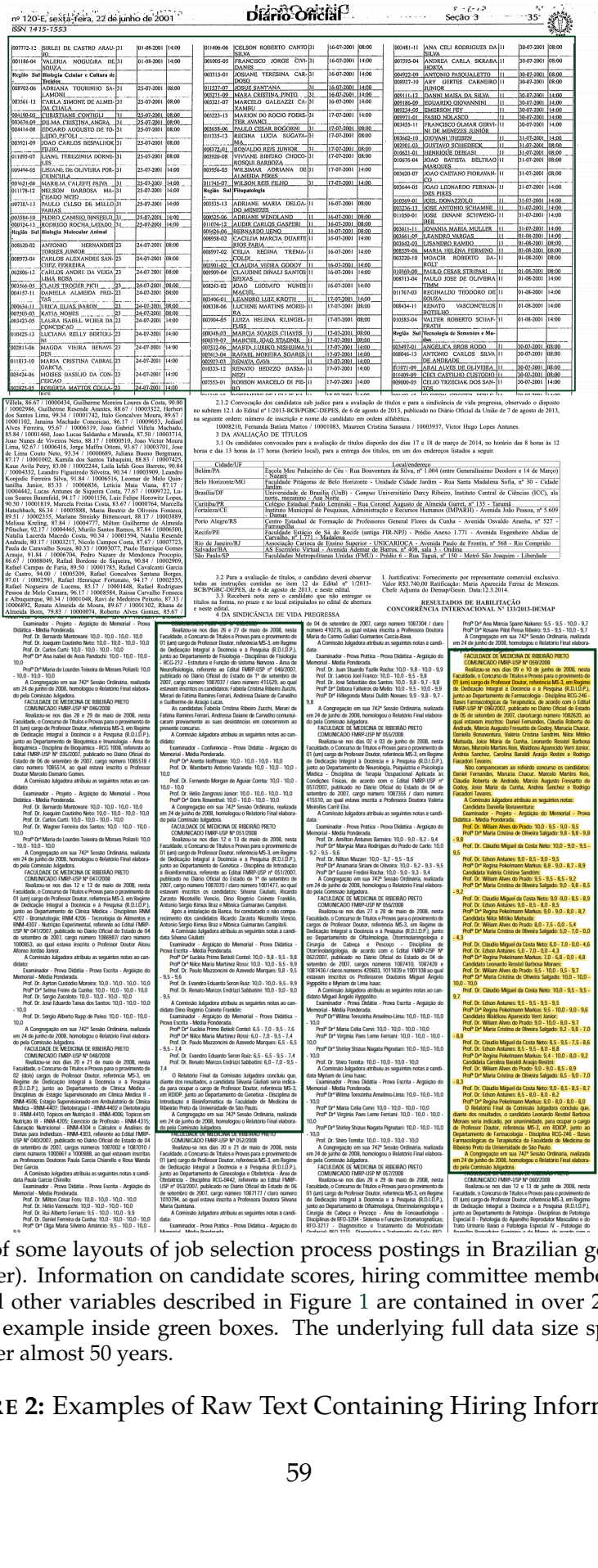
# 9 Figures and Tables



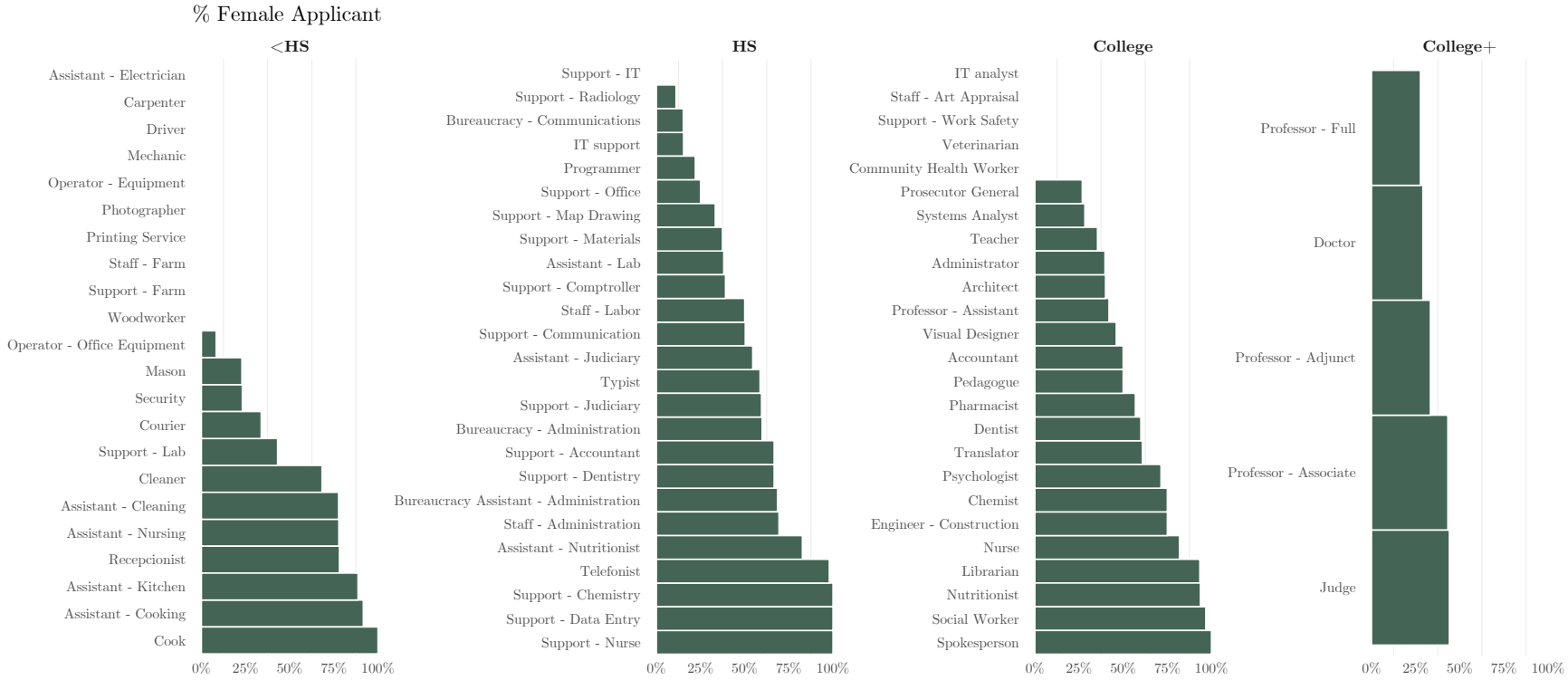

***Notes:*** This shows the stylized structure of a hiring process in the Brazilian public sector posted in raw government publications (official gazettes). Information at the top (dark green) describes the screening dynamics from the moment a job is announced until job offers are sent out. The lower part of the figure (light green) shows variables I construct based on observable information in the text of official government documents. The procedure for data extraction is described in Section 3.

**FIGURE 1:** Stylized Structure of Hiring Processes



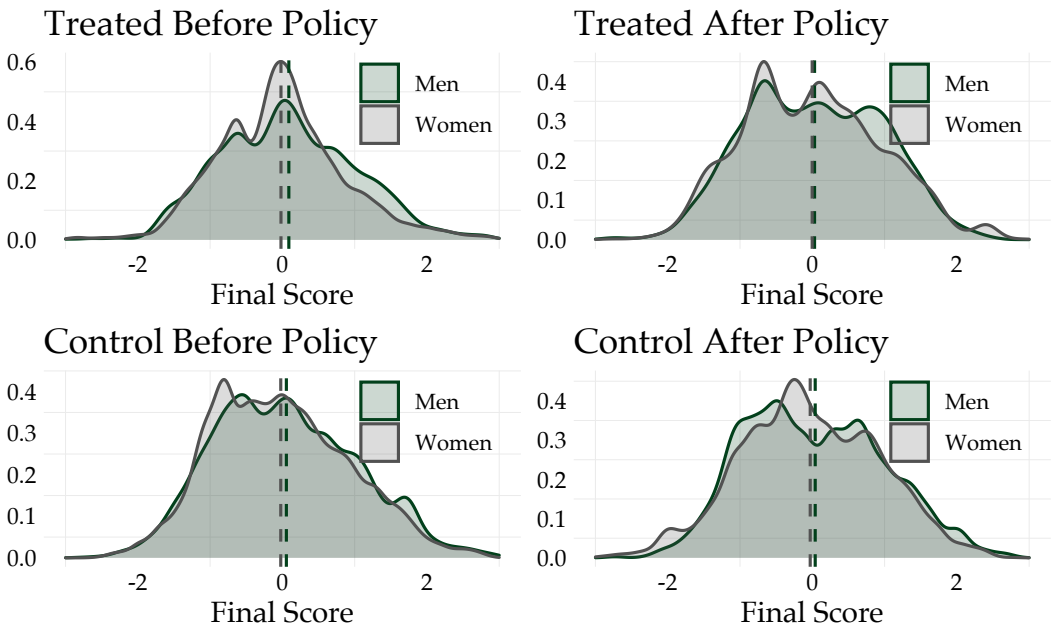
*Notes:* This is a sample of some layouts of job selection process postings in Brazilian government gazettes (similar to the Federal Register). Information on candidate scores, hiring committee members, exam types, selection process rules, results and other variables described in Figure 1 are contained in over 200 different layout types and can be found in the example inside green boxes. The underlying full data size spans over 35 million text documents published over almost 50 years.

**FIGURE 2:** Examples of Raw Text Containing Hiring Information



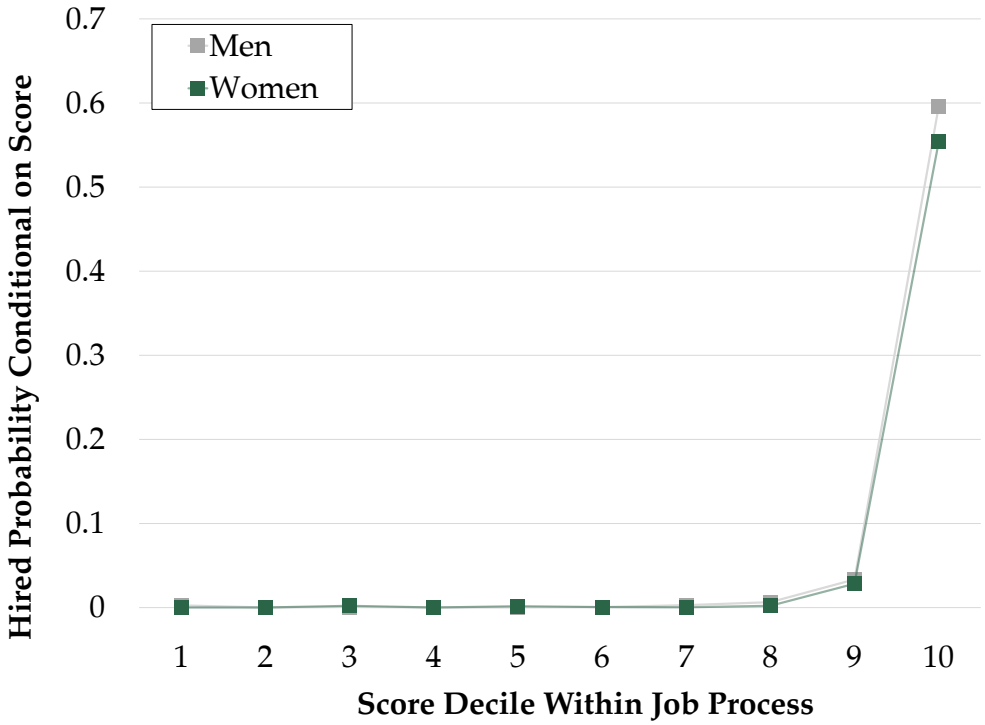

***Notes:*** This figure shows the gender share distribution of job applicants to various occupations and skill levels in Brazil's public sector from 1986 to 1991. Occupation titles in the data follow employer-specific career titles given each organizational structure. These titles are translated from Portuguese and then manually assigned occupation categories based on job or title description so that homogeneous occupation groups can be created. The occupation level displayed in the figure is intermediary — equivalent the Census Bureau Standard Occupational Classification (SOC) 4-digit code in most cases and slightly more granular in others. Skill levels are directly informed in job announcements, where only candidates attaining that educational level can apply for the job process. In the rare cases where different job titles are bridged by the same occupation name and they have distinct educational requirements, I consider the in the job title most closely reflecting the underlying occupational name or that is required more frequently. Occupations with blank bars had zero female applicants (e.g., carpenter, driver, mechanics) and some occupations had only women applying (e.g., data entry (support), spokesperson).

**FIGURE 3:** Distribution of Female Applicants by Occupation and Skill Level



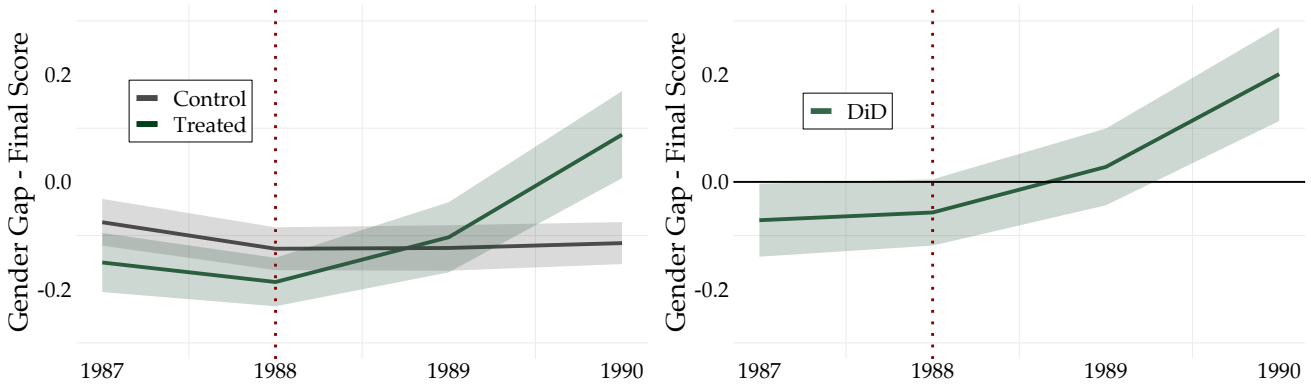

***Notes:*** These panels show standardized final scores of male and female job applicants in federal and state job processes. Federal (treated) and states (control) before and after the impartiality reform are displayed in each panel. To compare magnitudes across densities, tails are censored between 2 standard deviations right and left.

**FIGURE 4:** Final Scores Distributions




***Notes:*** This figures illustrates how final scores completely determine candidates' probability of bein hired in public sector job processes. In accordance with the law requiring that the highest scoring candidates across all evaluation stages are offered jobs until all openings are fulfilled, candidates with scores in the highest decile in their job process have a 60% probability of receiving a job offer. Top performing women have a slightly lower probability of receiving a job offer than top performing men (across all job processes). Not all top candidates receive offers because public sector jobs are oversubscribed.

**FIGURE 5:** Candidate Final Scores Determine Hiring Chances




***Notes:*** The figure on the left plots $\widehat{\gamma}$ estimates of the regression

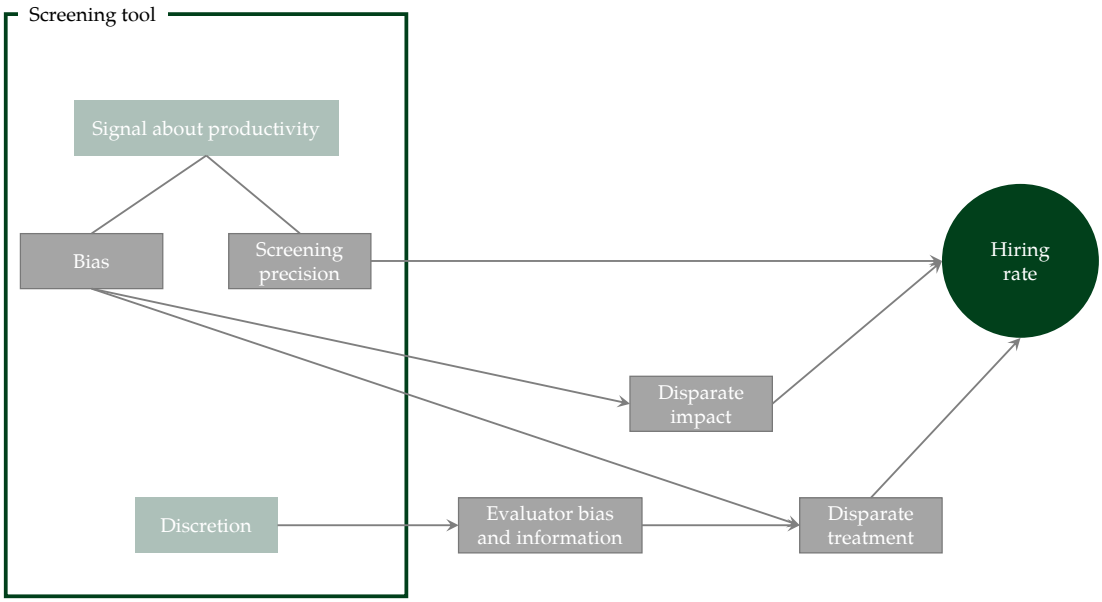
$$\text{Final Score}_{it} = \delta_{o(i)} + \gamma \text{Female}_i + u_{it}$$

for control (state governments) and treated (federal government) groups in each year, where $i$ denotes the job selection process and $o$ denotes the occupation or job title. The figure on the right shows dynamic effects of the baseline DiD model

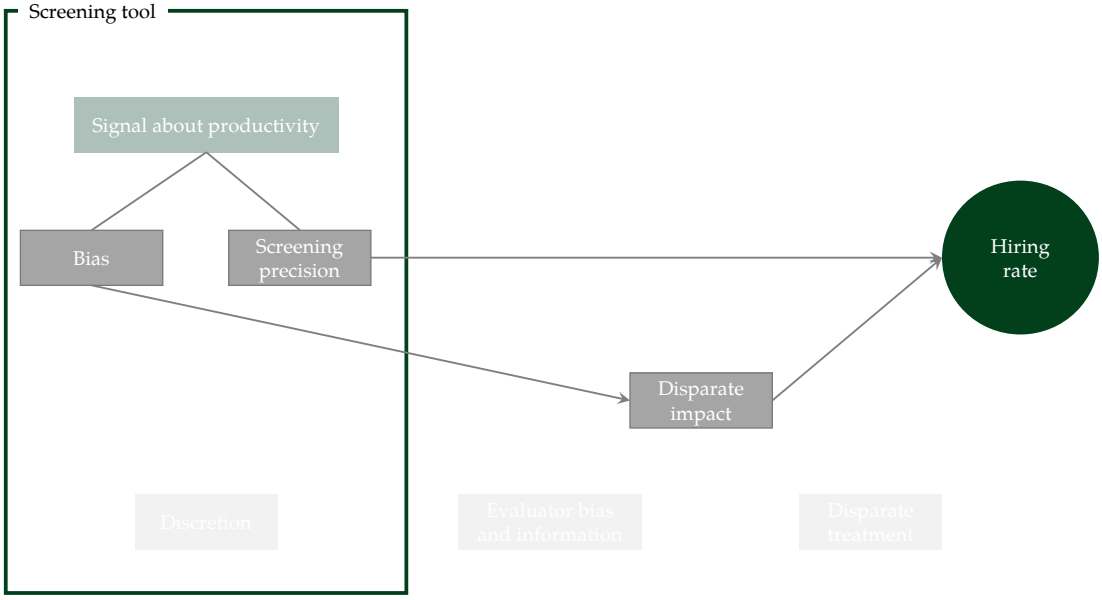
$$\text{Final Score}_{it} = \delta_{o(i)} + \beta \left( \text{Post}_{o(i),t} \times \text{Female}_i \right) + \gamma_t + u_{it}$$

where $\text{Post}_{o(i),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Standard errors are clustered at the job process level. Shaded areas are 95% confidence intervals. Pooled DiD estimates are shown in Table 2.

**FIGURE 6:** DiD Dynamic Effects of Impartial Hiring on Final Scores




**(a)** Standard Screening Tool




**(b)** Blinding Screening Tool

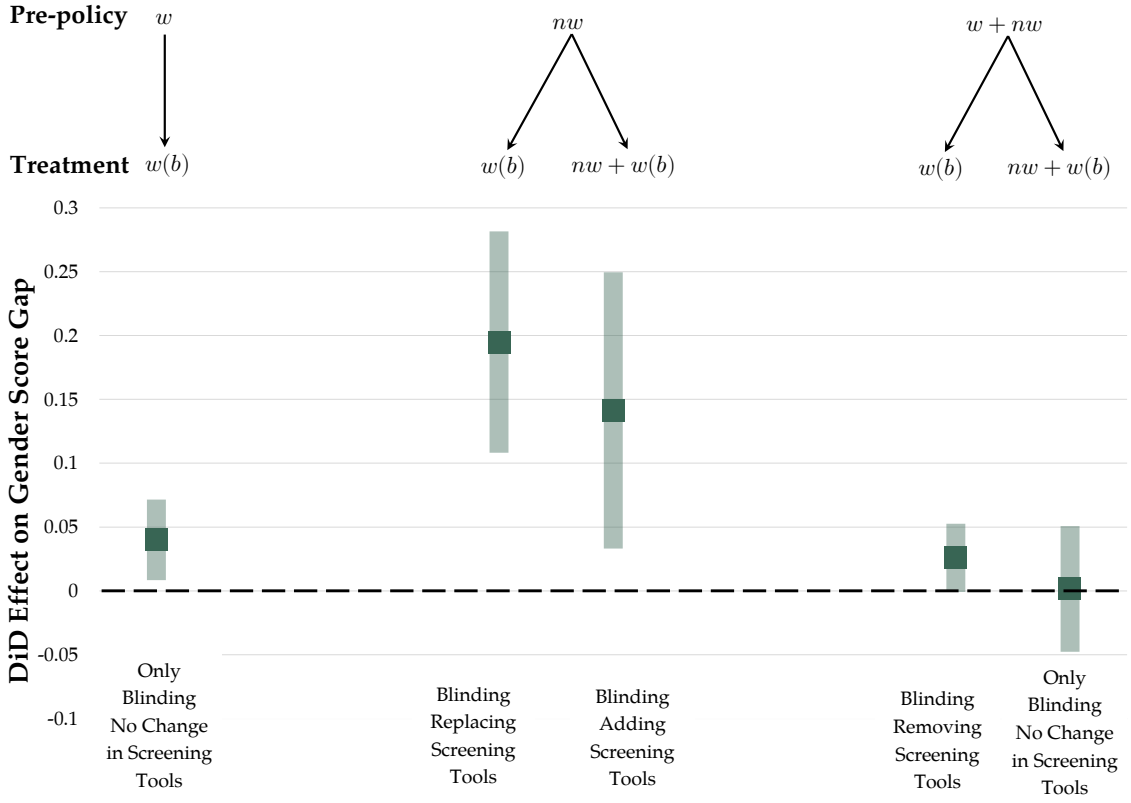
***Notes:*** This figure represents the main forces captured in my conceptual framework that determine hiring rates of candidates evaluated using a screening tool, such as a test or an interview. A screening tool provides a productivity signal with certain precision, but the signal can have a bias that favors a specific demographic group. In the model, this bias term receives the interpretation of a disparate impact. The other property of a screening tool is the degree of discretion it enables. More subjective practices allow a hiring manager's evaluation to deviate from the signal provided more easily. When evaluators are biased toward a group, the screening practice also allows disparate treatment. By concealing candidates' identities — when possible or desirable — in the screening tool, managers cannot express bias or statistically discriminate, leaving only precision and tool bias to determine hiring rates.

**FIGURE 7:** Hiring Rate Determinants: Conceptual Framework

| $z=1$ \ $z=0$ | **Written** | **Non-written** | **Written & Non-written** | **Written Blind** | **Written Blind & Non-written** |
|---|---|---|---|---|---|
| **Written** | Always Written | | | | |
| **Non-written** | | Always Non-written | | | |
| **Written & Non-written** | | | Always Written & Non-written | | |
| **Written Blind** | $w \to w(b)$ **[20%]** | $nw \to w(b)$ **[6.7%]** | $w+nw \to w(b)$ **[6.7%]** | Always Written Blind | |
| **Written Blind & Non-written** | $w \to w(b)+nw$ | $nw \to w(b)+nw$ **[20%]** | $w+nw \to w(b)+nw$ **[46.7%]** | | Always Written Blind & Non-written |

***Notes:*** This figure illustrates all possible potential treatments (strata) generated by the 1988 Impartiality Reform on federal jobs in Brazil's public sector. Areas shaded in gray are ruled out by standard DiD assumptions and 5 out of the 6 allowed treatments are consistent with the variation induced by the policy: a job process transitioning from written exam to blind-written exam ($w \longrightarrow w(b)$), a job process comprising a non-written exam switching to a blind-written ($nw \longrightarrow w(b)$), or only adding the blind-written test ($nw \longrightarrow w(b)+nw$), and a hiring process using a mix of written and non-written tools potentially dropping the non-written and blinding the written ($w+nw \longrightarrow w(b)$) or just blinding the written ($w+nw \longrightarrow w(b)+nw$). The potential treatment $w \longrightarrow w(b)+nw$ is accounts for less than 1% of transitions in the data. Written exams are shorthand for written or multiple-choice tests, and non-written indicate interviews, practical exams, or oral exams. Numbers in $[\cdot]$ give the frequency of each treatment type in the estimating sample.

**FIGURE 8:** Potential Treatments Induced by Reform

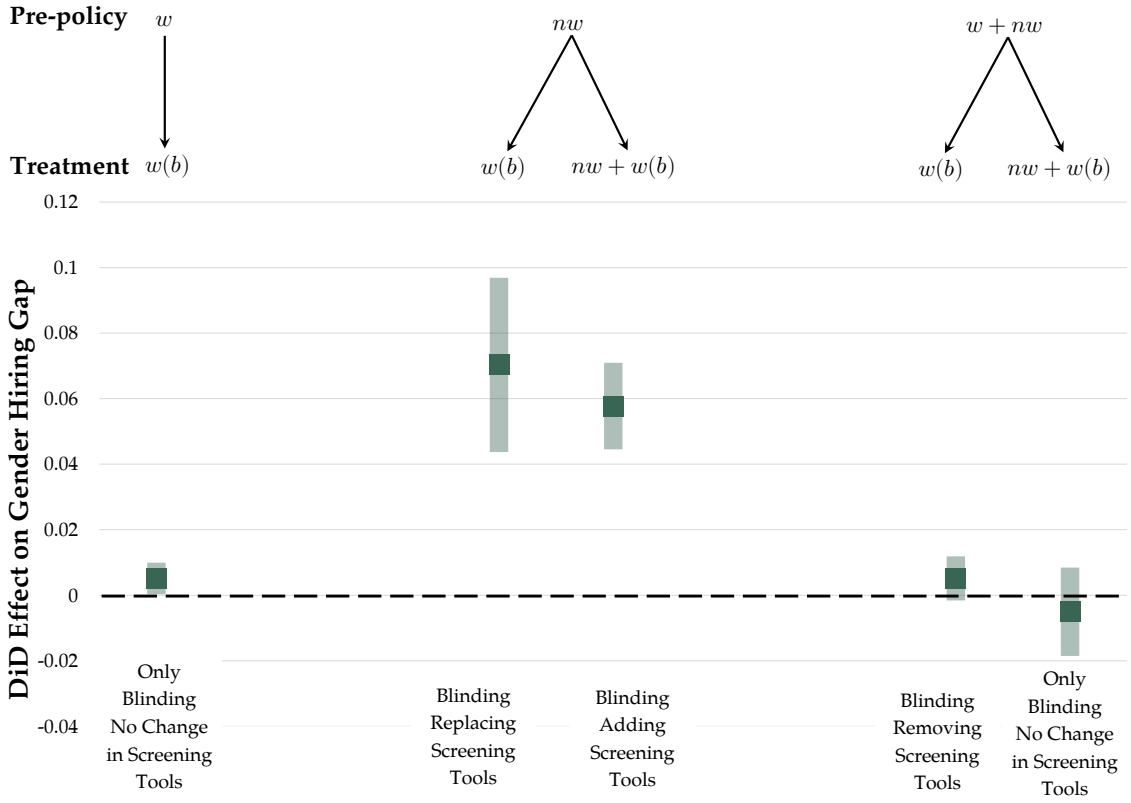



***Notes:*** This figure plots treatment effects for each treatment type $g$ induced by the 1988 Impartiality Reform in Brazil's public sector. Each bar central point estimates a version of the DiD regression

$$\text{Final Score}_{git} = \delta_{o(g,i)} + \beta_g \left( \text{Post}_{o(g,i),t} \times \text{Female}_i \right) + \gamma_t + u_{git}$$

where $\text{Post}_{o(g,i),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Treatment type $g$ represents a job process transitioning from written exam to blind-written exam ($w \longrightarrow w(b)$), a job process comprising a non-written exam switching for a blind-written ($nw \longrightarrow w(b)$), or only adding the blind-written test ($nw \longrightarrow w(b) + nw$), and a hiring process using a mix of written and non-written tools potentially dropping the non-written and blinding the written ($w + nw \longrightarrow w(b)$) or just blinding the written ($w + nw \longrightarrow w(b) + nw$). Standard errors are clustered at the job process level. Bars are 95% confidence intervals.

**FIGURE 9:** Treatment Effects of Changes in Screening Tools: Gender Final Score Gap

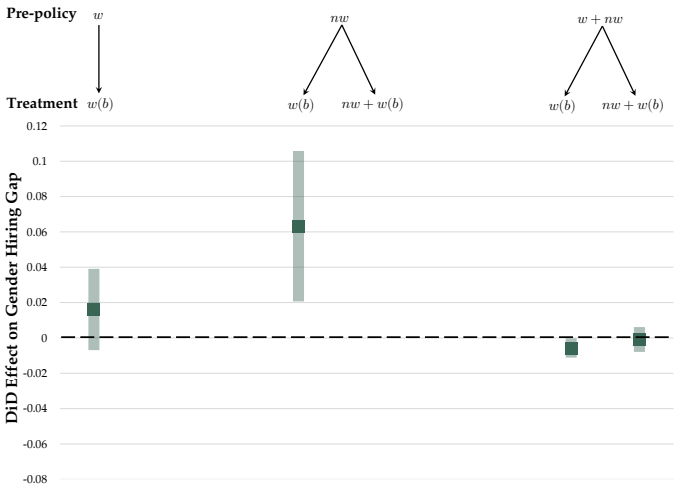



***Notes:*** This figure plots treatment effects for each treatment type $g$ induced by the 1988 Impartiality Reform in Brazil's public sector. Each bar central point estimates a version of the DiD regression

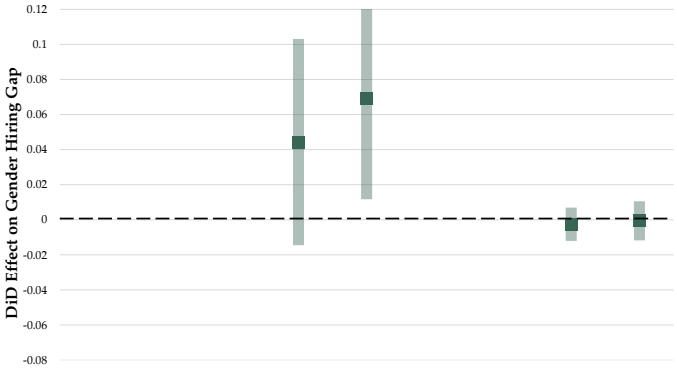
$$\mathbf{Pr}(Hired = 1)_{git} = \delta_{o(g,i)} + \beta_g \left( \text{Post}_{o(g,i),t} \times \text{Female}_i \right) + \gamma_t + u_{git}$$

where $\text{Post}_{o(g,i),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Treatment type $g$ represents a job process transitioning from written exam to blind-written exam ($w \longrightarrow w(b)$), a job process comprising a non-written exam switching for a blind-written ($nw \longrightarrow w(b)$), or only adding the blind-written test ($nw \longrightarrow w(b) + nw$), and a hiring process using a mix of written and non-written tools potentially dropping the non-written and blinding the written ($w + nw \longrightarrow w(b)$) or just blinding the written ($w + nw \longrightarrow w(b) + nw$). Standard errors are clustered at the job process level. Bars are 95% confidence intervals.

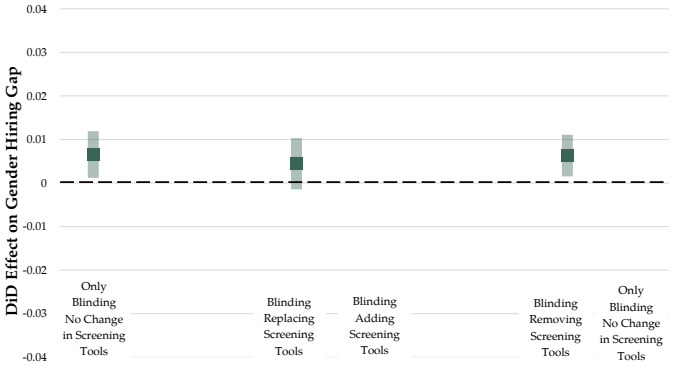
**FIGURE 10:** Treatment Effects of Changes in Screening Tools: Gender Hiring Gap




**(a)** Female-dominated Occupations



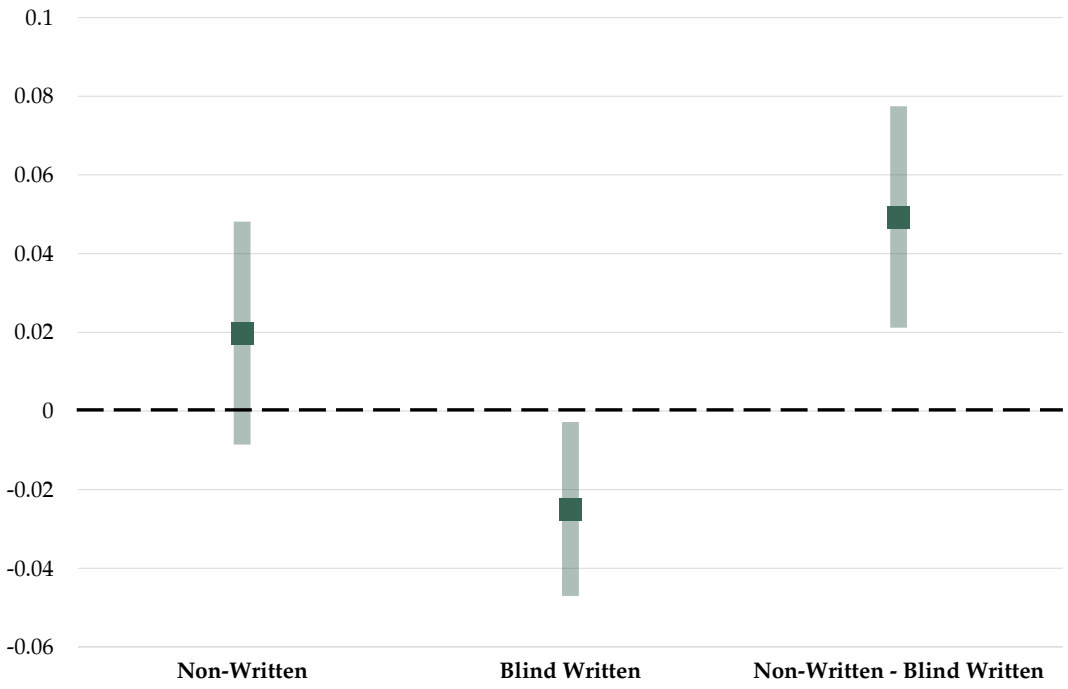

**(b)** Neutral Occupations




**(c)** Male-dominated Occupations

***Notes:*** This figure plots treatment effects for each treatment type $g$ induced by the 1988 Impartiality Reform by occupations with different degrees of feminization. Treatment type $g$ represents the following possible changes in screening: $w \longrightarrow w(b)$, $nw \longrightarrow w(b)$, $nw \longrightarrow w(b) + nw$, $w + nw \longrightarrow w(b)$, or $w + nw \longrightarrow w(b) + nw$. Occupations can differ in the types of changes in screening that the policy induced, and thus only the transition types that each group of occupations is treated with are estimated. Standard errors are clustered at the job process level. Bars are 95% confidence intervals.

**FIGURE 11:** Treatment Effects of Changes in Screening Tools, Occupation Feminization: Gender Hiring Gap




***Notes:*** This figure plots differences in scoring behavior of female applicants by female evaluators compared to male colleagues on the same committee for various score categories, $\text{Score}_{ic} | (\text{Female}_i = 1) = \alpha + \text{Female}_{jc} + \mu_j + u_{ijc}$. $\text{Female}_{jc}$ is a dummy for whether the score is given by a female committee member in job process $c$, and the sample is restricted to individual female candidate scores only. The outcome non-written−blind-written measures the blind-score gap given by the same committee member to a given candidate. Standard errors are clustered at the job process level. Bars are 95% confidence intervals.

**FIGURE 12:** Gender Differences in Scoring Behavior of Female Candidates

**TABLE 1:** Estimated Reaction to Impartiality Policy

| | At Least One Written Stage | | At Least One Non-Written Stage | | Only One Round & Written | All Rounds Non-Written |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\text{Post}_{o(j),t}$ | −0.006 | 0.252*** | −0.581*** | −0.145 | 0.476*** | −0.252*** |
| | (0.078) | (0.090) | (0.060) | (0.092) | (0.081) | (0.090) |
| Occupation FE | | X | | X | X | X |
| Year FE | X | X | X | X | X | X |
| Job Processes | 6,554 | 6,554 | 6,554 | 6,554 | 6,554 | 6,554 |

***Notes:*** This table displays regression coefficients of the model $y_{jt} = \delta_{o(j)} + \alpha \text{Post}_{o(j),t} + \gamma_t + u_{jt}$, where outcomes in columns (1) through (6) at the job process level $j$ are regressed on an indicator for treated post 1988 in federal jobs, $\text{Post}_{o(j),t}$, controlling for occupation title and year fixed effects. Each regression compares the effect of the impartiality reform on the outcome for the same occupation in the federal sector and states. Standard errors are clustered at the job process level.

**TABLE 2:** DiD Estimates of Screening Impartiality on Candidate Scores

| | Final Score | | | Written Score | | | Non-Written Score | | |
|---|---|---|---|---|---|---|---|---|---|
| | Women | Men | Gap | Women | Men | Gap | Women | Men | Gap |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| $\text{Post}_{o(i),t}$ | 0.067** | −0.075* | | 0.024 | −0.109* | | −0.010 | 0.020 | |
| | (0.030) | (0.037) | | (0.044) | (0.059) | | (0.071) | (0.099) | |
| $\text{Post}_{o(i),t} \times \text{Female}_i$ | | | 0.141*** | | | 0.134* | | | −0.031 |
| | | | (0.048) | | | (0.074) | | | (0.122) |
| Occupation FE | X | X | X | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X | X | X | X |
| Obs. | 54,892 | 32,067 | 86,959 | 34,511 | 15,546 | 50,057 | 29,444 | 10,764 | 40,208 |

***Notes:*** The table shows DiD estimates of the form $y_{it} = \delta_{o(i)} + \gamma \text{Post}_{o(i),t} + u_{it}$ only with female candidates in columns (1), (4), and (7), only with male candidates in columns (2), (5), and (8), and $y_{it} = \delta_{o(i)} + \beta \left( \text{Post}_{o(i),t} \times \text{Female}_i \right) + \gamma_t + u_{it}$ in the remaining columns. The outcome $y$ represents either a candidate's final score, written score (written exams), or non-written score (interview, oral, practical exams). $\text{Post}_{o(g,i),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Standard errors are clustered at the job process level.

**TABLE 3:** DiD Estimates of Screening Impartiality on Hiring and Application Rates

| | $\mathbf{Pr}(Hired\|\text{Female})$ | $\mathbf{Pr}(Hired\|\text{Male})$ | Hiring Gap | $\mathbf{Pr}(\text{Female})$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $\text{Post}_{o(i),t}$ | 0.003** | −0.004*** | | 0.010** |
| | (0.001) | (0.001) | | (0.005) |
| $\text{Post}_{o(i),t} \times \text{Female}_i$ | | | 0.007*** | |
| | | | (0.002) | |
| Occupation FE | X | X | X | X |
| Year FE | X | X | X | X |
| Obs. | 54,892 | 32,067 | 86,959 | 86,959 |

***Notes:*** The first column shows a regression coefficient capturing the probability that a given female job applicant receives a job offer: $\mathbf{Pr}(Hired = 1)_{it} = \delta_{o(i)} + \gamma \text{Post}_{o(i),t} + u_{it}$ which is ran only on female individuals, column (2) runs the same regression but in the male candidate subsample, column (3) runs $\mathbf{Pr}(Hired = 1)_{it} = \delta_{o(i)} + \beta \left( \text{Post}_{o(i),t} \times \text{Female}_i \right) + \gamma_t + u_{it}$ which measures the probability a female job applicant receives an offer relative to male job applicants. Finally, column (4) regresses the specification $\mathbf{Pr}(\text{Female} = 1)_{it} = \delta_{o(i)} + \beta \left( \text{Post}_{o(i),t} \times \text{Female}_i \right) + \gamma_t + u_{it}$. $\text{Post}_{o(g,i),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Standard errors are clustered at the job process level.

**TABLE 4:** DiD Estimates of Screening Impartiality on Job Process Outcomes

| | % Women of | % Female | Log # Candidates | | |
|---|---|---|---|---|---|
| | Hired | Candidates | All | Women | Men |
| | (1) | (2) | (3) | (4) | (5) |
| $\text{Post}_{o(j),t}$ | 0.134** | 0.061* | −0.245 | −0.212 | −0.316 |
| | (0.0692) | (0.042) | (0.322) | (0.382) | (0.251) |
| Occupation FE | X | X | X | X | X |
| Year FE | X | X | X | X | X |
| Obs. | 54,892 | 32,067 | 86,959 | 86,959 | 86,959 |

***Notes:*** The table shows selection process level regressions $y_{it} = \delta_{o(i)} + \gamma \text{Post}_{o(i),t} + u_{it}$, where $\text{Post}_{o(g,i),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Standard errors are clustered at the job process level.

**TABLE 5:** DiD Estimates of Effects on Scores and Hiring Probability, Skill Level

| | Final Score | | | Hiring Gap | | |
|---|---|---|---|---|---|---|
| | <High School | High School | College or Advanced Degree | <High School | High School | College or Advanced Degree |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\text{Post}_{o(i),t} \times \text{Female}_i$ | −0.016 | 0.160 | 0.204*** | 0.002 | −0.001 | 0.011* |
| | (0.084) | (0.111) | (0.080) | (0.003) | (0.002) | (0.005) |
| Occupation FE | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X |
| Obs. | 35,475 | 22,071 | 29,413 | 35,475 | 22,071 | 29,413 |

***Notes:*** This table reports, from columns (1) through (3), DiD estimates of the form $y_{it} = \delta_{o(i)} + \gamma \text{Post}_{o(i),t} + u_{it}$ where outcome $y$ represents a candidate's final score. $\text{Post}_{o(g,i),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Column (1) shows regression coefficients for a subsample of occupations with less than high-school education required. Column (2) runs the regression for a subsample of occupations with that require high-school education, and column (3) for a subsample of high-skill occupations that require a college degree or more. Columns (4) through (6) report regression coefficients for the respective subsamples capturing the probability that a given female job applicant receives a job offer relative to male applicants: $\mathbf{Pr}(Hired = 1)_{it} = \delta_{o(i)} + \beta \left( \text{Post}_{o(i),t} \times \text{Female}_i \right) + \gamma_t + u_{it}$. Standard errors are clustered at the job process level.

**TABLE 6:** DiD Estimates of Effects on Scores and Hiring Probability, Gender Identity

| Occupation Gender Identity | Final Score Gap | | | Hiring Gap | | |
|---|---|---|---|---|---|---|
| | Female | Neutral | Male | Female | Neutral | Male |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\text{Post}_{o(i),t} \times \text{Female}_i$ | 0.305*** | 0.149*** | 0.267** | 0.012* | 0.005* | 0.002** |
| | (0.105) | (0.043) | (0.117) | (0.006) | (0.002) | (0.001) |
| Occupation FE | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X |
| Obs. | 48,681 | 16,848 | 21,430 | 48,681 | 16,848 | 21,430 |

***Notes:*** This table displays regression coefficients of the model $y_{it} = \delta_{o(i)} + \beta \left(\text{Post}_{o(i),t} \times \textit{Female}\right) + \gamma_t + u_{it}$. The outcome represents either a female candidate's final score relative to a male candidate in columns (1)-(3), or the probability that a female candidate receives a job offer relative to a male candidate in columns (4)-(6). $\text{Post}_{o(g,i),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Columns (1) and (3) report estimates for a subsample of female-dominated occupations, defined as the proportion of women in that occupation$>$ 60%. Columns (2) and (5) run the regression for a subsample of occupations that are neutral or gender balanced, if the proportion of women in that occupation is between 40% and 60%. Columns (3) and (6) run the regression for a subsample of male-dominated occupations, defined as the share of women $<$ 40%. Standard errors are clustered at the job process level.

**TABLE 7:** DiD Estimates of Effects on Female Share of Hires, Gender Identity

| Occupation Gender Identity | % Women of Hired | | |
|---|---|---|---|
| | Female-dominated | Neutral | Male-dominated |
| | (1) | (2) | (3) |
| $\text{Post}_{o(i),t} \times \text{Female}_i$ | 0.104 | 0.131** | 0.259*** |
| | (0.068) | (0.064) | (0.081) |
| Occupation FE | X | X | X |
| Year FE | X | X | X |
| Obs. | 48,681 | 16,848 | 21,430 |

***Notes:*** The table shows selection process level regressions $y_{it} = \delta_{o(i)} + \gamma \text{Post}_{o(i),t} + u_{it}$, where $\text{Post}_{o(g,i),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Each column runs the regression for subsamples of female-dominated (share of women $> 60\%$), neutral (share of women $\in (40\%, 60\%)$), or male-dominated occupations (share of women $< 40\%$). Standard errors are clustered at the job process level.

**TABLE 8:** Treatment Effects of Changes in Screening Tools: % Female Applicants

| | % Female Applicants | | | | |
|---|---|---|---|---|---|
| | $w \to w(b)$ | $nw \to w(b)$ | $nw \to w(b) + nw$ | $w + nw \to w(b)$ | $w + nw \to w(b) + nw$ |
| | (1) | (2) | (3) | (4) | (5) |
| $\text{Post}_{o(c),t}$ | 0.023*** | −0.004 | −0.022 | 0.054*** | 0.043*** |
| | (0.008) | (0.013) | (0.020) | (0.009) | (0.012) |
| Occupation FE | X | X | X | X | X |
| Year FE | X | X | X | X | X |
| Obs. | 1,145 | 900 | 1,822 | 4,252 | 3,106 |

***Notes:*** This table plots treatment effects for each treatment type $g$ induced by the 1988 Impartiality Reform in Brazil's public sector. Each column estimates a version of the DiD regression

$$\%\text{ Female Applicants}_{gct} = \delta_{o(g,c)} + \beta_g \text{Post}_{o(g,c),t} + \gamma_t + u_{gct}$$

where $\text{Post}_{o(g,c),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Treatment type $g$ represents job process transitioning from written exam to blind-written exam ($w \longrightarrow w(b)$), a job process comprising a non-written exam switching for a blind-written ($nw \longrightarrow w(b)$), or only adding the blind-written test ($nw \longrightarrow w(b) + nw$), and a hiring process using a mix of written and non-written tools potentially dropping the non-written and blinding the written ($w + nw \longrightarrow w(b)$) or just blinding the written ($w + nw \longrightarrow w(b) + nw$). Standard errors are clustered at the job process level.

**TABLE 9:** Decomposing Treatment Effects by Weight of Blind Stages

| Treatment Group: $w + nw \longrightarrow w(b) + nw$ | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Final Score | | | | | Non-Blind Score | | Hiring Gap | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| $\text{Post}_{o(i),t} \times \text{Female}_i$ | 0.009 | 0.052*** | −0.011 | 0.059*** | −0.039 | 0.011 | −0.006 | 0.014*** | 0.009 |
| | (0.014) | (0.022) | (0.042) | (0.017) | (0.040) | (0.008) | (0.009) | (0.005) | (0.026) |
| Blind Score | | | | 0.630*** | 0.446*** | | | | |
| | | | | (0.049) | (0.029) | | | | |
| Job Process Blind Weight | | $> 50\%$ | $< 50\%$ | $> 50\%$ | $< 50\%$ | $> 50\%$ | $< 50\%$ | $> 50\%$ | $< 50\%$ |
| Occupation FE | X | X | X | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X | X | X | X |

***Notes:*** This table plots treatment effects for treatment type $g$ induced by the 1988 Impartiality Reform in Brazil's public sector. Each column estimates a version of the DiD regression

$$y_{git} = \delta_{o(g,c)} + \beta_g \left( \text{Post}_{o(g,i),t} \times \text{Female}_i \right) + \gamma_t + u_{gct}$$

where $\text{Post}_{o(g,c),t}$ is an indicator for whether the job process is for a federal-level position post Impartiality reform ($t \geq 1989$). Treatment type $g$ represents job process transitioning from using a mix of written and non-written tools to blinding the written ($w + nw \longrightarrow w(b) + nw$). The outcome $y$ represent either a candidate's final score, non-blind (non-written) score, or probability of being offered a job. Columns (2), (4), (6) and (8) condition on the weight on the blind written test in a job process to be $> 50\%$, and columns (3), (5), (7) and (9) for the weight on the blind written test to be less than 50%. Standard errors are clustered at the job process level.

**TABLE 10:** Summary Statistics, Hiring Committee Analysis

*Panel A. Job Applicant Statistics*

| | Resume Score | Blind Written Score ($w(b)$) | Non-Written Score ($nw$) | Score Gap $nw - w(b)$ | Final Score |
|---|---|---|---|---|---|
| Female Applicants | 0.863 | 0.860 | 0.816 | $-0.044$ | 0.871 |
| Male Applicants | 0.880 | 0.855 | 0.852 | $-0.004$ | 0.892 |
| Female $\neq$ Male? | No | No | Yes** | Yes*** | Yes* |
| Obs. | 51,809 | 51,809 | 51,809 | 51,809 | 51,809 |

*Panel B. Job Process Statistics*

| | % Female Evaluators | # Candidates | # Female Candidates | # Evaluators |
|---|---|---|---|---|
| Job Process Average | 46.1% | 4.58 | 2.52 | 3.24 |

***Notes:*** This table shows summary statistics of job applicants used in the hiring committee analysis in the paper. $w(b)$ represents blind written exams, $nw$ represents non-written exams (interview, oral examinations, practical exams). Female $\neq$ Male? reports whether the sample statistics between men and women are statistically different than zero.

**TABLE 11:** Raw Hiring Probabilities, Committee Gender Composition

| | $\mathbf{Pr}(Hired = 1)$ | | | |
|---|---|---|---|---|
| | $< 30\%$ Female Evaluators | $< 50\%$ Female Evaluators | $> 50\%$ Female Evaluators | $> 70\%$ Female Evaluators |
| Female Applicants | 0.25 | 0.46 | 0.33 | 0.33 |
| Male Applicants | 0.67 | 0.49 | 0.25 | 0.14 |
| Female $\neq$ Male? | Yes** | No | No | Yes* |
| Obs. | 30,701 | 38,004 | 22,500 | 10,800 |

***Notes:*** This table reports raw hiring probabilities (raw data) of female and male candidates for various gender make-ups in the evaluation committees they face. Female $\neq$ Male? reports whether the hiring probabilities are different between the two groups.

**TABLE 12:** Effect of Committee Gender Composition on Gender Equity

| | $\text{Score}^{nw} - \text{Score}^{w(b)}$ | | | | Final Score | | | |
|---|---|---|---|---|---|---|---|---|
| | Overall | $< 50\%$ Female | $> 50\%$ Female | Overall | Overall | $< 50\%$ Female | $> 50\%$ Female | $\mathbf{Pr}(Hired = 1)$ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $\text{Female}_i$ | $-0.023$* | $-0.069$*** | 0.034* | | | | | |
| | (0.014) | (0.017) | (0.018) | | | | | |
| $\text{Female}_i \times$ | | | | 0.407*** | 0.163*** | 0.587*** | $-0.396$*** | 0.414*** |
| $\%\text{Female Evaluator}_c$ | | | | (0.102) | (0.052) | (0.113) | (0.126) | (0.138) |
| Job Applicant FE | | | | X | X | X | X | X |
| Job Process FE | X | X | X | X | X | X | X | X |
| Obs. | 60,504 | 38,004 | 22,500 | 9,901 | 9,901 | | | 9,901 |

***Notes:*** This table plots estimates of the model $\text{Score}_{icj}^{nw} - \text{Score}_{icj}^{w(b)} = \beta\text{Female}_i + \gamma_c + \varepsilon_{icj}$ in columns (1) through (3). The regression captures the non-written penalty female candidates receive in the full sample (1), when being evaluated by committees with male-majority (2), and women-majority (3). Column (4) runs the augmented model $\text{Score}_{icj}^{nw} - \text{Score}_{icj}^{w(b)} = \beta\left(\text{Female}_i \times \%\text{Female Evaluator}_c\right) + \gamma_c + \mu_i + \varepsilon_{icj}$, where $\beta$ captures evaluator bias in non-written exams. Columns (5) and (6) run the same model but with candidate final score and probability of receiving a job offer as outcomes. Standard errors are clustered at the job process level.

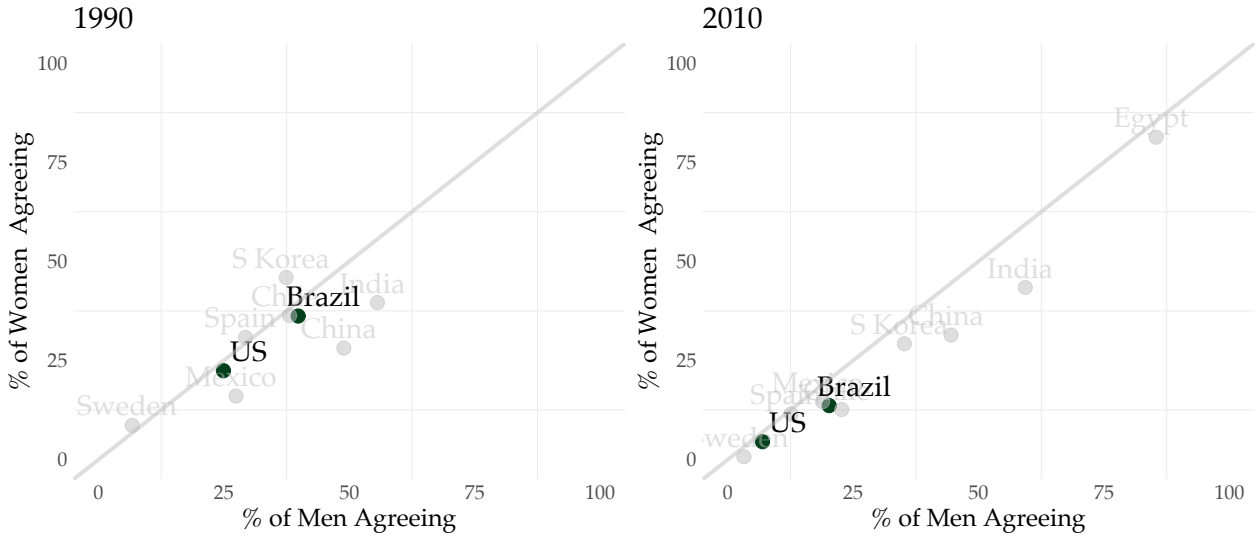
**TABLE 13:** Do Male Committee Members React to More Female Colleagues?

| | Scores from Female Committee Member | | | | Scores from Male Committee Member | | | |
|---|---|---|---|---|---|---|---|---|
| | $nw$ | $w(b)$ | $nw - w(b)$ | Final Score | $nw$ | $w(b)$ | $nw - w(b)$ | Final Score |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Female$_i \times$ | 0.026 | $-0.003$ | 0.008* | $-0.012$ | 0.073* | $-0.035$ | 0.139*** | $-0.010$ |
| %Female Evaluator$_c$ | (0.042) | (0.032) | (0.042) | (0.029) | (0.039) | (0.031) | (0.039) | (0.024) |
| Committee Member FE | X | X | X | X | X | X | X | X |
| Obs. | 60,504 | 60,504 | 60,504 | 60,504 | 60,504 | 60,504 | 60,504 | 60,504 |

***Notes:*** This table compares, from columns (1) through (4), how female committee members score female candidates depending on different levels of female composition in the hiring committee. Columns (5) through (8) perform the same exercise but with scores from male committee members. Standard errors are clustered at the job process level.

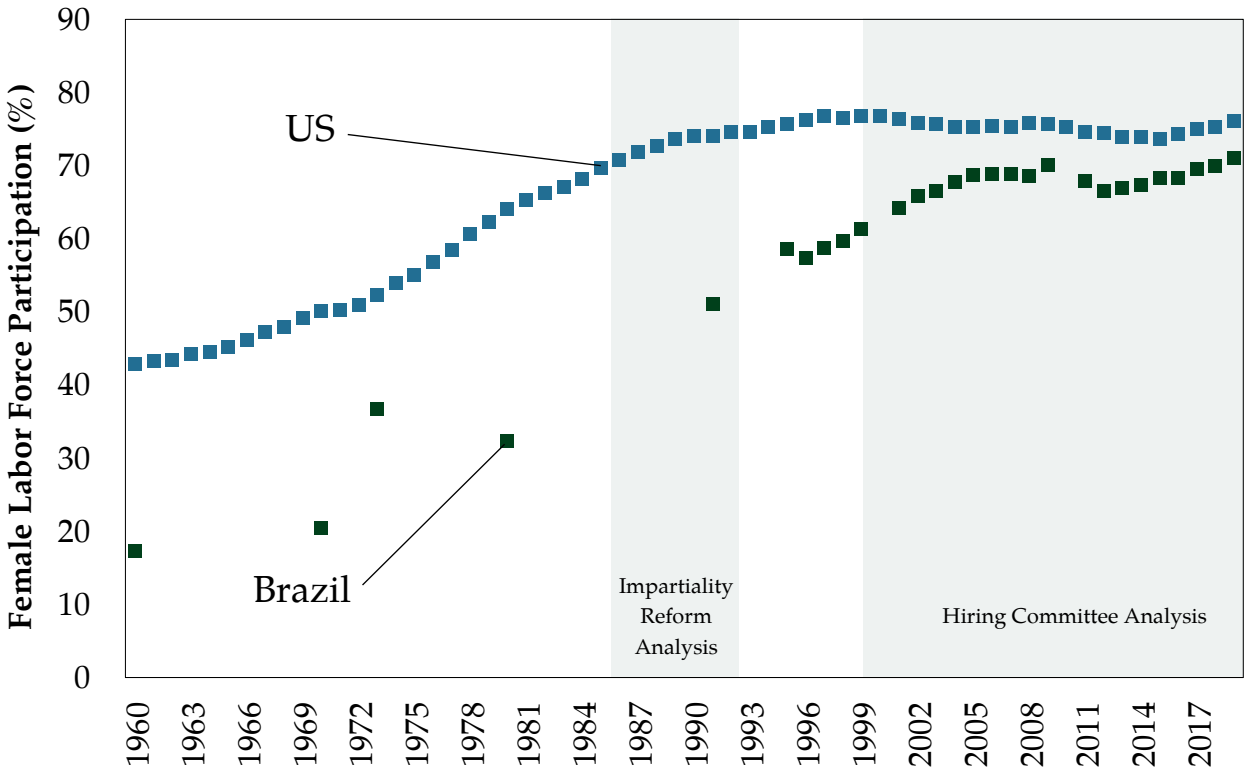
# APPENDIX

## A Appendix Tables and Figures



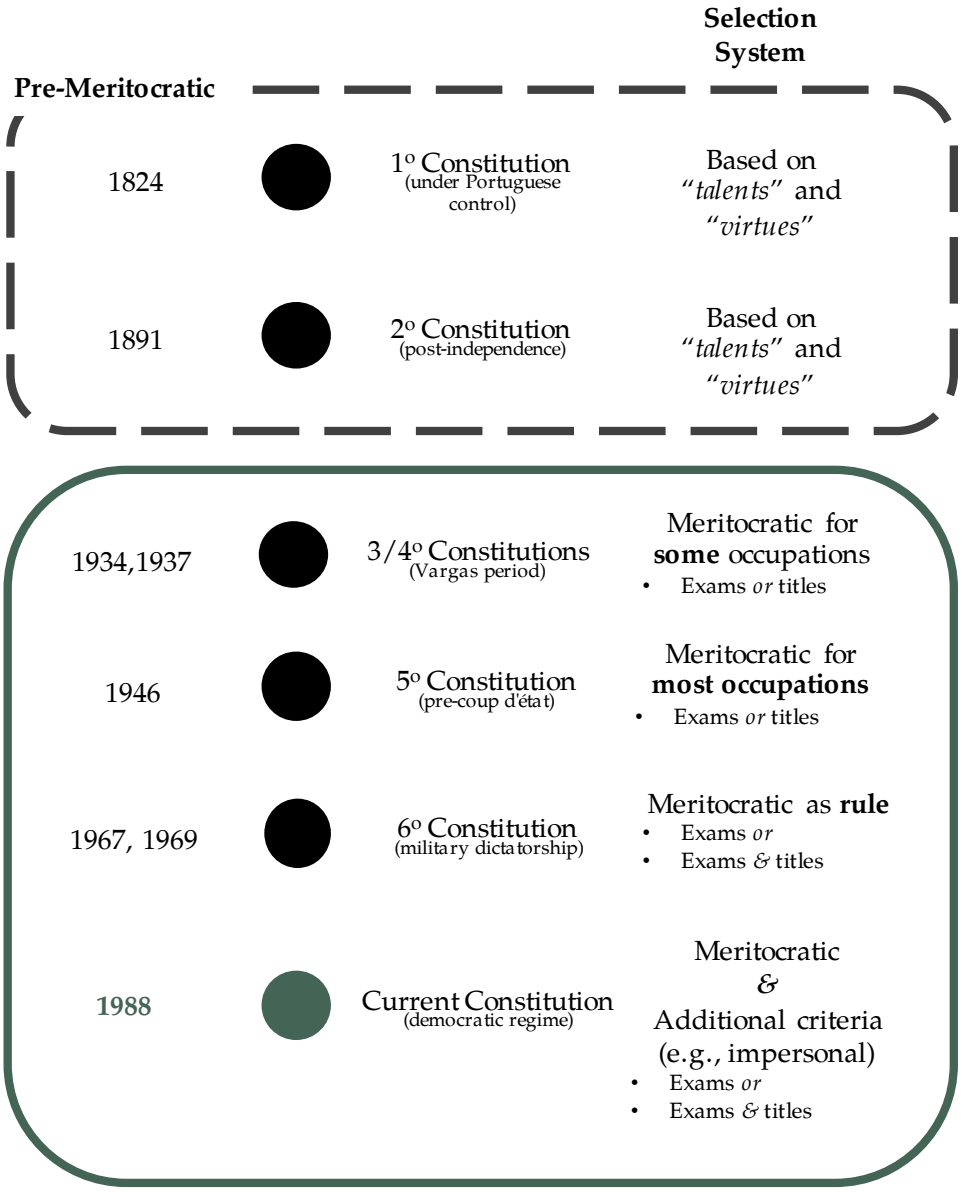

***Notes:*** International Value Survey (IVS) answers for the 1990-1994 and 2010-2014 waves of women and men agreeing with the statement "*when jobs are scarce, men have more of a right to a job than women*". Countries plotted: South Korea, China, India, Brazil, US, Mexico, Spain, Sweden, and Egypt (for 2010 only).

**FIGURE A.1:** Gender Attitudes Across Countries



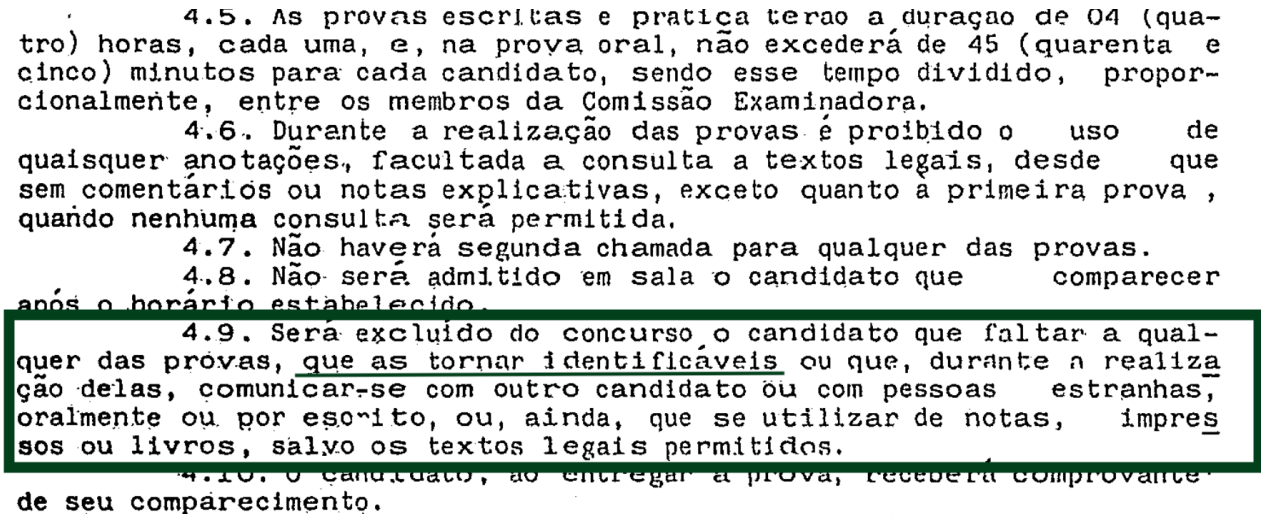

*Notes:* Female labor force participation rates (aged 25-54) for Brazil and the US. Shaded areas represent periods for different empirical analyses in the paper.

**FIGURE A.2:** Female labor force participation in Brazil and U.S.




***Notes:*** This figure shows the history of all changes to the selection process of public servants in Brazil, beginning from when the country was still under Portuguese domain, and spanning democratic and military control periods. Brazilian legal experts and historians consider the 1934 Constitution (amended in 1939) to establish meritocratic public servant selection — one of the first countries in Latin America. This early stage, however, provisioned the use of examinations or titles (resume) for some occupations. The 1946 Constitution expanded the selection criteria for most government jobs, until in 1967 and 1969 under military regime, the selection of every public servant through the legal device known as *Concurso* had to include at least one type of examination, ruling out the sole use of resumes. Despite the language, the definition of examination at that moment was fairly broad, so that interviews would be character or personality "exams", for example. In the end of 1988, Brazil passed a new Constitution which kept all public servant selection criteria from the previous Constitution but required public sector job processes to be conducted impartially. I exploit the introduction of this requirement as the main source of variation for part of the empirical analysis in the paper.

**FIGURE A.3:** History of Changes in the Selection of Public Servants in Brazil



4.5. As provas escritas e pratica terao a duraçao de 04 (quatro) horas, cada uma, e, na prova oral, não excederá de 45 (quarenta e cinco) minutos para cada candidato, sendo esse tempo dividido, proporcionalmente, entre os membros da Comissão Examinadora.

4.6. Durante a realização das provas é proibido o uso de quaisquer anotações, facultada a consulta a textos legais, desde que sem comentários ou notas explicativas, exceto quanto à primeira prova, quando nenhuma consulta será permitida.

4.7. Não haverá segunda chamada para qualquer das provas.

4.8. Não será admitido em sala o candidato que comparecer após o horário estabelecido.

4.9. Será excluído do concurso o candidato que faltar a qualquer das provas, que as tornar identificáveis ou que, durante a realização delas, comunicar-se com outro candidato ou com pessoas estranhas, oralmente ou por escrito, ou, ainda, que se utilizar de notas, impressos ou livros, salvo os textos legais permitidos.

4.10. O candidato, ao entregar a prova, receberá comprovante de seu comparecimento.

***Notes:*** Selection Process Rules for Hiring Federal Judges (Sep 4, 1989). Reads as: *"Candidates identifying themselves in any exam will be excluded from the hiring process."*

**FIGURE A.4:** Enforcing Blind Exams After Reform

**TABLE A.1:** Raw Text Data Availability: Government Official Gazettes

| Entity | Online Archives Available Since | Government Level |
|---|---|---|
| **Brazil** | **1808** | **Federal** |
| Rondônia | 2011 | State |
| Acre | 2010 | State |
| **Amazonas** | **1956** | **State** |
| Roraima | 1998 | State |
| Pará | 2016 | State |
| Amapá | 1988 | State |
| Tocantins | 2005 | State |
| Maranhão | 2001 | State |
| Piauí | 2004 | State |
| Ceará | 1999 | State |
| Rio Grande do Norte | — | State |
| Paraíba | 2003 | State |
| **Pernambuco** | **1936** | **State** |
| Alagoas | 2010 | State |
| Sergipe | 2012 | State |
| Bahia | 2007 | State |
| Minas Gerais | 2011 | State |
| Espírito Santo | 2006 | State |
| Rio de Janeiro | 2005 | State |
| **São Paulo** | **1891** | **State** |
| Paraná | 2004 | State |
| Santa Catarina | 2011 | State |
| **Rio Grande do Sul** | **1968** | **State** |
| **Mato Grosso do Sul** | **1979** | **State** |
| **Mato Grosso** | 1967 | State |
| Goiás | 2008 | State |
| **Distrito Federal** | **1967** | **State** |
| Porto Velho | 2007 | Municipality (State Capital) |
| Manaus | 2000 | Municipality (State Capital) |
| Rio Branco | | Municipality (State Capital) |
| Campo Grande | 1998 | Municipality (State Capital) |
| Macapá | 2018 | Municipality (State Capital) |
| Brasília | | Municipality (State Capital) |
| Boa Vista | 2010 | Municipality (State Capital) |
| Cuiabá | | Municipality (State Capital) |
| Palmas | 2001 | Municipality (State Capital) |
| **São Paulo** | **1975** | **Municipality (State Capital)** |
| **Teresina** | **1986** | **Municipality (State Capital)** |
| Rio de Janeiro | | Municipality (State Capital) |
| Belém | 2005 | Municipality (State Capital) |
| Goiânia | | Municipality (State Capital) |
| Salvador | | Municipality (State Capital) |
| Florianópolis | | Municipality (State Capital) |
| São Luís | | Municipality (State Capital) |
| Maceió | | Municipality (State Capital) |
| Porto Alegre | | Municipality (State Capital) |
| Curitiba | | Municipality (State Capital) |
| Belo Horizonte | | Municipality (State Capital) |
| Fortaleza | | Municipality (State Capital) |
| Recife | | Municipality (State Capital) |
| João Pessoa | | Municipality (State Capital) |
| Aracajú | | Municipality (State Capital) |
| Natal | | Municipality (State Capital) |
| Vitória | | Municipality (State Capital) |

***Notes.*** This table shows the primary sources of job hiring processes in various levels in Brazil's public sector. Each administrative level displayed publishes its own official gazette in a separate online repository. The middle column lists dates when online archives of each journal became available.

**TABLE A.2:** Do Male Committee Members React to More Female Colleagues?

| | Scores from Female Committee Member | | | | Scores from Male Committee Member | | | |
|---|---|---|---|---|---|---|---|---|
| | $nw$ | $w(b)$ | $nw - w(b)$ | Final Score | $nw$ | $w(b)$ | $nw - w(b)$ | Final Score |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Female$_i \times$ | $-0.095^{**}$ | $-0.038$ | $-0.068^{*}$ | $-0.010$ | $0.094^{***}$ | 0.006 | $0.108^{***}$ | 0.029 |
| %Female Evaluator$_c$ | (0.043) | (0.028) | (0.041) | (0.027) | (0.039) | (0.031) | (0.033) | (0.023) |
| Obs. | 60,504 | 60,504 | 60,504 | 60,504 | 60,504 | 60,504 | 60,504 | 60,504 |

***Notes:*** This table compares, from columns (1) through (4), how female committee members score female candidates depending on different levels of female composition in the hiring committee. Columns (5) through (8) perform the same exercise but with scores from male committee members. Standard errors are clustered at the job process level.

# B Theory Appendix

This appendix provides detailed derivations to the conceptual framework laid out in Section 5.

## B.1 Hiring Rates for $w$

Start with the case of selecting candidates based on a written test, which is allowed to be biased. The distribution of written signals is given by:

$$s^* = y + \nu_s(x) + \varepsilon_s,\ \varepsilon_s \sim N(0, 1/h_s)$$
$$s^* \sim N(y + \nu_s(x), 1/h_s)$$

where $s$ represents the unbiased signal $s = y + \varepsilon_s$, $h_s$ is the inverse of the variance of the written signal, measuring the precision of written testing and does not depend on group membership $x$. $\nu_s(x)$ represents disparate impact of the screening tool, which favors men when $\nu_s(m) > \nu_s(w)$.

Given the written signal, $s$, and the perceived group productivity, $\mu_0(x)$, the hiring manager updates her assessment of expected productivity of candidates according to:

$$y \mid_s \sim N(\mu(x,s), 1/(h_0 + h_s)).$$

Here, the updated degree of precision is $(h_0 + h_s)$ and the updated mean equals:

$$\mu(x,s) = s\frac{h_s}{h_0 + h_s} + \mu_0(x)\frac{h_0}{h_0 + h_s} + \nu_s(x).$$

The hiring decision that maximizes the evaluator's objective function satisfies the rule Hire $= I\{\mu(x,s) > k_s\}$, where $k_s$ is the threshold that yields a hiring rate of $K$. Plugging the expression for $\mu(x,s)$ into the hiring rule yield the following:

$$s > \frac{(h_0 + h_s)(k_s - \nu_s(x) - d_s\pi_j(x)) - h_0\mu_0(x)}{h_s}.$$

Since the distribution of $s$ is $N(\mu_0(x), 1/h_0 + 1/h_s)$, the above inequality can be rewritten as:

$$\frac{s - \mu_0(x)}{\frac{1}{h_0} + \frac{1}{h_s}} > \frac{(h_0 + h_s)(k_s - \nu_s(x) - d_s\pi_j(x) - \mu_0(x))}{h_s\sqrt{(\frac{1}{h_0} + \frac{1}{h_s})}}$$

which, can finally be expressed as:

$$\frac{s-\mu_0(x)}{\frac{1}{h_0}+\frac{1}{h_s}} > \underbrace{\frac{k_s-\nu_s(x)-\mu_0(x)-d_s\pi_j(x)}{\sigma_0\rho_s}}_{z_s^*(x)} \tag{4}$$

where $\rho_s \equiv Corr(\mu(x,s),y) = (1-\frac{h_0}{h_0+h_s})^{1/2}$ and $z_s^*(x)$ is the hiring threshold for group $x$ established by using written exams. The probability that an applicant from group $x$ is hired is $1-\Phi(z_s^*(x))$. same math for interview only

### B.2 Hiring Rates for $nw$

The hiring rate for group $x$ is obtained following the same steps in the previous case, observing the different distribution of non-written signals:

$$\eta^* = y + \nu_\eta(x) + \varepsilon_\eta,\ \varepsilon \sim N(0, 1/h_\eta)$$

where $\nu_\eta(x)$ represents the possible disparate impact of non-written tests and $\eta$ is the unbiased non-written signal: $\eta = y + \varepsilon_\eta$. Additionally, non-written tests also differ in the discretion allowed to evaluators, $d_\eta$. Since non-written screen tools, such as interviews or oral exams, are more subjective than written tests, it follows that the discretion given to managers is higher with non-written than written tests: $d_\eta > d_s$.

In this case, an applicant screened with a non-written exam is hired if

$$\frac{\eta-\mu_0(x)}{\frac{1}{h_0}+\frac{1}{h_\eta}} > \underbrace{\frac{k_\eta-\nu_\eta(x)-\mu_0(x)-d_\eta\pi_j(x)}{\sigma_0\rho_\eta}}_{z_\eta^*(x)} \tag{5}$$

The corresponding probability that a candidate from group $x$ is hired is given by $1-\Phi(z_\eta^*(x))$.

### B.3 Hiring Rates for $w+nw$

Given the two signals previously determined, $\eta^*$ and $s^*$, and the perceived group productivity, $\mu_0(x)$, the hiring manager updates her assessment of expected productivity according to:

$$y\mid_{\eta^*,s^*} \sim N(\mu(x,\eta^*,s^*), 1/(h_0+h_\eta+h_s)).$$

From the above, the updated degree of screening precision is $h_0 + h_\eta + h_s \equiv h_T$ and the updated posterior is:

$$\mu(x, \eta^*, s^*) = s\frac{h_s}{h_T} + \eta\frac{h_\eta}{h_T} + \mu_0(x)\frac{h_0}{h_T} + \nu_s(x)\frac{h_s}{h_T} + \nu_\eta(x)\frac{h_0 + h_\eta}{h_T}.$$

Thus, the hiring decision is is given by:

$$\mu(x, \eta, s) > k_T - \pi_j(x)(d_\eta + d_s)$$

$$\frac{h_s s + h_\eta \eta + h_0\mu_0(x)}{h_T} > k_T - \pi_j(x)(d_\eta + d_s) - \nu_s(x)\frac{h_s}{h_T} - \nu_\eta(x)\frac{(h_0 + h_s)}{h_T}.$$

Since $\eta = y + \varepsilon_\eta$, $s = y + \varepsilon_s$, and $y, \varepsilon_\eta, \varepsilon_s$ are independent, the left-hand side of the above inequality is distributed as:

$$\begin{aligned}
\frac{h_s s + h_\eta \eta + h_0\mu_0(x)}{h_T} &\sim N\left(\mu_0(x), \left(\frac{h_s}{h_T}\right)^2\left(\frac{1}{h_0} + \frac{1}{h_s}\right) + \left(\frac{h_\eta}{h_T}\right)^2\left(\frac{1}{h_0} + \frac{1}{h_\eta}\right) + 2\frac{h_s}{h_T}\frac{h_\eta}{h_T}\frac{1}{h_0}\right) \\
&\sim N\left(\mu_0(x), \frac{h_s^2 + h_\eta^2 + h_0^2 - h_0^2 + 2h_s h_\eta h_s h_0 + h_\eta h_0}{h_0 h_T^2}\right) \\
&\sim N\left(\mu_0(x), \frac{h_T - h_0}{h_0 h_T}\right) \\
&\sim N\left(\mu_0(x), \sigma_0^2\rho_T^2\right).
\end{aligned}$$

Further manipulation gives the final hiring threshold:

$$\frac{\mu(x, \eta, s) - \mu_0(x)}{\sigma_0\rho_T} > \underbrace{\frac{k_T - \frac{h_s}{h_T}\nu_s(x) - \frac{h_0 + h_\eta}{h_T}\nu_\eta(x) - \pi_j(x)(d_\eta + d_s) - \mu_0(x)}{\sigma_0\rho_T}}_{z_T^*(x)}. \tag{6}$$

### B.4 Hiring Rate for $w(b)$

A blind written exam provides the following signal:

$$b^* = y + \nu_s(x) + \varepsilon_s, \;\; \varepsilon \sim N(0, 1/h_s)$$

with the same screening precision $h_s$ and the same disparate impact $\nu_s(x)$ as the written test. Because discretion is entirely removed after blinding, evaluators rely on the *population* produc-

tivity for updating, $\mu_0 = \frac{\mu_0(x)+\mu_0(y)}{2}$, since group membership is not identifiable,

$$\mu(x, b^*) = s\frac{h_s}{h_0 + h_s} + \frac{\mu_0(x) + \mu_0(y)}{2}\frac{h_0}{h_0 + h_s} + \nu_s(x).$$

Manipulating the above gives the hiring threshold for group $x$:

$$\frac{s - \mu_0(x)}{\frac{1}{h_0} + \frac{1}{h_s}} > \frac{(h_0 + h_s)(k_b - \nu_s(x) - \mu_0(x))}{h_s\sqrt{(\frac{1}{h_0} + \frac{1}{h_s})}} - \frac{h_0(\mu_0(y) - \mu_0(x))}{2h_s\sqrt{(\frac{1}{h_0} + \frac{1}{h_s})}}$$

and finally,

$$\frac{s - \mu_0(x)}{\frac{1}{h_0} + \frac{1}{h_s}} > \underbrace{\frac{k_b - \nu_s(x) - \mu_0(x)}{\sigma_0\rho_s} - \frac{h_0\rho_s}{2h_s\sigma_0}(\mu_0(y) - \mu_0(x))}_{z_b^*(x)}. \tag{7}$$

### B.5 Hiring Rate for $w(b) + nw$

Finally, the second type of screening combination post-reform includes blind written and non-written exams. Given the two signals, $\eta^*$ and $b^*$, the posterior is:

$$y \mid_{\eta^*, b^*} \sim N(\mu(x, \eta^*, b^*), 1/h_T)$$

and the updated mean

$$\mu(x, \eta^*, b^*) = \frac{h_s s + h_\eta \eta + h_0\mu_0(x) + \nu_s(x)h_s + \nu_\eta(x)(h_0 + h_\eta)}{h_T}.$$

Since $\eta$ and $s$ can be rewritten as $\eta = y + \varepsilon_\eta$ and $s = y + \varepsilon_s$, and $y, \varepsilon_s, \varepsilon_\eta$ are independent, it follows that:

$$\mu(x, \eta, b) \equiv \frac{h_s s + h_\eta \eta + h_0\mu_0(x)}{h_T} \sim N\left(\mu_0(x), \sigma_0^2\rho_T^2\right)$$

The hiring decision can then be rewritten as:

$$\frac{h_s s + h_\eta \eta + h_0\mu_0(x)}{h_T} > k_{\eta b} - \nu_s(x)\frac{h_s}{h_T} - \nu_\eta(x)\frac{(h_0 + h_\eta)}{h_T} - d_\eta\pi_j(x)$$

$$\frac{\mu(x,\eta,b)-\mu_0(x)}{\sigma_0\rho_T} > \underbrace{\frac{k_{\eta b}-\nu_s(x)\frac{h_s}{h_T}-\nu_\eta(x)\frac{h_\eta+h_0}{h_T}-d_\eta\pi_j(x)-\mu_0(x)}{\sigma_0\rho_T}}_{z^*_{\eta b}(x)}. \tag{8}$$

### B.6 Change in Hiring Rate for $w \longrightarrow w(b)$

Without loss of generality, assume that female candidates are less productive on average than men: $\mu_0(f) < \mu_0(m)$, or in other words, that women are the minority group. By blinding the written exam, how do screening thresholds $z^*_s(x)$ and $z^*_b(x)$ compare and thus how are hiring rates affected? By inspecting expressions (4) and (7) and considering that written tests — whether blind or not — have the same screening precision and disparate impact, since they are otherwise identical save for hiding a candidate's identity, women face a lower hiring threshold in the blinded exam, $z^*_s(f) > z^*_b(m)$, if and only if

$$d_s\pi_j(f) < \frac{h_0\rho_s^2}{2h_s}(\mu_0(m)-\mu_0(f)).$$

The expression above captures the following intuition. As long as the evaluator favors male candidates, either through statistical discrimination or evaluator bias, blinding the written exam increases hiring rates for women. The right-hand side is always positive since $\mu_0(f) < \mu_0(m)$, and it represents the improvement in women's hiring odds from removal of the ability to statistically discriminate. Therefore, if the left-hand side — which captures evaluator bias — is negative, i.e., if hiring managers favor men, or if it is sufficiently small due to either low discretion or low bias, then the hiring rate for women increases and the hiring rate for men decreases after blinding the written exam. Alternatively, if an evaluator is biased in favor of women, blinding the exam curbs the evaluator's ability to balance women's penalty from statistical discrimination with personal bias, potentially decreasing the female hiring rate.

### B.7 Change in Hiring Rate for $nw \longrightarrow w(b)$

I now analyze the potential change induced by the policy that most dramatically alters the mix of screening tools. To build intuition, consider an employer that solely relies on interviews to screen candidates. From the expression in (5), the disparate impact of interviews, their precision, and how much they enable evaluator bias to be expressed all determine an applicant's hiring odds. Only in terms of evaluator bias, under the assumption that interviews offer more discretion than written exams, this pre-policy state contains the highest expression

of evaluator bias. In contrast, as discussed before, screening solely based on written exams is likely to provide a setting with low disparate treatment.

Assume $h_s = h_\eta$ and $\mu_s = \mu_\eta$. It follows that the hiring threshold for men is higher with the blind-written signal than with the non-written signal, $z^*_\eta(m) < z^*_b(m)$, as long as evaluators favor men $\pi_j(m) > 0$ or, alternatively, if the following is satisfied

$$\frac{d_\eta \pi_j(m) + (k_b - k_\eta)}{\sigma_0 \rho} > \frac{h_0 \rho}{2 h_s \sigma_0}(\mu_0(f) - \mu_0(m)),$$

which allows for sufficiently small evaluator bias toward women. Because the above inequality implies a higher threshold for hiring male candidates, it increases selectivity for men, and, given a constant total hiring rate, $K$, the gender hiring gap decreases.

Next, conduct the same exercise but now allow for written and non-written exams to have different disparate impacts, $\Delta\nu_s \neq \Delta\nu_\eta$, where $\Delta\nu_s = \nu_s(m) - \nu_s(f)$. In this case, changing from non-written screening stages to blind-written exams increases female hiring rates if and only if:

$$\frac{h_0 \rho}{h_s \sigma_0}(\mu_0(f) - \mu_0(m)) < \frac{d_\eta(\pi_j(m) - \pi_j(f)) + (\Delta\nu_\eta - \Delta\nu_s)}{\sigma_0 \rho} \tag{9}$$

Note that the left-hand side of the expression above is negative, so that if evaluators are men-favoring and interviews have a larger disparate impact than written exams, the inequality is satisfied and female hiring rates increase. In other words, if the principal substitutes a hiring tool for one that has a smaller disparate impact and eliminates discretion, the change will raise hiring rates of the minority group. More generally, if either evaluator bias favors men, or if the relative bias of non-written tests is lower than that of written tests, it can still increase female hiring as long as it satisfies the inequality above. Another way to interpret the inequality (9) is to rewrite it as

$$\frac{h_0 \rho}{h_s \sigma_0}\mu_0(f) + \frac{d_\eta \pi_j(f)}{\sigma_0 \rho} + \frac{(\nu_\eta(f) - \nu_s(f))}{\sigma_0 \rho} < \frac{h_0 \rho}{h_s \sigma_0}\mu_0(m) + \frac{d_\eta \pi_j(m)}{\sigma_0 \rho} + \frac{(\nu_\eta(m) - \nu_s(m))}{\sigma_0 \rho} \tag{10}$$

The left-hand side represents the perceived productivity of female applicants, equal to true productivity plus bias, either from the evaluator or screening tool. The right-hand side represents the perceived productivity of male applicants. Thus, if female applicants are perceived as less productive under non-written screening relative to written screening, then the transition increases their hiring rate.

Finally, relax the assumption of identical screening precisions. If written tests are more precise, $h_s > h_\eta$, switching from non-written screening to written testing raises the hiring rate

of the group with lower perceived productivity, that is, it raises the female hiring rate if (10) holds. However, if interviews have higher precision, $h_s < h_\eta$, the transition from interviews to written test decreases screening precision and leads to higher hiring rates of the favored group, men. The net effect then depends on the losses from decreased screening precision relative to the gains from lower bias if (10) is satisfied.

### B.8 Change in Hiring Rate for $nw \longrightarrow w(b) + nw$

This case maintains the use of non-written exams but, to comply with the impartiality policy, the employer adds a blind-written exam to the hiring process. By having an additional evaluation tool, total hiring precision increases, $h_0 + h_\eta + h_s > h_0 + h_\eta$, without introducing disparate treatment, since $d_b = 0$. Adding the blind-written tool reduces the weight that discretion in the non-written test plays in determining hiring rates (recall that $\frac{d_\eta \pi_j(x)}{\sigma_0 \rho_T} < \frac{d_\eta \pi_j(x)}{\sigma_0 \rho_\eta}$). However, introducing a different screening tool potentially incorporates that tool's disparate impact.

To start assume that screening tools do not favor any group, that is $\nu_\eta(f) = \nu_\eta(m)$, $\nu_s(f) = \nu_s(m)$, and that $\nu_\eta = \nu_s$. Then,

$$z^*_{\eta b}(x) = \frac{k_{\eta b} - \nu(x) - \mu_0(x) - d_\eta \pi_j(x)}{\sigma_0 \rho_T} < \frac{k_\eta - \nu(x) - \mu_0(x) - d_\eta \pi_j(x)}{\sigma_0 \rho_\eta} = z^*_\eta$$

That is, the hiring threshold is lower for group $f$ if $\mu_0(f) + d_\eta \pi_j(f) < \mu_0(m) + d_\eta \pi_j(m)$ — women have lower perceived productivity. For the minority group both effects help as long as the same condition holds: $\mu_0(f) + d_\eta \pi_j(f) < \mu_0(m) + d_\eta \pi_j(m)$.

The increase in screening precision and decrease in relative importance of evaluator bias increases women's hiring rates if they are the group with the lower perceived productivity: $\mu_0(f) + d_\eta \pi_j(f) < \mu_0(m) + d_\eta \pi_j(m)$, reflecting that the change in hiring probability with respect to screening precision is:

$$\frac{\partial \left[1 - \Phi(z^*_{\eta b}(x))\right]}{\partial \rho_T} = \phi(z^*_{\eta b}(x)) \left[\frac{z^*_{\eta b}(x)}{\rho_T} - \frac{\partial k_{\eta b} / \partial \rho_T}{\sigma_0 \rho_T}\right] > 0 \tag{11}$$

Here $\phi(\cdot) > 0$ and $z^*_{\eta b}(m) < z^*_{\eta b}(f)$ if the above inequality of women being perceived as the group with lower productivity is satisfied.

Now, allow for screening tool bias to differ between written and non-written tests and to favor one group, $\nu_\eta(f) \neq \nu_\eta(m)$. Then, women benefit from the added precision if the

following inequality holds:

$$\mu_0(f) + d_\eta \pi_j(f) + \nu_s(f)\frac{h_s}{h_T} + \nu_\eta(f)\frac{h_0 + h_\eta}{h_T} < \mu_0(m) + d_\eta \pi_j(m) + \nu_s(m)\frac{h_s}{h_T} + \nu_\eta(m)\frac{h_0 + h_\eta}{h_T}$$

If the written test that is added is bias increasing, $|\Delta\nu_s| > |\Delta\nu_\eta|$, it causes excess hiring of the group that is favored by the bias. Then, if the bias favors men, $\nu_s(m) - \nu_s(f) \equiv \Delta\nu_s > \Delta\nu_\eta \equiv \nu_\eta(m) - \nu_\eta(f)$, the net effect on the female hiring rate depends on the gains from increased screening precision relative to the losses from increased bias. On the other hand, if the bias favors women, and written tests are more biased than interviews, it leads unambiguously to higher hiring rates of women since all three forces have a positive effect.

### B.9 Change in Hiring Rate for $w + nw \longrightarrow w(b) + nw$

Removing the non-written signal from a screening mix of written and non-written decreases total screening precision, $h_0 + h_s < h_0 + h_s + h_\eta$, removes evaluator bias within the non-written test, $d_\eta \pi_j(x)$, and removes the non-written screening tool bias, $\nu_\eta(x)$. In addition, blinding the written test removes evaluator bias within the exam, $d_s \pi_j(x)$, as well as the use of group means (statistical discrimination) in determining the evaluator's posterior.

To begin with, assume $\nu_s = \nu_\eta$, which does not however eliminate the effect of removing the non-written screening tool bias, but just assumes that the type of tool bias reduced is the same in magnitude and sign (favors the same group), as the bias characterizing the written test.

Removing both screening tool and evaluator biases raises selectivity of the favored group and reduces selectivity of the non-favored group: $z_T^*(m) < z_b^*(m)$. Thus

$$\left[\frac{k_T - \nu(m) - \mu_0(m)}{\sigma_0 \rho_T} - \frac{k_b - \nu(m) - \mu_0(m)}{\sigma_0 \rho_s}\right] - \frac{\pi_j(m)(d_\eta + d_s)}{\sigma_0 \rho_T} + \frac{h_0 \rho_s}{2 h_s \sigma_0}(\mu_0(f) - \mu_0(m)) < 0$$

where the inequality holds for $m$ if this is the favored group. Thus, removing the non-written tool and evaluator bias, as well as evaluator bias within the written screening tool reduces selectivity of women and thus raises women hiring rates if they are the non-favored group.

However, the decrease in screening precision due to removal of the non-written signal has the opposite effect on hiring rates of the non-favored group:

$$\gamma_f \equiv \frac{\partial\left[1 - \Phi(z_T^*(f))\right]}{\partial \rho_T} = \phi(z_T^*(f))\left[\frac{z_T^*(f)}{\rho_T} - \frac{\partial k / \partial \rho_T}{\sigma_0 \rho_T}\right]$$

with $\rho_T$ decreasing as $\rho_S < \rho_T$, $\phi(\cdot) > 0$, and $z_b^*(m) < z_b^*(f)$ if:

$$\frac{\mu_0(f) + \nu_s(f) - (\mu_0(m) + \nu_s(m))}{\sigma_0 \rho_s} + \frac{h_0 \rho_s}{h_s \sigma_0}(\mu_0(m) - \mu_0(f)) < 0$$

This later inequality holds if $\mu_0(f) + \nu_s(f) < \mu_0(m) + \nu_s(m)$ (men are the favored group, perceived to have higher productivity). Note that the inequality of men having the higher perceived productivity can hold even if the written test favors women, $\nu_s(f) > \nu_(m)$, if it is small enough: $\nu_s(f) - \nu_s(m) < \mu_0(m) - \mu_0(f)$. So with $\rho$ decreasing, $\gamma_f < 0$ and $\gamma_m > 0$ if men are the favored group. Consequently, the net effect depends on the positive effect on female hiring rates from decreased bias relative to the negative effect from decreased screening precision.

Third, removing the non-written tool also eliminates its bias, $\nu_\eta$, which affects hiring rates depending on whether the bias favored men or women, and how it compares to the bias in the written tool. Consider the following cases.

Fist, suppose that the written tool favors women, $\Delta\nu_s < 0$, while the non-written favors men, $\Delta\nu_\eta > 0$, where $\Delta\nu_\theta = \nu_\theta(m) - \nu_\theta(f)$. Then, removing the non-written signal is bias-reducing and reduces excess hiring of the group favored by the non-written bias — men — increasing selectivity for the group and increasing the hiring rate for women. More formally, this follows from:

$$\begin{aligned}
(z_T^*(m) - z_b^*(m)) - (z_T^*(f) - z_b^*(f)) &< 0 \\
\frac{h_s\rho_s - h_T\rho_T}{\sigma_0\rho_s\rho_T h_T}(\nu_s(f) - \nu_s(m)) + \frac{h_0 + h_\eta}{\sigma_0\rho_T h_T}(\nu_\eta(f) - \nu_\eta(m)) &< 0 \\
(\nu_\eta(f) - \nu_\eta(m)) &< \frac{h_T\rho_T - h_s\rho_s}{(h_0 + h_\eta)\rho_s}(\nu_s(f) - \nu_s(m))
\end{aligned}$$

where the fraction term is positive from $h_T = h_0 + h_\eta + h_S > h_s$. It follows that the right-hand side is also positive and the left-hand side is negative. This implies an increase in women's hiring rates.

Second, if instead the written signal favors men $\Delta\nu_s > 0$, while the non-written favors women, $\Delta\nu_\eta < 0$, then using the same inequality, it follows that removing the women-favoring bias from non-written increases women's selectivity, decreasing their hiring rate.

Third, if both the written and non-written tools favor men, $\Delta\nu_s > 0, \Delta\nu_\eta > 0$, then, regardless of which bias is larger, removing the non-written signal is bias-reducing and thus reduces excess hiring of the group favored by the bias, men, which in turn increases hiring rate for women. If, instead, both tools favor women, $\Delta\nu_s < 0, \Delta\nu_\eta < 0$, then, similarly, the transition is bias-reducing and decreases excess hiring of the favored group, which in this case are women. This increases selectivity for women, which decreases their hiring rate.