Personal Connections and Hiring Decisions in the Public Sector*

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Abstract

Relying on comprehensive employment records of applicants in the universe of public jobs in Brazil from 1986 until 2017, an institutional reform that reduced discretion in hiring, and novel performance metrics for public administration, we provide the first evidence of how personal connections influence recruitment processes. We find that a reduction in the ability to leverage personal connections affects both the job search behavior of connected individuals and employers' hiring decisions. Moreover, hires after the reform are of higher quality, have lower separation rates and absenteeism, and are more likely to be promoted. We also show that the decrease in connected hires improves the operational and budgetary efficiency of public sector establishments. These results carry significance in enhancing our understanding of how to bolster state capacity.

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1 Introduction

Hiring decisions play a pivotal role in the success of any organization. Although personal connections are often portrayed as influential factors, their actual value to job seekers and employers and how they operate are a subject of controversy. The lack of definitive evidence stems from three key measurement challenges. First, using connections may measure unobservable characteristics correlated with performance, making it difficult to establish a causal effect on labor market performance. Second, only realized matches are observable. The absence of information on the pool of applicants and their connections makes it challenging to discern whether connections impact job search behavior, hiring decisions, or both. Third, to understand whether connections give access to better or worse jobs, one would need knowledge of wages in the absence of connections. Empirically, this is not feasible, leaving this question unanswered.

In this paper, we investigate the causal effects and operational intricacies of personal connections on hiring decisions and productivity consequences, focusing on recruitment processes within the crucial domain of the public sector. Hirings decisions are especially important in the public sector since the quality of employees play a pivotal role in determining government performance.

Our study leverages comprehensive data encompassing both selected and unselected candidates across the entire spectrum of public sector positions in Brazil. We merge this data with administrative employer-employee records spanning from 1985 to 2017. Consequently, we gain detailed insights into job applications and labor market outcomes before and after each recruitment process for both successful and unsuccessful applicants. Our preferred definition of personal connections is as follows. Because the data on labor market outcomes contains information about employers at the establishment level, we define as connected applicants who, at the moment of the recruitment process, worked in the past in the same establishment as at least one employee of the recruiting establishment.

The Brazilian context offers a unique opportunity for causal assessment due to a constitutional reform in 1988 that reduced the level of discretion in public sector hiring processes —a natural experiment concerning the use of personal connections. For example, written tests had to be implemented, keeping the identity of the applicant concealed, which, when combined with the rigid set of rules in screening, effectively meant connected applicants could no longer be identified until job offers were made.

Our data and institutional set-up allows us to address the measurement challenges outlined earlier. First, the reform substantially reduced the ability to use personal connections unrelated to observable and unobservable employee and employer characteristics. Second, by scrutiniz-

ing detailed applicant pool characteristics, we discern the effects of personal connections in disseminating job information versus facilitating access conditional on application. Third, while we cannot measure realized wages in the absence of connections, we come as close as possible by examining wages before and after the reform for individuals in the same establishment and occupation, including connected applicants not hired in the same recruitment process who secured alternative employment. Additionally, comparing the quality of hires before and after the reform helps understand whether personal connections provide screening information or exhibit favoritism.

Our empirical approach employs a traditional difference-in-differences strategy, comparing changes in job search behavior and employer hiring decisions before and after the reform in federal recruitment processes with those in state recruitment processes. This comparison is restricted to jobs in the same establishment and occupation, narrowly defined, while accounting for time trends. The assumption of parallel trends, that is outcomes in state and federal recruitment processes would have followed similar trends over time in the absence of the treatment holds notably true in our setting.

The findings reveal that the decrease in the ability to use connections changes the structure of the job search market along several margins. Following the new constitutional requirements, individuals become 21% less likely to apply to jobs at establishments where they have connections, connected individuals are more likely to increase the total number of applications compared to unconnected job seekers, and are more likely to apply to jobs in occupations similar to their past employment. The decrease in discretion also affected employers' hiring decisions, decreasing the probability that a connected applicant is hired relative to an unconnected candidate by 17% (for a pre-reform baseline gap of 1.2 percentage points). We show that the results are not driven by alternative mechanisms related to an individual's employment history or demand for a worker's skills. When looking at the effect of connections on wages, we find that when connections cannot be used, wages of connected workers relative to unconnected fall, both for those who are hired and for those who are not.

After establishing that connections are giving access to higher-paid public sector jobs, by affecting the sorting and selection of workers, the next question we ask is whether that increases or decreases productivity. On the one hand, connections could offer private screening information to employers, which would improve the selection and increase the productivity of hires. On the other hand, connections could merely facilitate favoritism, which would be detrimental to productivity. In order to disentangle between the two mechanisms, we construct several measures of individual quality first, based on previous private sector wages, and second, based on on-the-job outcomes. In terms of both observable and unobservable measures of quality based on workers' previous wages, the quality of connected relative to unconnected

job applicants and hires increases following the reform. More specifically, while the average quality of unconnected individuals remains roughly constant before and after the reform, the quality of connected individuals increases dramatically. This is true for both applicants and hires. Next, we analyze the quality of hires by evaluating their on-the-job performance. The results reveal that following the inability to leverage connections, the quality of hires improved across all the aspects examined: hires are less likely to separate, show decreased absenteeism and suspension rates, and an increased likelihood of receiving promotions.

In the last part of the part, we seek to determine the consequences of the use of personal connections in hiring on employer productivity. The challenge here is that measuring the performance of government agencies is notoriously challenging due to the lack of comparable data (Best et al. (2023a)). We overcome this challenge by scraping additional data from government gazettes on several measures of budget efficiency. First, we measure budgetary shortfalls that may arise due to either poor planning, poor operational execution, or poor expenditure quality. Second, we measure the quality of expenditure allocation along three dimensions: the share of spending that is mandatory, the share of the budget allocated to investment, and the share of non-wage expenditures and investment in core activities. Utilizing this auxiliary data alongside the institutional reform as an instrument for the share of new unconnected hires, we contribute, for the first time, evidence of the causal effect of personal connections on the productivity of public administration. The 2SLS results reveal that increasing the share of unconnected hires reduces budget shortfalls and improves quality-adjusted spending, mainly geared toward core activities.

Overall, our findings suggest that discretion in public servant hiring distorts the hiring top-tier workers in favor of individuals with connections, aligning with utilizing connections for preferential treatment. This, in turn, has negative consequences for state capacity.

Our paper is related to the vast literature leveraging the information available in matched employer-employee data or large administrative datasets containing work history to provide insights into the effects of referrals on individual labor market outcomes.[1] Our analysis stands out from existing research in at least two key aspects. Firstly, we introduce exogenous variation in the utilization of personal connections, a novel approach within this field. Secondly, while previous studies assume that connections alleviate job search frictions by disseminating information about job opportunities, our study is unique in providing evidence on the actual role of connections in the job search process. Many existing studies rely on survey data where workers report how they found their current job, revealing that a significant proportion of jobs

[1] In particular, this literature documents that individuals are more likely to work together with former coworkers (Hensvik and Skans (2016), Caldwell and Harmon (2019), Cingano and Rosolia (2012)), same ethnicity peers (Dustmann et al. (2016)), neighbors (Bayer et al. (2008), Schmutte (2015)), and family members (Kramarz and Skans (2014)).

worldwide (between 25% and 65%) are secured through friends or relatives.[2] However, little is known about the specific contribution of these connections to finding and obtaining a job. Our ability to observe the entire search history and career trajectory of applicants, hires, and unsuccessful applicants for each job enables us to address this gap and demonstrate the implications of connections on job search behavior.[3]

Our analysis is different from prior research on referrals as it shifts its focus from the private sector to the examination of the public sector. This distinction is crucial in understanding why our findings diverge from many existing studies. In contrast to private sector recruitment processes, where personal connections can expedite hiring searches and save resources, public sector recruitment processes are bound by strict legal regulations, prohibiting any shortcuts in the recruitment procedures. In addition, employees in the public sector operate under different incentives compared to those in the private sector. Given that public sector wages are typically fixed and less contingent on individual performance, employees have less motivation to refer high-quality candidates compared to their counterparts in the private sector. Our results align with the handful of experimental studies in this literature (Beaman and Magruder (2012), Bandiera et al. (2009)), which indicate that individuals are more inclined to favor superior candidates when incentivized through monetary rewards.[4]

Our paper integrates two previously distinct bodies of literature. One is the extensive research on referrals as discussed above, while the other delves into understanding the selection of public servants. Despite growing interest in how bureaucratic effectiveness contributes to development (Dal Bó et al. (2013), Finan et al. (2017), Besley et al. (2022), Best et al. (2023b)), the latter literature has remained relatively small. It has primarily investigated the influence of financial and career incentives on the applicant pools for public service positions through Randomized Controlled Trials (RCTs) (Dal Bó et al. (2013), Deserranno (2019), Ashraf et al. (2020)). Our contribution to this field lies in revealing another mechanism affecting the selection of civil servants: the use of personal connections. In addition, we present novel findings on how this practice impacts the caliber of public servants and, consequently, state capacity.

Existing studies on the impact of personal connections in public organizations have predominantly focused on the effects of political connections and patronage and have found that

[2] See Ioannides and Loury (2004), Topa (2011), and Hoffman (2017) for surveys of this literature.

[3] This also distinguishes our work from previous field experiments (Pallais and Sands (2016), Friebel et al. (2023)) that have focused on attrition and productivity impacts of referrals.

[4] Mocanu (2022) studies the effect of different changes in screening practices on gender diversity using the same constitutional shock as in our paper, with the goal of identifying more broadly which combinations of hiring tools are effective in improving female representation of applicants and hires. This relates to the literature that has studied the effects of blinding on gender gaps in different settings (Blank (1991), Goldin and Rouse (2000), Card et al. (2020)). We focus on the fact that blinding curbs the use of connections, which are usually not observed in these other settings.

it results in favoritism for private gain (Xu (2018), Colonnelli et al. (2020), Voth and Xu (2020), Aneja and Xu (2023), Moreira and Pérez (2021), Jia et al. (2015)).[5] We go beyond this focus by examining the effect of coworker networks in the allocation of public sector jobs, as well as productivity implications at both the individual and employer levels. There are three key distinctions of our study from the existing literature on the effects of political connections mentioned above. First, political connections within the context of patronage entail political interference on civil servants' careers beyond just the hiring stage.[6] In contrast, we isolate the role of personal connections in hiring in a meritocracy. Second, unlike political connections, which may have clearer incentives for engaging in political favoritism, we focus on coworker networks, which are more likely to possess and provide information about a candidate's productivity. Yet, we find evidence against this notion. Third, we introduce an essential component to studying the selection of public servants that is absent in this literature: the pool of applicants. This novel information enables us to disentangle between the selection and sorting effects.

Finally, our research contributes to the literature on search and matching (Wright et al. (2021), DellaVigna et al. (2022), Faberman et al. (2022)) by demonstrating how personal connections alter the dynamics of the job search market and the returns to search effort. The study of job search behavior and its outcomes is fundamental to understanding wage determination and employment patterns. While existing literature has primarily focused on the job search behavior of the unemployed and the impact of informational interventions or search assistance, often without directly observing search outcomes (Belot et al. (2019), Marinescu and Skandalis (2021), Le Barbanchon et al. (2021)), our approach extends beyond by examining not only job applications and their results but also the influence of personal connections. We are the first to uncover that personal connections decrease search efforts while enhancing the likelihood of securing employment, thereby improving the efficiency of labor market matching.

This paper is organized as follows. Section 2 details the institutional setting of Brazil's public sector, how public servants are selected, and the reform we exploit as an exogenous reduction to the use of connections in hiring. Section 3 describes the data assembly process, which combines a novel public sector applicant-hire rich dataset with administrative records from the Brazilian formal sector. Section 4 presents the set of results on the sorting and selection of public servants. Section 5 provides estimates to the value of personal connections by conducting several exercises comparing earnings of connected and unconnected applicants. Section 6 provides results for individual-level on-the-job outcomes and quality of hires and ap-

[5]The question of discretion in hiring has also been studied in the private sector, in terms of effects of job testing (Hoffman et al. (2018)). We focus on the fact that discretion affects the degree to which individuals can leverage their connections.

[6]These discretionary jobs constitute approximately 20% of the positions in our dataset, which we exclude from our analysis.

plicants, revealing whether connections provided screening information or favoritism. Section 7 estimates establishment-level productivity effects. Section 8 concludes.

2 Institutional Context

2.1 Public Servant Selection

Most public sector positions in Brazil are allocated through a mandatory competitive-based selection system known as *concurso público*. Across federal and state governments, over 75% of positions are filled through this meritocratic hiring device.[7] Brazilian law requires the entire *concurso* selection process — from job announcement to enrolled candidates to scores in each screening stage and offers — to be made publicly available in gazettes published daily by each government entity.

This hiring process is highly structured. Every job process officially begins when its job announcement (*edital*) is published. This rigid, highly axiomatic set of legally-binding rules and instructions will guide the entire job process, hiring decisions, and fulfillment of openings. Job announcements describe, for example, the position details, including wage and number of openings, application requirements, and the types of screening tools and stages (blind test, interview, practical exams, resume analysis). Evaluators or hiring managers in charge of screening must follow all instructions. Unlike private sector jobs, job openings cannot be closed earlier than advertised. A constitutional requirement that selection processes must guarantee equal access to every citizen who qualifies for that position (e.g., has the required schooling) prevents job announcements with unreasonably short timeframes for applications and any shortening to be made once rules are published.[8]

Job offers are made following the final ranking based on the screening stages. Hiring steps and their relative weights toward the aggregate score cannot be ignored or modified. All in all, any deviation from the instructions in the *edital* are subject to immediate judicial action, leading to suspension or annulment of the job process. Scores are published individually for each job applicant in each screening step. The wage is posted, so every candidate hired for the same position receives the same wage and the same pecuniary and non-pecuniary benefits dictated by the job title's career plan. Public sector servants are awarded life tenure after a brief

[7] The remaining jobs available are exempt from civil service exams and break down into commissioned posts (4%) and positions of trust (12%), which are primarily politically appointed, and temporary jobs (8%). Colonnelli et al. (2020) study the role of political patronage in Brazilian municipal governments (rather than federal or state) with a focus on the subset of workers hired outside the formal civil service selection system.

[8] Extensions are accepted, however, and usually there are at a minimum 30 days for candidates to apply, with lengthier application windows ranging from 2-3 months.

2-year probation period after hiring (which 98% of hires successfully pass). We provide more on-the-job institutional details later in the paper when analyzing career outcomes.

2.2 Reform

In 1988, Brazil ratified a new Constitution as the country exited a period of military dictatorship. The new Constitution kept relatively intact the pre-existing text regarding the meritocratic selection of public servants (*concurso público*), with added legal requirements aimed to guarantee greater impartiality in hiring. These provisions were not meant to overhaul the selection of public servants or to address potential failures of proper meritocratic hiring. Instead, they aligned with the broader new constitutional text, which focused extensively on establishing human and individual rights protection and the social welfare responsibilities of the state.

The codified guidelines of impartial screening produced changes to screening practices that decreased discretion. High-level bureaucrats and government legal aides suggested modifications to screening steps on an occupation-by-occupation basis, which all employers in the federal sector should follow. For example, written and multiple-choice tests became necessarily blind, and more discretionary stages such as interviews were replaced or augmented with less discretionary ones.[9] These provisions generated an exogenous variation to the degree that employers could leverage personal connections.

3 Data

We assemble a new dataset that combines the universe of public sector job applications and private and public-sector employment. This provides a unique past and forward look at a job applicant's career path --- whether they're hired in that job process or not.

3.1 Employer-Employee Linkages and Hiring Processes

We combine two primary datasets. First, we recover the universe of public sector hiring processes in Brazil from 1985 to 1995. We rely on data generated by Mocanu (2022), who develops an algorithm to extract and parse all job processes in the public sector for federal and state governments. These records provide us with applicant information (failed candidates and hires

[9] Although there may be alterations in the blend of screening technologies post-reform, our results also hold for job processes maintaining consistent screening technology while reducing discretion. These processes involve transitioning from written non-blind tests to written blind tests or from a combination of written non-blind tests and interviews to a combination of written blind tests and interviews.

in each job process), which we then match to Brazil's employment registry known as RAIS (*Relação Anual de Informações Sociais*).

RAIS contains the universe of formal jobs in formal firms in the country. This includes formal organizations of any legal nature, including NGOs, privately-held companies, publicly traded companies, and government. Firms should report payroll information to Brazil's Ministry of Labor so that the mandatory federal social contribution Social Integration Program (PIS) can be computed. Each observation in the data is a worker-establishment match — a job — in a given point in time. For each observation, we observe the worker variables (e.g., name, gender, age, education, race, and unique worker identifier), firm variables (e.g., sector of activity, establishment size, name municipality, and unique establishment and firm identifiers), and job variables (e.g., mean monthly earnings during the current year, contractual weekly hours, tenure, occupation, hiring and separations, and reasons for separation).

To match job applicants to employment records in RAIS, we first match on names using a three-step algorithm that iteratively delivers perfectly sharp mappings followed by quasi-fuzzy matches. In summary, we first match the subsample of hired applicants. In these cases, we can augment full name and national identification numbers with the employer's name and entry year on the job as linking variables. For hires without a sharp match, we increasingly allow for the fuzziness in how names must coincide. First, we allow for abbreviated last names two ways (either in the RAIS or job processes data), for full names with more than three words. The majority of Brazilian names have two words as a last name (the first usually passed down by the mother and the second by the father). Often, however, workers have composite given names or more than two family names, which may be abbreviated particularly in the job processes postings due to capacity constraints in government gazettes. For those workers without a one-to-one match, we then allow for typos in the name which appear more common in the RAIS dataset.[10] We set an error tolerance of 2%, which implies on average, one mismatched letter in an individual's name with at least three words.

Finally, we consider names with only two words and match those based on how rare (frequency below 0.5%) the given and family names are in the RAIS sample. There are only a handful of different workers with the same pair of rare first and last names. We then move to unhired applicants using the same process. Our resolution procedure makes false positives or negatives extremely unlikely since progressively unmatched names become less likely to match rather than being forced to be linked. Overall, we successfully match 72% of job applicants to RAIS, where leakage also incorporates applicants who never held a job in the formal sector (type II errors).

[10] We can accurately estimate typos in worker names within RAIS by comparing variations across observations of a worker with the same unique identifier. Typos are relatively rare (less than 2%).

3.2 On-the-Job Outcomes

We construct three main groupings of on-the-job outcomes for hired workers. First, we observe directly from RAIS absenteeism or suspensions. Second, because the observation level in RAIS is a job, by definition any employer-employee link modification appears directly in the data. As a result, we observe separations.[11]

Our third productivity-related measure is promotions. Public sector job progression in Brazil is governed by a career-specific plan, which generally requires legislative action to be changed. Every career is grouped into "category" brackets, which are further divided into "levels". Public servants automatically progress between levels, which exclusively follow tenure criteria. This progression is accompanied by piece-wise small wage increases, with potential gradual changes in task domain and other benefits. An example would be progressing from junior associate to senior associate.

Once workers reach the highest level for a career bracket, they must advance to the next bracket through a promotion. Promotions are not automatic and carry a higher wage bump relative to level progression and changes in job attributes and benefits. An example would be to move from senior associate to junior manager. Each career has specific guidelines to determine promotion decisions, which generally include merit considerations --- though there are rarely formal performance evaluations, and decisions are ultimately discretionary. We consider worker i has been promoted in year t if we observe a *permanent* change in their contract wage of at least 10% in excess of the average wage change of workers in that establishment from $t-1 \rightarrow t$ (e.g., Cullen and Perez-Truglia (2023), Benson et al. (2024)).

3.3 Network Construction

We construct our baseline network with the following linking criteria: individuals i and j are connected from a point in time t if they shared a workplace during $\tau \leq t$. Besides being a standard network definition adopted in the literature on referrals in the labor markers (Barwick et al. (2023)), it is the ideal choice in our context where we aim at distinguishing whether connections help to share information about on-the-job skills and ability of applicants or promote favoritism. Our baseline definition considers a workplace at the establishment level — a university campus instead of the university system, a hospital facility instead of the health ministry — and the co-working period to be at at least a month. We limit our sample to establishments with less than 1000 employees at any given month since it is unlikely that an

[11] We can further refine separation rates by separation reason — transfers requested by the employee, employer, dismissal by employer, or voluntary leave by the employee. Due to tenure in the public sector, firings are rare and generally occur after a lengthy investigation process into severe infraction allegations is concluded. The low separation rate in the public sector prevents us from delving into the different reasons separately.

individual knows all of her coworkers in a very large firm. Because of the low separation rate in public jobs discussed above, the vast majority of connections come from the private sector.

We modify this definition along several dimensions for different exercises in the paper. A placebo connection involves two individuals i and j that worked in the same establishment without any time overlap. Precisely, if i is a job applicant to a public sector job and j a placebo connection there, j started working in their previous establishment after i had already left. This notion of a placebo connection is not only useful as a "fake" connection but also helps to test whether a specific public sector job gives preference to a particular employer. For example, if i and j are connected (in the baseline definition), i may be more likely to be hired due to preferences for her previous workplace, which could also explain why j was hired in the first place. In a similar vein, we construct placebo connections based on whether individuals worked at the same employer but not the same establishment at different points in time.

A different network definition considers a connection as colleagues that work during a common period for the same employer but in different establishments. While this makes it less likely they actually connected than imposing a shared workplace, it allows for broader network formation models. In a similar vein, we construct alternative measures of tie strength including the length of co-working and the size of the establishment where the connection took place. While our baseline network includes connections that occurred at any time in the past, we also consider a specification that focuses solely on recent connections, defined as those occurring within the last year. This approach ensures that the maximum number of individuals in our sample have connections that are recorded in our data.[12]

Furthermore, we consider the quantity and quality of connections each applicant has with different establishments. This involves measuring the number of past-coworkers currently employed at the hiring establishment, their pay grade, and the size of the hiring establishment.

3.4 Measuring Establishment Performance: Budgetary Efficiency

We measure budget execution at the government establishment or employer level using transparency data published in government gazettes. Public employers collect budget plans for a fiscal year and submit them to Congress or state legislatures, which in turn pass an annual budget authorizing employer-specific budgets. We observe the total amount authorized, executed, and spent, often by type. The Data Appendix details the data sources we homogenize and public accounts manipulations.

[12]It is important to note that the existence of unobserved connections stemming from work history before the RAIS data collection could bias our results bias towards showing no effects. Workers with connections established outside the observed period would likely be assigned to the control group, thus diminishing observed differences.

We measure budget efficiency as the budget shortfall at an establishment or employer in the public sector defined as $\$spent/\$requested$. We only consider establishment-year cases with a shortfall bounded by one --- we drop employers that obtain further appropriations during the fiscal year and effectively spend more than the annual budget approved by Congress. In the baseline case, we lump all expenditures together, regardless of whether they cover mandatory expenses or execute projects, investments, etc. This takes an agnostic view with respect to the quality of budgetary allocations. In additional measures, we dissect specific expense lines, including those that can be mapped to particular divisions or roles within an establishment. We measure the quality of expenditure allocation along three dimensions: the mandatory share spending, the share of the budget allocated to investment, and the share of non-wage expenditures and investment in core activities.

3.5 Description of Final Sample

Our final sample spans applicants to federal and state public sector jobs, as well as public sector workers, from 1986 to 1992. The first year of RAIS is 1985, which we use to construct connections in our baseline specification. The overall sample comprises over 3 million applicant-job observations, and almost 1 million public sector worker-jobs. Table 1 shows descriptive statistics for the pre-reform baseline, focusing on differences between both government levels. About 11% of hired workers in federal jobs had a past co-worker who is employed in their hiring establishment, about a third of the incidence of connected hiring in state jobs. In contrast, on average, the share of connected applicants was similar between federal and state job processes.[13]

As explained earlier, employee turnover is very low due to generous pay and tenure policies across all government positions, with separation rates around 2%. Promotions are relatively rare — the average public sector worker hired during 1986-1992 has a chance of 10% of being promoted five or ten years out. This is also consistent with the structure of public jobs, where promotions are not automatic (only progression) and can only happen at specific points of the career ladder. Federal jobs are disproportionately college-based, reflecting their service mix relative to states.

On average, public sector job applicants have about 1.4 connections in a government workplace (regardless of whether they are hiring or not) and are connected to one establishment.

[13] Baseline differences between Federal and State worker and job characteristics are washed out by our estimation strategy.

Finally, even though our baseline connection measure imposes a minimum shared workplace period of one month, the average co-working duration in the sample is about seven months.[14]

With the data outlined earlier, we have a comprehensive view of the entire job market within the public sector. This provides us a unique opportunity to study the role of connections beyond commonly used datasets, which encompass solely individuals already employed and working together. One benefit of our data collection is that it renders future outcomes from unsuccessful hires observable. For example, if a worker isn't hired in an establishment with a connection, where do they ultimately find employment? How does this change as a consequence of the impartiality shock? Our data and setting allows us to address these questions.

4 Sorting and Selection of Public Servants

Our empirical analysis begins by estimating the causal impact of being connected to public sector employers on an individual's propensity to apply and be hired in the public sector. We approximate the ideal experiment where personal connections are randomly removed by comparing outcomes of job seekers whose ability to leverage their connections is exogenously reduced to those of job seekers who can still use them.

Empirical strategy. To test the importance of personal connections during the job search process, we estimate the following difference-in-differences model:

$$\mathbb{1}\{\text{Connected}_{i,t}(e)\} = \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t \geq 1989}\} + \theta x_{i,e,t} + \varsigma_e + \varsigma_t + u_{i,t}, \tag{1}$$

where the left-hand side is an indicator for whether job applicant i has any connections (past coworker) in establishment e at the time t of her job application. Treated establishments are those in the federal sector and untreated in state governments. Post is a dummy variable equal to 1 if the job application was made *after* the reform and equal to 0 if it was done before. Establishment fixed effects, ς_e, absorb establishment-specific heterogeneity that may be correlated with having connected applicants, for example, the organizational structure of the business or factors related to local institutions. Time-fixed effects, ς_t, address concerns of unobserved aggregate shocks, such as general elections or macroeconomic policies that can affect public employment outcomes. $x_{i,e,t}$ denotes a set of individual characteristics, including gender, age,

[14]There are also differences in share of connections by gender and race, with women being six percentage points less likely to have connections and non-whites about half of that. Our empirical strategy accounts for group differences by controlling for gender and for race (when the data allows). Therefore, we focus on analyzing the effects of connections conditioning on group membership. This mitigates concerns that the reform also had impacts on dimensions other than connections.

and education, all of which can affect the probability of applying before and after the reform.[15] Further, these controls mitigate concerns that the reform had impacts on dimensions other than personal connections.

In additional specifications, we further include occupation fixed effects and use establishment by time fixed effects, $\varsigma_e \times \varsigma_t$. Occupation fixed effects capture occupation-specific propensities of having connected applicants. Establishment by time fixed effects allow for establishment-specific time-varying factors. Alternatively, we include establishment effects and occupation by time fixed effects to further control for occupation-specific time trend in the use of connections. Since the same establishment hires from multiple job processes, common shocks at the establishment level, including job-process-specific shocks, affect applicants to different positions. Because of that, we cluster standard errors at the establishment-level, which also matches the treatment-assignment unit in our setting.

To test the importance of personal connections for hiring decisions, we estimate the difference-in-differences model:

$$P\left(\text{Hired}=1|\text{Applicant}\right)_{i,t,e} = \delta_0 \mathbb{1}\{\text{Connected}_{i,t}\left(e\right)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t\geq 1989}\} + \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t\geq 1989} \times \text{Connected}_i\left(e\right)\} + \theta x_{i,e,t} + \varsigma_e + \varsigma_t + u_{i,e,t} \quad (2)$$

where the outcome is the probability that individual i applying to a job process in establishment e is hired, with the remaining variables defined as before. β measures the differential effect of lower hiring discretion on connected relative to unconnected applicants. Because this specification conditions hiring probabilities on the set of individuals that apply for a job, it nets out direct supply-side responses — for instance, having fewer individuals with connections applying — which we measure separately in model (1).

Evidence in support of the identification strategy. The main identification assumption is that, in the absence of the institutional change, job processes in federal and state employers would exhibit parallel trends in the share of connected applicants and hires. To investigate potential differences in trends of how connections were leveraged across different government levels and the dynamics of the reform effects, we estimate event-study style treatment effects of equations (1) and (2), centered around the pre-reform year, 1988.

Figure 1 presents plots for the dynamic estimated effects on applications ((1)) and conditional hires ((2)) in two separate panels. Dashed lines are 95% confidence intervals calculated with establishment-clustered standard errors. Following the new constitutional requirements,

[15] The inclusion of other characteristics such as private and public sector experience or race does not change our main estimates.

individuals become less likely to apply to jobs at establishments where they have connections and less likely to be hired, given that they apply. Estimated effects for connected hires fall faster and more sharply than application rates, consistent with immediate compliance through changes in screening in the federal sector limiting discretion. On the application margin, the more gradual estimated response can be explained by a delay in applicants internalizing the reduced opportunities to leverage connections. Pre-reform effects are indistinguishable from zero, suggesting that decreases in the use of connections are attributable to the reform rather than already in place through an alternative mechanism.

One concern regarding the proper identification of the treatment effect induced by the reform is that the relative desirability between federal and state jobs might have changed for connected and unconnected individuals. Perhaps those workers with connections in both government levels switch away from applying to federal jobs toward state jobs, where connections could still be more easily levered. We address this concern by estimating the impact of the reform separately for federal and state jobs. Figure 2 shows that the treatment effect estimated in our difference-in-differences design picks up a sharp decrease in the conditional probability of applicants to federal jobs having connections, while applicants in state jobs remain just as likely to have connections before and after the impartiality reform.

Figure 3 in the Appendix further shows that the total number of jobs did not differentially change in federal compared to state jobs.

4.1 Effects on Job Search and Access

Main results. Table 2 shows the results of models (1) and (2) with different sets of fixed effects. Results in column (1) show that the probability of an applicant being connected drops by 2.4 percentage points following the reform (around 21% of the pre-reform baseline gap in federal job processes). Controlling for year-by-establishment fixed effects and occupation fixed effects results in small changes to this estimated effect. In column (4), in addition to establishment fixed effects, we also allow for occupation-specific time trends to address the potential concern of whether the effect is picking up occupations that rely less on connections over time. This has little impact on the magnitude of the estimated effect.

Columns (5) through (8) compare the change in the probability that a connected applicant is hired relative to an unconnected candidate. With different sets of fixed effects, the estimated effect is around a decrease of 0.2 percentage points, for a pre-existing gap of 1.2 percentage points or a decrease of 17% at the baseline. This evidence shows that connections are important for both providing information about jobs and giving access to jobs. When the use of personal

connections is hampered, changes in both the pool of applicants and hiring decisions contribute to lower shares of new hires that are connected.

Next, we document how personal connections influence the structure of the job search market by examining changes in the job search behavior of connected and unconnected applicants before and after the reform. To do so, we construct a measure of all public sector establishments that are hiring in that year to which an individual has a connection, regardless of whether the worker applies to any of them and measure the proportion of potential connections that a worker can tap into in the federal compared to the state sector for every year. Establishments where a worker may have a connection but do not hire in year t are not counted in that year. Results in column (1) of Table 3 show that in response to the decrease in the ability to leverage connections, connected job seekers are more likely to increase the total number of applications compared to unconnected job seekers. In column (2), we include municipality fixed effects to capture the job openings more easily available to workers and find an effect of the same magnitude: for every additional 10 percentage points in the proportion of potential connections in the federal sector, the number of applications from connected workers increases by 0.8 compared to unconnected workers.

In column (4), we find a decrease in the probability of the applicant applying to just one job following the reform, and in column (5), we find that connected applicants are now more likely to apply to jobs in similar occupations as their past employment, although not statistically significant. Overall, we find that the decrease in the ability to use connections changes the structure of the job search market along several margins.

Heterogeneous effects. Figure 4 and Figure 5 show evidence on the effects when varying the strength of connections and the plausible influence of that connection in the public job. From the left to the right (starting with our preferred baseline specification of the treatment effect as reference on the left), we use the number of connections in the establishment (instead of the indicator for the presence of at least one connection), then the average time sharing a workplace with those connections, only counting recent connections (those made within the previous year before the job application), and connections made in small workplaces (< 50 coworkers). These measures effectively change the universe of previous coworkers considered to be a connection or how along which dimension the network is constructed.

The last two estimated effects in each figure instead focus on counting as a connection those coworkers with specific employment characteristics in a public establishment. Namely, we consider connections as those working in small establishments in government (<50 employees) or in high-level positions, measured as the top wage decile in that establishment. Recall that an establishment corresponds, for example, to a hospital or community clinic, a primary school, or

a police precinct. Results for both job application and access show some variation in estimated coefficients across specifications, all falling well within the baseline treatment effect confidence interval.

We next investigate whether the effects are different for high-skilled and low-skilled jobs. Table 4 shows heterogeneity analysis based on the education required by the public sector job. It may be the case that in college jobs workers place greater weight on connections due to private information, or that occupations are structured in a way that connection-building activities (e.g. networking events, rotation opportunities) are more widely fostered. Indeed, results show that the reform induces a drop in the share of connected applicants in college jobs almost two-fold the size relative to those without the requirement. Net of the application margin effect, connected applicants become even more unlikely to be hired in government college jobs: a 0.2 p.p. decrease in the hiring gap between connected and unconnected of 0.6 percentage points, or about a 33% reduction. For less-than-college jobs, the estimated decrease in conditional hiring odds is about 7% of the pre-existing gap. Taken together, these results imply that increased impartiality in high-skill jobs had a larger reduction in supply and demand for connected workers.

Robustness tests. We conclude this section by conducting a robustness analysis, shown in Table 5. A possible concern is that having a shared working experience with a current employee in the hiring establishment captures unobserved factors related to an individual employment history rather than personal connections, such as common skills, preference for a given past employer, or demand shocks for specific industries. The first column implements a placebo connection, defined as workers who shared the same establishment in different points in time. While an individual's placebo coworkers are likely to have similar skills in ways we can and cannot observe, they are less likely to have interacted because they have not worked together. While these are not proper connections, this measure captures common skills or training requested or valuable for the current employer. Our estimated effects for both placebo applicants and hires (column (4)) are statistically insignificant and around zero, showing that our measure of personal connection is not reflecting common skills.

In columns (2) and (5), we rule out the further possibility that our measure of connection captures a preference of the hiring establishment towards a given employer more broadly. Specifically, we construct a similar placebo connection measure based on workers who shared an employer at different points in time but who worked in different establishments within that employer. The estimated effect is around zero and statistically insignificant. Finally, we implement the basic connections definition specification but augmented with industry of origin fixed effects (using Brazil's IBGE CNAE codes). This specification therefore compares connected and

unconnected workers applying to the same public establishment in a year who have a work history in the same industry. For instance, if federal courts reward candidates who previously had experience clerking compared to corporate lawyers, a connection may simply pick up a common demand shock for the already hired connection and the connected applicant. The inclusion of the fixed effect produces a minimal change in the coefficients of columns (3) and (6), suggesting that our results are also robust to this possibility.

5 The Value of Personal Connections

An open question in the literature on referrals is whether personal connections give access to better-paid jobs. The challenge is that the wage obtained in the absence of connections is usually unobserved. The richness of our data allows us to construct two counterfactuals. For each recruitment process, we observe i) the wage of the connected applicants who are hired in the same public sector establishment when connections cannot be used (i.e., after the reform), ii) the wage of the connected applicants who fail the job process but are hired for a different job, in either public or private sector when connections cannot be used.

We use the wage of the connected applicants who are hired in the same public sector establishment around the reform to estimate the following model:

$$\begin{aligned} wage_{i,e,t} = \delta_0 \mathbb{1}\{\text{Connected}_{i,t}(e)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t\geq 1989}\} + \\ \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t\geq 1989} \times \text{Connected}_{i,t}(e)\} + \theta x_{i,e,t} + \varsigma_e \times \varsigma_t + u_{i,e,t} \end{aligned} \tag{3}$$

where $wage_{i,e,t}$ is the wage of individual i at establishment e in time t. The coefficient β estimates how the wage gap between connected and unconnected hires in the same public sector establishment changes following the reform. In this model, we control for establishment-by-time factors so that the model relies on variation across applicants to different job postings by the same employer in the same establishment in a given year. If the impartiality reform changed which jobs in the same establishment workers with connections are able to get relative to those unconnected, this will be reflected in the average wage differential between the two groups.

We then use the wage of the connected and unconnected applicants who fail a job process but are hired in different jobs to measure the outside options with and without connections. Specifically, we compare the wages these failed applicants would have earned if hired to the ones they eventually earn in a different job following the rejection. We estimate the following model:

$$wage_{i,j(e),m\neq e,\tau\geq t} - wage_{j(e),t} = \delta_0 \mathbb{1}\{\text{Connected}_{i,t}(e)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t\geq 1989}\} + \quad (4)$$
$$\beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y\geq 1989} \times \text{Connected}_{i,t}(e)\} + \theta x_{i,e,t} + \varsigma_{j(e)} + \varsigma_t + u_{i,e,t}$$

where $wage_{i,j(e),m\neq e,\tau\geq t}$ is the wage of individual i who failed job process $j(e)$ at establishment e and is eventually hired at establishment m at time $\tau \geq t$ in either public or private sector, and $wage_{j(e),t}$ is the wage in the job process $j(e)$ that the failed job candidates applied to. The model uses a job process fixed effect, $\varsigma_{j(e)}$,to calculate how these differences between connected and unconnected applicants change after the reform within the same applicant pool. Figure 6 illustrates this exercise.

Estimated effects. Columns (1) and (2) of Table 6 show the estimation results for models (3) and (4) respectively. We use different fixed effects. In column (1), since we are comparing connected and unconnected hires in the same establishment, we include the establishment and year fixed effects, with the establishment by year fixed effects to allow for establishment-specific time trends in public sector wages. In column (2), we include job process fixed effects in order to compare unsuccessful connected and unconnected individuals in the same applicant pool, as well as year-fixed effects to address any concerns about unobserved aggregate shocks that can affect wages.

We measure wages in multiples of Brazil's federal minimum wage, which binds for all formal jobs in the country. During the 1980s and 1990s, Brazil faced continuous acute inflation waves reaching as high as 90% a month in 1990. Price adjustments of goods and services were common at a daily frequency, and several currencies were introduced in various different regimes. This makes it hard to compare earnings over time even in real terms. The minimum wage multiple provides a more consistent purchasing power style metric that allows us to compare across workers in space and time.

The results reveal that when connections cannot be used, wages of connected workers relative to unconnected fall, both for those who are hired and for those who are not. Column (1) shows that wages of connected hires fall by 0.4% relative to unconnected hires in the same establishment. This evidence thus indicates that not only were connections giving access to public sector jobs, but higher-wage jobs, even within the same establishment. Column (2) shows that connected workers who did not get hired ended up in worse-paid jobs compared to unconnected who did not get hired, specifically, with a wage 24.7% lower than the wages they would

have otherwise received in the public sector, suggesting that connected's next best option (or counterfactual wage) is lower. [16]

6 Screening Information or Favoritism?

The analysis in the previous section shows that personal connections grant access to better-paying jobs. The next question is whether connections provide undisclosed information that could otherwise go unnoticed during the hiring process or promote favoritism. In the former scenario, by granting access to private information about workers' skills, connections would improve the selection and subsequently increase the productivity of new hires. Conversely, in the latter scenario, they might simply foster favoritism, which could hinder productivity. The primary obstacle in teasing out these two hypotheses is that the true quality of a worker is often not readily observable.

Our comprehensive dataset, covering the complete employment records of individuals in both public and private sectors, spanning pre- and post-job applications, as well as their career paths for both selected and unselected applicants, enables us to investigate this question along several dimensions Firstly, we assess quality by using past private-sector earnings as a measure of their outside opportunity (Dal Bó et al. (2013)). The idea here is that previous wages capture workers' productivity and skills that are valued by the market. Additionally, we evaluate unobservable quality through deriving residuals from a Mincer earnings regression, controlling for a wide array of interactions involving age, education, and sector of employment, alongside fixed effects for municipalities to account for wage disparities across different locations. The core concept is simple: if two individuals in the private sector share similar observable characteristics but one earns a higher income, it suggests a higher likelihood of possessing superior abilities.

Secondly, we assess unobservable quality by estimating residuals from a Mincer earnings regression controlling for an extensive set of interactions involving age, education, and employment sector, as well as municipality-fixed effects to account for wage differences across locations. The underlying idea is straightforward: if two individuals in the private sector share similar observable characteristics but one earns a higher income, it suggests a higher likelihood of possessing superior abilities (Dal Bó et al. (2017), Besley et al. (2017)).[17] Specifically, follow-

[16] The higher magnitude of the effect is expected since the spread of wages within a given establishment is way lower than that between establishments and sectors.

[17] An alternative proxy of worker quality might be an individual's education level. In Brazil's public sector, however, jobs that are filled through *concurso público* impose a minimum educational requirement that is binding — for example, college jobs automatically disqualify non-college applicants. Overall, 93% of public sector workers exactly match the announcement job requirements at the moment of hiring.

ing the approach of Besley et al. (2017), we estimate residuals $\widetilde{q_i}$ from the following regression

$$w_{i(gender),t,m} = g\left(age_{i,t,m}, education_{i,t,m}, sector_{i,t,m}\right) + \alpha_m + \alpha_t + q_{i,t,m} \tag{5}$$

where $w_{i,t,m}$ is the private sector wage for worker i for year t working in municipality m. We fit the regression separately for men and women in order to avoid confounding abilities with labor market behavior driven by gender norms. For each worker i, we average the residuals over the years up to the moment of each job application. For example, if an individual worked between 1991 and 1993 and first applied to a public sector job in 1993, we use this history when estimating her ability to proxy in 1993. If she applies again in 1995, her residuals in that year will incorporate additional wages earned, if any, during 1993-1995. Further details on our Mincer estimation can be found in the Appendix.

Thirdly, and notably novel, we exploit information regarding workers' careers and consider metrics of on-the-job outcomes for workers hired in the public sector. This latter measure addresses concerns that competence exhibited in the private sector may not necessarily translate into performance in the public sector. Specifically, we examine separation rates, suspensions, absenteeism, and promotions (see Section 3). First, low separation or turnover rates are common objectives among employers since it is costly to replace workers and it is a standard measure of match quality (e.g., Dustmann et al. (2016)). Second, suspensions and absenteeism are relatively clear-cut objective measures of (non-)performance. Finally, our measure of promotions is, at least in principle, based on productivity thus providing an additional measure of on-the-job performance of hired employees.

Estimated effects: Observed and unobserved ability. Table 7 presents the results when we use the previous wage as a measure of worker quality (panel A) and then Mincer residuals (panel B). All regressions include establishment and year-fixed effects, with additional specifications including establishment by year, job process fixed effects, and standard errors clustered at the level of the establishment. The table shows both worker quality measures with different sets of fixed effects. We include establishment-year FE or just establishment and year FE to control for all time-invariant characteristics that affect both the share of connected hires and on-the-job outcomes - for example, the organizational structure of establishments, establishment-specific pay grades or compensation schemes.

In column (1), we examine how the reform affected the quality of the applicant pool overall. In columns (2)-(4), we test how the reform differentially changed the quality of connected relative to unconnected applicants. Columns (5) through (8) use the unobserved quality measure in the same sequence of specifications. Across the board, the quality (both observable and

unobserved) of connected relative to unconnected job candidates increases following the reform. This is the case even after controlling for establishment-specific trends (column (3)) and by comparing applicants to the same job opening (column (4)). This latter specification rules out that the results are driven by connected applying to better jobs. Gains in wage-equivalent quality range from 0.2 to 1 additional minimum wage across specifications, a large economic effect in a country where the average worker earned no more than two minimum wage multiples during the period. Table 8 performs a similar analysis but focuses on the quality of hires. Again, the reform induces an increase in the average quality of public sector hires, particularly for connected individuals.

The next question is whether higher-quality individuals are applying at a higher rate (or hired) or lower-quality individuals are discouraged from applying. In Table 9, we divide applicants into quartiles based on the distribution of residual ability scores $\widetilde{q}_i$ from equation (5). We then use two alternate dummies as dependent variables of model (2). The first dummy takes value one if the applicant is in the top quartile, 0 otherwise (column 1). The second dummy takes value one if the applicant is in the bottom quartile and 0 otherwise (column 2). The results show that while the gap between high-quality connected and unconnected applicants remains roughly unchanged before and after the reform, there is a sizeable gap in quality at the bottom of the quality distribution. This evidence suggests that the overall effects are not due to a surge in applications from higher-quality individuals who are connected, but rather a decrease in the propensity of lower-quality individuals to apply to establishments with connections.

However, our findings so far are obtained from comparing individuals who have connections versus those who do not. They do not reveal whether the quality of the unconnected changes as well. Our ability to observe applicants and employees for an extended duration enables us to further our analysis of how personal connections operate by pin down which one of the two groups are responsible for the changes in quality we have discovered.

Table 10 show the averages of our measures of observable and unobservable quality before and after the reform in different panels. In each panel, we report the information for workers with and without personal connections applying for or hired in the federal sector jobs separately. The message from Table 10 is clear: while the average quality of unconnected individuals remain roughly constant before and after the reform, it is the average quality of connected individuals, which increased dramatically after the reform. This is true when using either measure of quality for both applicants and hires. The table shows that the quality of unconnected applicants does not change while the quality of connected applicants increases 3.5-fold and almost 9-fold in Panel A and B respectively. For hires, the panels show broadly the same pat-

terns.[18] While before the reform connected and unconnected hires had a similar unobserved quality (with connected applicants being slightly of lower quality), after the reform connected hires are of considerably higher average quality compared to unconnected. This aligns with evidence from Table 9 where low-quality connected individuals refrained more strongly from applying to public jobs following the reform. It is worth pointing out that the finding that the quality of unconnected applicants and hires further supports our evidence in Section 4 that the reform did not affect the hiring process in ways different from the ability to use connections.

Estimated effects: On-the-job outcomes. Next, we analyze the quality of hires before and after the use of connections was endogenously removed by evaluating on-the-job performance measures. The results in Table 11 reveal that following the inability to leverage connections, the quality of hires improved across all the aspects examined. We control for year and establishment fixed effects to account for establishment-specific pay-grades, promotion standards, or other organizational structure that can affect our on-the-job measures. Column (1) shows that newly employed individuals post-reform are less likely to separate or resign, and thus show a prolonged job tenure. Relative to a pre-reform mean of 2%, the separation rate of new hires decreased by 0.6%. The magnitude of the effect is relatively small, but it is in line with an overall low separation rate in the public sector.

Column (4) shows that post-reform hires also display decreased rates of absenteeism or suspension. Specifically, their probability of ever being suspended or absent decreased by 5%. Next, we analyze a third productivity-related measure, promotion rates. This measure, constructed as described in Section 3, is meant to capture non-automatic career progression that generally includes merit considerations. Column (7) shows that post-reform, employed individuals have a 5% higher likelihood of being promoted. The effects are especially large when comparing connected versus unconnected hires, and it remains so after controlling for establishment-by-year fixed effects that capture establishment-level changes in promotion or suspension protocols. This is consistent with the previous results that showed that post-reform low-quality connected individuals were disincentivized from applying and less likely to be hired given they applied.

Figure 7 implements a number of heterogeneity exercises both for separation and promotion rates, exploring the organizational importance and quality of the connection, skill level, and previous experience in the public sector. First, we explore whether the referrer's own quality, as measured by their Mincerian residuals from past private earnings, affects the quality of hires. This might be the case if, for example, higher quality connections are able to screen better than lower quality individuals. Second, we define high-level connections as those earn-

[18] The smaller relative increases in the quality of connected hires relative to connected applicants following the reform is not unsurprising since offers are expected to be made only to the right tail of the quality distribution.

ing above the 75th percentile in the establishment to capture connections in more influential positions within the establishment and may thus have different incentives when hiring connected workers. Third, we distinguish between coworkers who worked together in the same *public* sector establishment before. This may matter if screening information on workers' productivity in the public sector is different than private sector, which only connections made in the public sector can provide. The figure shows that the results are of similar magnitude across the different subsamples of connections, consistent with the use for favoritism across different types of connections. Finally, we examine whether effects on productivity differ by occupational skill level. In the fourth bar of the figure, we find a more marked decrease in separation rates, and higher increase in promotion rates, for college degree jobs. This suggests that the use of favoritism is higher in jobs that are high-skill and thus more sought after.

Overall, our findings suggest that discretion in public servant hiring distorts the hiring of top-tier workers in favor of individuals with connections, aligning with the practice of utilizing connections for preferential treatment. Our results on productivity should be interpreted in the light of the incentives employees in public sector organizations face. These organizations differ from private firms in substantive ways, such as low exit rates and a usual absence of performance pay (Finan et al. (2017)), thus, decreasing public employees' incentive to refer high-quality individuals compared to the private sector. This is in line with findings from experimental literature that individuals favor high-ability workers when they are incentivized via pay (Bandiera et al. (2009), Beaman and Magruder (2012)). The negative estimates of the use of private connections on productivity also complement the literature on political connections that find that patronage results in favoritism for private gain (Xu (2018), Colonnelli et al. (2020).

7 Personal Connections and State Efficiency

Our analysis so far shows that personal connections distort public servant hiring from high-quality workers towards connected individuals, which is consistent with connections being used for favoritism. The reform that decreased the ability of individuals to leverage their connections led to the hiring of higher-productivity workers. In this section, we seek to determine the consequences of the use of personal connections in hiring on public sector efficiency. The challenge here lies in the fact that measuring the performance of government agencies is well-known to be challenging due to the lack of comparable data.

To overcome the challenge of measuring establishment efficiency, we use transparency data published in government gazettes to construct several measures of budget efficiency. We observe the total amount authorized, executed, and spent, by type. We construct several measures of budget efficiency. First, we measure budget shortfall at an establishment in the public sector

defined as $\$spent/\$requested$. The idea is that micro-budgetary deviations that capture a mismatch between budget requests and actual spending may arise due to either poor planning, poor operational execution, or poor expenditure quality. Second, we measure the quality of expenditure allocation along three dimensions: the mandatory share spending, the share of the budget allocated to investment, and the share of non-wage expenditures and investment in core activities.

The first quality measure is a standard way to impute free resources, once obligations such as debt service, social security, and wage bill have been honored. Then, with the resources available, the public sector employers may choose to spend on plausibly more relevant items, like kevlar vests in federal police establishments, books in schools, or IT training for IRS agents. Core investments are more likely to improve productivity, service coverage or quality, than those in non-core investments (e.g., painting the office or having a more generous travel allowance).

Estimates of the following equation are of intrinsic interest because they provide an assessment of the impact of the reform on public servant hiring:

$$\%\text{New unconnected hires} = \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t \geq 1989}\} + \theta x_{e,t} + \varsigma_e + \varsigma_t + u_{e,t}$$

where $\varsigma_e$and ς_t represent establishment and year effects, respectively, and $x_{e,t}$ are establishment level covariates, including size and industry. This is equivalent to our analysis in Section 4 but at the establishment level. β captures the effect of reducing discretion on the shares of new hires that are connected and encompasses changes in both the pool of applicants and hiring decisions.

The estimates of the equation above also represent the first stage of a two-stage least-squares (2SLS) estimation of the impact of hiring more unconnected workers on establishment productivity. Consider the following equation that characterizes the effect of unconnected hires on employer productivity:

$$y_{et} = \beta \times \%\text{New unconnected hires} + \theta x_{e,t} + \varsigma_e + \varsigma_t + u_{e,t}, \tag{6}$$

Ordinary least-squares (OLS) estimates of the equation may lead to biased estimates if there is a correlation between $u_{e,t}$ and $\%NewUconnectedHires$. For example, if there is selection of more productive establishments into making more use of connections in their hiring decisions, or, on the other hand, if more corrupt establishments are more likely to hire more connected workers. To address this concern we use the reform as an instrument for the share of new unconnected hires. The identification assumptions are that differences in the share of connected and productivity across federal and state establishments would not have been systematically

correlated with the reform, and that the reform did not have a direct effect on establishment productivity.

Estimated effects. The results are presented in Table 12. The instrument has good explanatory power in the first stage, which indicates that the different budget measures respond relatively contemporaneously to changes in the make-up of new hires. To ease the interpretation of the second-stage estimates, all results are in terms of an increase in unconnected new hires. Across all columns, the results show that both budgetary efficiency and allocative quality improve — increasing the share of unconnected hires by one percentage point (80-90% at baseline) reduce shortfalls by 0.78%, and improve quality-adjusted spending, particularly geared toward core activities. While the share of mandatory spending falls, the effect is economically small. This is in line with the high commitment to the Brazilian budget with endowments, pensions, and the debt service, which have been increasing steadily since the 1980s.

8 Conclusions

Social networks play an important role in labor markets. Yet, the question of whether connections primarily provide information about job opportunities or directly influence hiring decisions, either by reducing informational uncertainties on worker quality or through favoritism, remains unanswered. Which, if any, of these mechanisms matters empirically will have different implications for job search behavior, employment decisions, and organizational performance.

This paper shows that an exogenous reduction in the ability to leverage personal connections in Brazil's public sector decreased the probability of individuals applying and being hired in establishments where they had connections. While there is no significant change in the average quality of unconnected applicants, we find substantial changes in the quality of the connected applicant pool in terms of both observed and unobserved characteristics. In particular, we document that the reform has disincentivized low-quality connected candidates to apply, increasing their search effort by applying more broadly, particularly to jobs without connections. Moreover, connected workers who did not get hired end up in worse-paid jobs compared to unconnected applicants in the same job process. Thus, connections gave access to higher-paid public sector jobs, affecting the sorting and selection of workers. Consequently, curbing connections improved promotion rates, decreased separations and absenteeism, and boosted operational and budgetary efficiency.

Our analysis reveals that even in a meritocratic civil service, common in many countries, discretion in hiring distorts the allocation of public sector jobs and reduces performance of

public servants and institutional capacity. We focus on coworker networks, which are more likely to possess and provide information about a candidate's productivity. Yet, we still find evidence consistent with the practice of using connections for preferential treatment. Nevertheless, our findings should be interpreted in the context of public sector organizations, characterized by highly structured recruitment processes, low separation rates, no performance pay, and lack of incentives to refer high-quality social connections. Future work should shed light on how the influence of personal connections on job searches, hiring decisions, and performance varies across settings characterized by different designs of recruitment processes and varying incentives for workers when making referral decisions.

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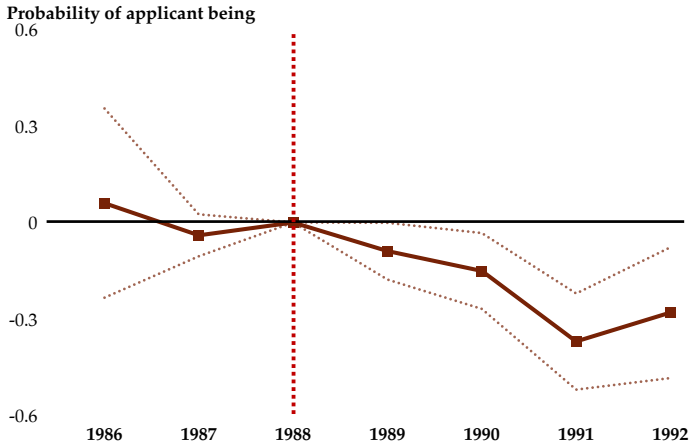
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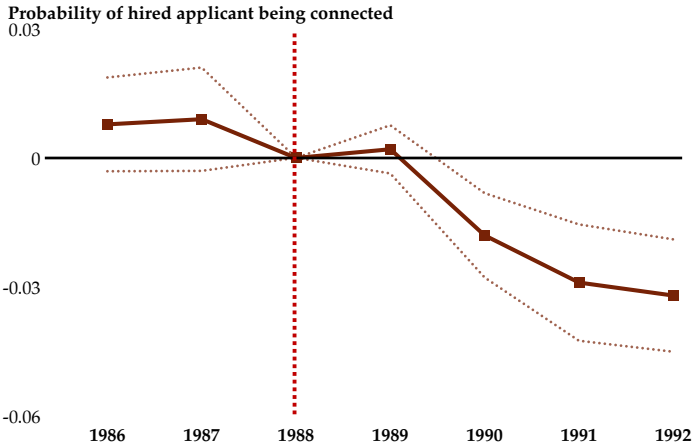
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Figures and Tables

Figure 1: DiD Dynamic Effects



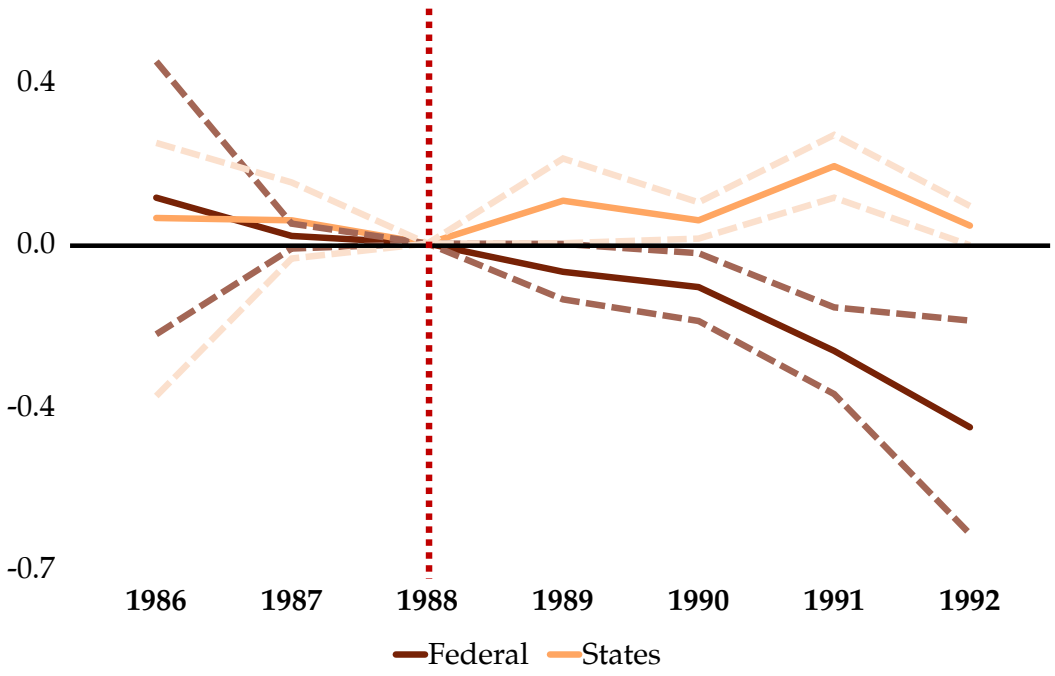
Probability of Connected Applying



Probability of Connected Applicants Being Hired

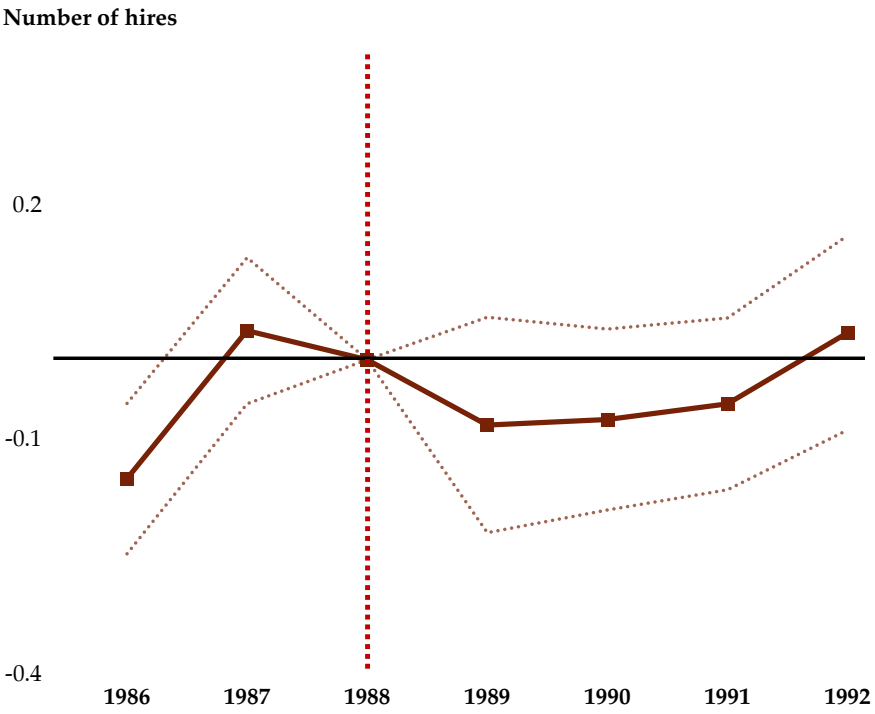
Notes: This figure plots dynamic effects of the baseline difference-in-differences models: $\mathbb{1}\{\text{Connected}_i(e)\} = \sum_{r\neq -1}\beta_r \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y\geq 1989}\} \times \mathbb{1}\{t = r+1\} + \theta x_{i,e,t} + \varsigma_e + \varsigma_t + u_{i,e,t}$; and $\Pr(\text{Hired=1} \mid \text{Applicant})_{i,e,t} = \delta_0 \mathbb{1}\{\text{Connected}_i(e)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t\geq 1989}\} + \sum_{r\neq -1}\beta_r \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y\geq 1989} \times \text{Connected}_i(e)\} \times \mathbb{1}\{t = r+1\} + \theta x_{i,e,t} + \varsigma_e \times \varsigma_t + u_{i,e,t}$ respectively. Treated establishments are those in the federal sector and untreated in state governments. Worker i has connections in establishment e if they shared a workplace sometime in the past. Controls include gender, age, education, and other worker characteristics. Standard errors are clustered at the establishment level.

Figure 2: Probability of Connected Applying, Federal and State Jobs



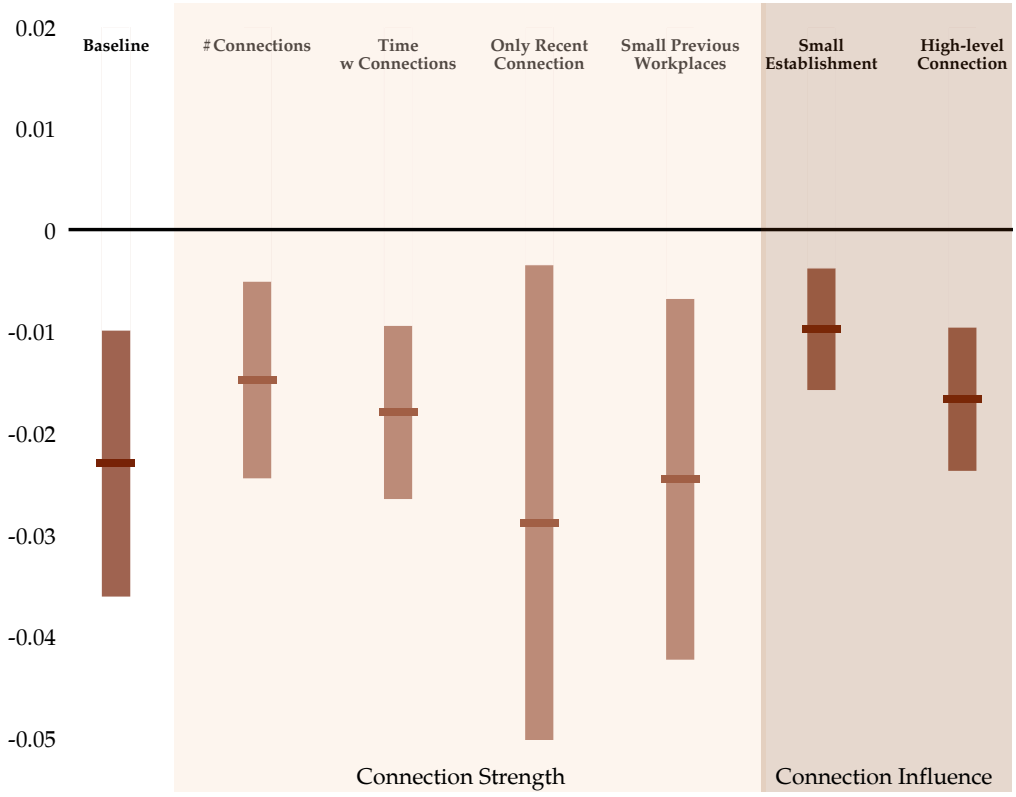
Notes: This figure plots dynamic effects of the baseline difference-in-differences model: $\mathbb{1}\{\text{Connected}_i(e)\} = \sum_{r \neq -1} \beta_r \mathbb{1}\{\text{Post}_{y \geq 1989}\} \times \mathbb{1}\{t = r + 1\} + \theta x_{i,e,t} + \varsigma_e + \varsigma_t + u_{i,e,t}$, separately for federal and state jobs. Treated establishments are those in the federal sector and untreated in state governments. Worker i has connections in establishment e if they shared a workplace sometime in the past. Controls include gender, age, education, and other worker characteristics. Standard errors are clustered at the establishment level.

Figure 3: Placebo Test: DiD Total Number of Job Hires



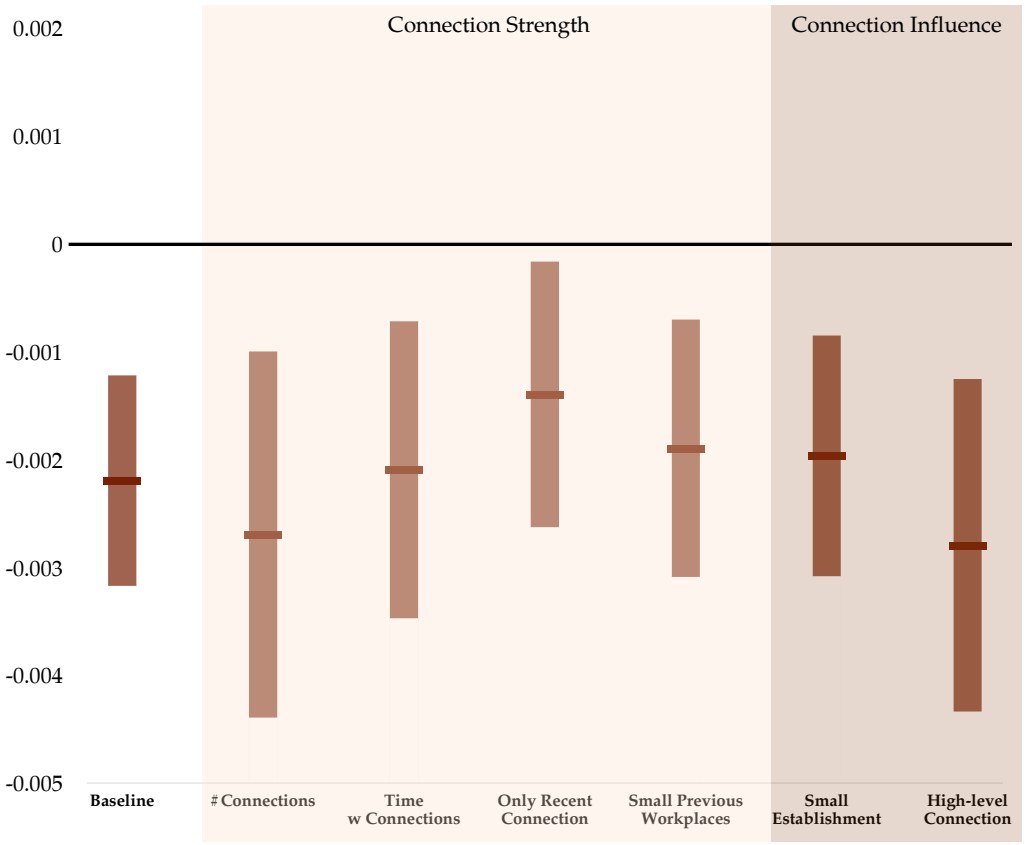
Notes: This figure plots dynamic effects of the difference-in-differences model: $log \sum_e \text{Hires}_{e,t}(o) = \sum_{r \neq -1} \beta_r \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y \geq 1989}\} \times \mathbb{1}\{t = r + 1\} + \varsigma_e + \varsigma_t + \varsigma_o + u_{e,t}$. Treated establishments are those in the federal sector and untreated in state governments. The outcome is the total number of hires for a establishment-occupation pair in a given year. Fixed effects include year, establishment, and occupation code. Standard errors are clustered at the establishment level.

Figure 4: Heterogeneity: Probability of Connected Applying



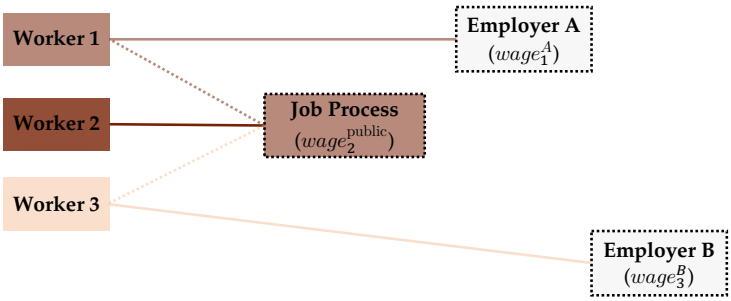
Notes: This figure plots the effects of the baseline difference-in-differences model: $\mathbb{1}\{\text{Connected}_i(e)\} = \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y \geq 1989}\} + \theta x_{i,e,t} + \varsigma_e + \varsigma_t + u_{i,e,t}$, except for the estimates of # *Connections* and *Time w Connections*. In these cases, we regress $\mathbb{1}\{\text{Applicant}_i(e)\} = \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y \geq 1989} \times \text{Connected measure}_i(e)\} + \theta x_{i,e,t} + \varsigma_e + \varsigma_t + u_{i,e,t}$. Treated establishments are those in the federal sector and untreated in state governments. Worker i has connections in establishment e if they shared a workplace sometime in the past. Controls include gender, age, education, and other worker characteristics. Standard errors are clustered at the establishment level.

Figure 5: Heterogeneity: Probability of Being Hired



Notes: This figure plots the effects of the baseline difference-in-differences model: $\Pr(\text{Hired=1} \mid \text{Applicant})_{i,e,t} = \delta_0 \mathbb{1}\{\text{Connected}_i\,(e)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t \geq 1989}\} + \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y \geq 1989} \times \text{Connected}_i\,(e)\} + \theta x_{i,e,t} + \varsigma_e \times \varsigma_t + u_{i,e,t}$. Treated establishments are those in the federal sector and untreated in state governments. Worker i has connections in establishment e if they shared a workplace sometime in the past. Controls include gender, age, education, and other worker characteristics. Standard errors are clustered at the establishment level.

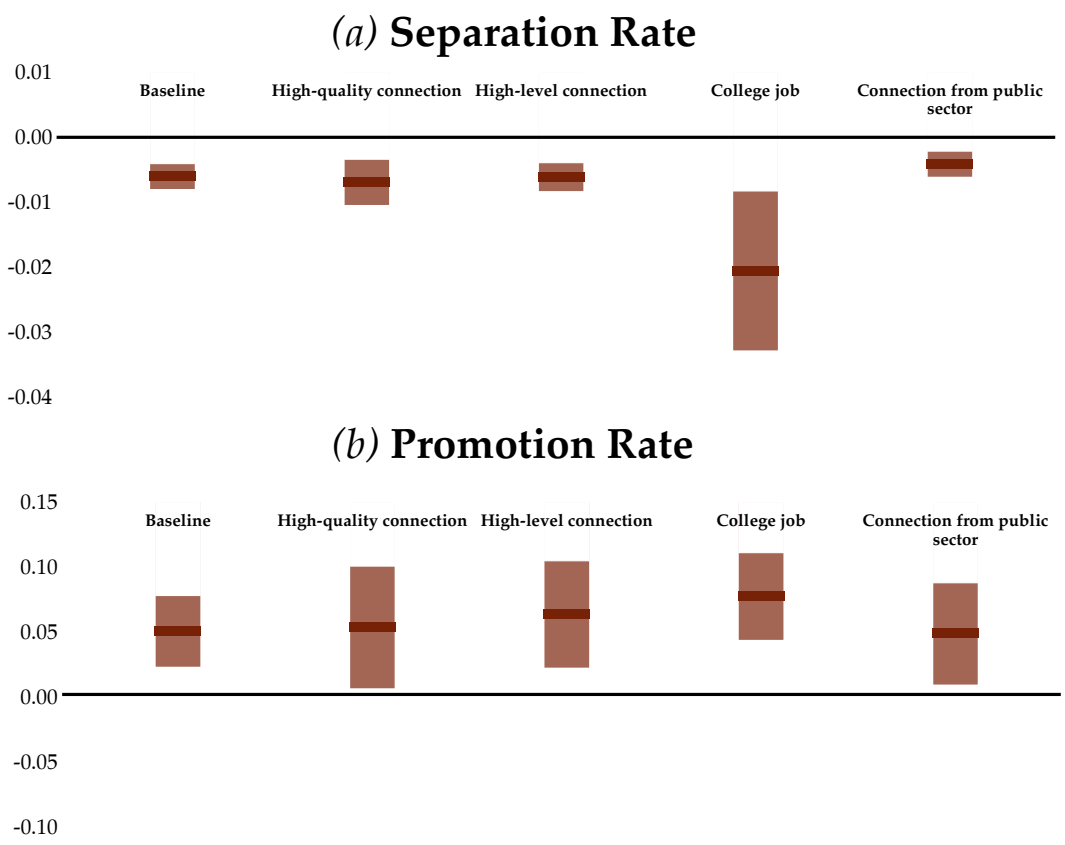
Figure 6: Empirical Strategy for Identifying the Effects of Connections on Wages for Rejected Applicants



Comparing wages of connected and unconnected applicants who fail the same job process

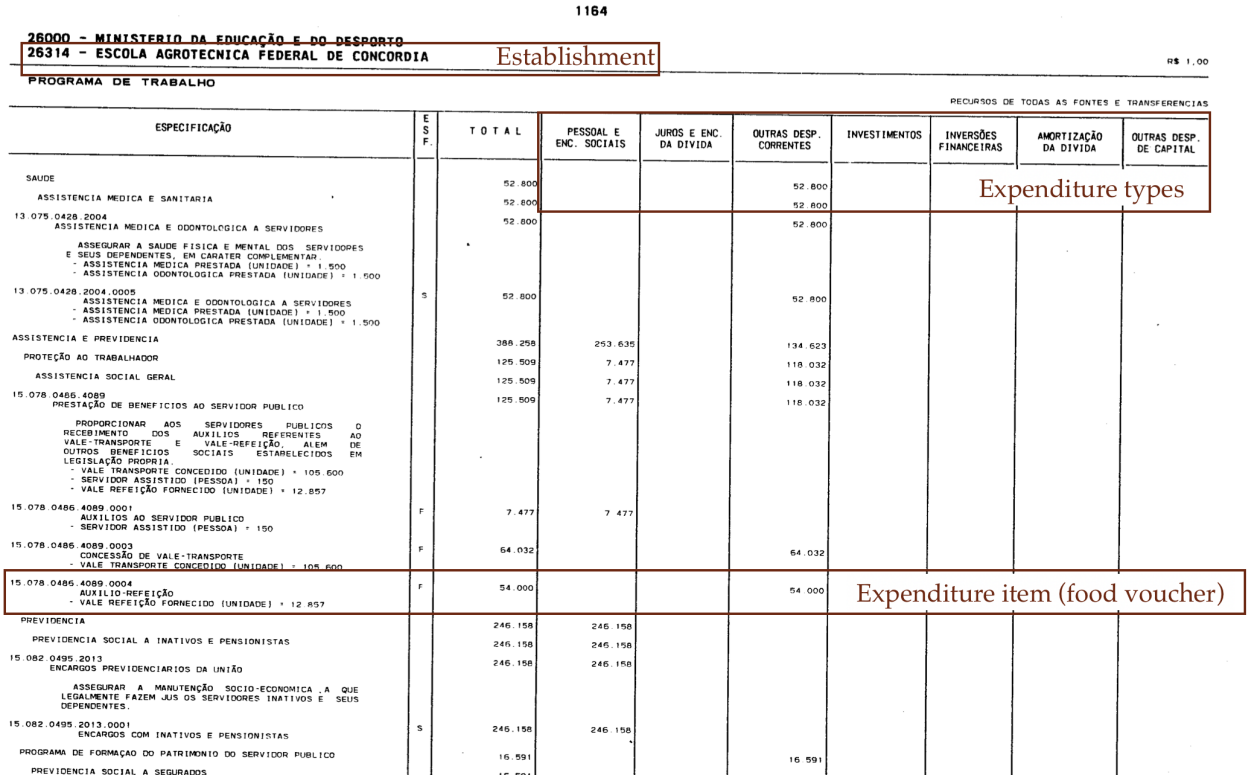
Notes: This figure illustrates our empirical exercise that compares ex-post wages of connected ($wage_1^A$) and unconnected ($wage_3^B$) applicants who fail a job process with "counterfactual" wage ($wage^{public}$). This is implemented by model $wage_{i,j(e),m\neq e,\tau\geq t} - wage_{j(e),t} = \delta_0 \mathbb{1}\{\text{Connected}_i\,(e)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t\geq 1989}\} + \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y\geq 1989} \times \text{Connected}_i\,(e)\} + \theta x_{i,e,t} + \varsigma_{j(e)} + \varsigma_t + u_{i,e,t}$ in the main text. Worker 1 has a connection to public sector establishment and applies to a job process there. Worker 2 and 3 also apply to the same job process, but neither of them have connections in that workplace. Unconnected worker 2 is hired, receiving the posted wage. Because we are able to track both the failures of workers 1 and 2, as well as their future job search behavior, we keep tracking their subsequent careers. Worker 1 ends up hired at a different employer (Employer A), while worker 3 lands a job at a third place (Employer B).

Figure 7: Heterogeneity: Effects of On-the-Job Worker Outcomes



Notes: This figure shows heterogeneous treatment effects of on-the-job outcomes for hires before and after the reform in Brazil's federal and state governments. The first bar in panels (a) and (b) show baseline estimates in columns (1) and (7) of Table 11 for comparison purposes. High-quality connections subsamples those hires whose connections' unobserved ability (Mincer residuals) are above the 75th percentile, while high-level connections are those earning above the 75th percentile in the establishment. College job indicates connections with a higher degree and finally connections from public sector track only those who previously worked at another government employer.

Figure 8: Itemized Budget Execution: Example



Notes: This figure shows an example of a snippet of the budget execution of a establishment in Brazil's federal government (*Escola Agrotécnica de Concórdia*) for the year 1995. Expenditure types include wage bill and social security, debt service, other current expenditures, investments, financial operations, debt amortization, and other capital expenditures. Rows in the first column are various expenditure items for that fiscal year.

Table 1: Descriptive Statistics

	Federal	States
Public sector job applicants and new hires:		
%Connected hires	11%	34%
%Connected applicants	13%	17%
Public sector new hires:		
Separation rate	2%	2.2%
Promotion rate	10%	13%
%College	38%	24%
%Connected with college	48%	42%
Applicants to public jobs:		
Hiring gap (connected−unconnected), p.p.	1.2	1.7
College gap (connected−unconnected), p.p.	6	3.2
#Connections in establishment	1.4	
#Establishments with connections	1	
Time sharing workplace with connection	7 months	

Notes: This table shows descriptive statistics (sample means) of selected variables for Brazil's federal and state government labor markets during 1986-1988. Following the main text, workers are connected if when they apply to a public sector job they have at least one connection there. Two workers are connected if they worked in the same establishment at a previous point for at least one month. Separation rates count both employee and employer-initiated splits. Promotion rates are measured as a permanent change in their contract wage of at least 10% in excess to the average wage change of workers in that establishment. A promotion in Brazil's public sector imply a move from one career bracket to another. Share of college connected measures the proportion of all connected individuals who are college-educated.

Table 2: Estimates of Effects on Application and Hiring Probabilities

	P (Connected = 1|Applicant)				P (Hired = 1|Applicant)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post×Treated	−0.024***	−0.023***	−0.019***	−0.019***				
	(0.006)	(0.007)	(0.005)	(0.005)				
Post×Treated×Connected					−0.003***	−0.002***	−0.001**	−0.001***
					(0.0002)	(0.0005)	(0.0006)	(0.0006)
Baseline in federal sector ($t < 1989$):								
%Connected applicants			11%					
Hiring prob. connected−unconnected						2% − 0.8%		
Observations (millions)	3.3	3.1	2.7	3.1	3.3	3.1	2.7	3.1
Fixed effects:								
Year	✓				✓			
Establishment	✓			✓	✓			✓
Year×establishment		✓	✓			✓	✓	
Occupation			✓				✓	
Year×occupation				✓				✓

Notes: The first three columns implements $P(\text{Connected=1} \mid \text{Applicant})_{i,e,t} = \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y \geq 1989}\} + \theta x_{i,e,t} + u_{i,e,t}$ with added fixed effects $\varsigma_e + \varsigma_t$ in column (1), $\varsigma_e \times \varsigma_t$ in column (2), and $\varsigma_e \times \varsigma_t + \varsigma_o$ in column (3). Columns (4) through (6) implement the triple-difference specification $\Pr(\text{Hired=1} \mid \text{Applicant})_{i,e,t} = \delta_0 \mathbb{1}\{\text{Connected}_i\,(e)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t \geq 1989}\} + \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y \geq 1989} \times \text{Connected}_i\,(e)\} + \theta x_{i,e,t} + u_{i,e,t}$ with added fixed effects in the same order as the first three columns. Treated establishments are those in the federal sector and untreated establishments are from state governments. Worker i has connections in establishment e if they shared a workplace sometime in the past. Controls include gender, age, and education. Standard errors are clustered at the establishment level. The joint tests measure whether we fail to reject that every pre-reform estimated effects in a dynamic model are zero; and whether we reject that all after-reform estimates are zero.

Table 3: Effect on Job Search

	# Applications		Application take-up	$\mathbb{1}\{\#\text{ Applications} = 1\}$	$\mathbb{1}\{\text{Applies to similar occupation}\}$
	(1)	(2)	(3)	(4)	(5)
Post×%Connections in federal	0.081***	0.087***	0.051**	−0.002***	0.001
	(0.023)	(0.024)	(0.026)	(0.001)	(0.001)
Fixed effects:					
Year	✓	✓	✓	✓	✓
Municipality		✓			
State			✓		

Notes: The interaction term *%Connections in federal* is computed as follows for applicant i: we select from all public sector establishments hiring in that year those the worker has a connection: $\sum \mathbb{1}\{\text{Connected}_i\,(e)\}$, regardless of whether the worker applies to any of them. We then filter those establishments in the federal sector to compute the variable: $\frac{\sum \mathbb{1}\{\text{Connected}_i\,\left(e^{federal}\right)\}}{\sum \mathbb{1}\{\text{Connected}_i\,(e)\}}$. This measures the proportion of potential connections that a worker can tap into in the federal sector for every year. Establishments where a worker may have a connection but that do not hire in year t are not counted in that year. Column (1) has as outcome the total number of applications across public sector establishments, while (2) runs the same specification but with added municipality fixed effects. Column (3) regresses application take-up, calculated as the ratio of applications to qualified job processes, which include all those in the same state of work and with the educational requirement cleared. Column (4) has as outcome the probability that the worker applies to only one job, while the last column the probability the worker applies to a job with the occupational code close to her current employment.

Table 4: Estimates of Effects on Application and Hiring Probabilities

	P (Connected = 1|Applicant)				P (Hired = 1|Applicant)			
Job requires...	No College (1)	College (2)	No College (3)	College (4)	No College (5)	College (6)	No College (7)	College (8)
Post×Treated	−0.011***	−0.035***	−0.014***	−0.032***				
	(0.003)	(0.004)	(0.003)	(0.002)				
Post×Treated×Connected					−0.001**	−0.002***	−0.002***	−0.002***
					(0.001)	(0.001)	(0.001)	(0.001)
Baseline in federal sector ($t < 1989$):								
%Connected applicants	9%	17%						
Hiring prob. connected−unconnected					6% − 4.6%	1.1% − 0.5%		
Observations (millions)	2.1	1.0	2.2	1.0	2.1	1.0	2.2	1.0
Fixed effects:								
Year×establishment	✓	✓			✓	✓		
Establishment			✓	✓			✓	✓
Year×occupation			✓	✓			✓	✓

Notes: This table implements model $P(\text{Connected=1} \mid \text{Applicant})_{i,e,t} = \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y \geq 1989}\} + \theta x_{i,e,t} + \varepsilon_e \times \varepsilon_t + u_{i,e,t}$ in columns (1) - (4) and $\Pr(\text{Hired=1} \mid \text{Applicant})_{i,e,t} = \delta_0 \mathbb{1}\{\text{Connected}_i\,(e)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t \geq 1989}\} + \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y \geq 1989} \times \text{Connected}_i\,(e)\} + \theta x_{i,e,t} + \varsigma_e \times \varsigma_t + u_{i,e,t}$ in columns (5) - (8). The odd columns (1), (3), (5), and (7) only consider public sector jobs that do not require a college degree, while the even columns restrict the estimation sample to those jobs requiring college. Treated establishments are those in the federal sector and untreated establishments are from state governments. Worker i has connections in establishment e if they shared a workplace sometime in the past. Controls include gender, age, and education. Standard errors are clustered at the establishment level. The joint tests measure whether we fail to reject that every pre-reform estimated effects in a dynamic model are zero; and whether we reject that all after-reform estimates are zero.

Table 5: Robustness: Estimates of Effects on Application and Hiring Probabilities

Applicant is...	Connected$^{\text{placebo est}}$	Connected$^{\text{placebo emp}}$	Connected	P (Hired $= 1$)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post×Treated	0.0004	0.001	−0.017**			
	(0.012)	(0.005)	(0.008)			
Post×Treated×Connected (placebo est)				0.0004		
				(0.016)		
Post×Treated×Connected (placebo emp)					0.002	
					(0.003)	
Post×Treated×Connected						−0.001**
						(0.0006)
Fixed effects:						
Year	✓	✓		✓	✓	
Establishment	✓	✓		✓	✓	
Year×establishment			✓			✓
Previous industry			✓			✓

Notes: This table implements alternative versions of the paper's baseline difference-in-differences models. Columns (1) and (4) construct *placebo* connections using workers in the same establishment but in different periods: worker i applying to a public sector job has placebo connections in that establishment if those "connections" worked in the same establishment but left before the worker began employment. Columns (2) and (5) construct an alternative placebo connection measure based on *different* establishments of the same employer and in different periods. This allows individuals to work for the same employer but not share the same workplace. Columns (3) and (6) use the baseline definition of connections (shared workplace prior to a worker's application), but includes a fixed effect for the previous sector of the worker. These specifications compare connected and unconnected workers applying to the same public sector establishment with previous employment in the same industry. The model in the first three columns is $P(\text{Connected=1} \mid \text{Applicant})_{i,e,t} = \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y \geq 1989}\} + \theta x_{i,e,t} + \varepsilon_e \times \varepsilon_t + u_{i,e,t}$, with ε_s added in column (3), and the last three columns implement $\Pr(\text{Hired=1} \mid \text{Applicant})_{i,e,t} = \delta_0 \mathbb{1}\{\text{Connected}_i\,(e)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t \geq 1989}\} + \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y \geq 1989} \times \text{Connected}_i\,(e)\} + \theta x_{i,e,t} + \varsigma_e \times \varsigma_t + u_{i,e,t}$ with the additional fixed effect in column (6). Treated establishments are those in the federal sector and untreated establishments are from state governments. Controls include gender, age, and education. Standard errors are clustered at the establishment level.

Table 6: Estimates of Effects on Wages

Wage...	Public Sector Hires (1)	After Failing Job Process (2)
Post×Treated×Connected	−0.004***	−0.247***
	(0.001)	(0.065)
Baseline in federal sector ($t < 1989$):		
Wage gap connected−unconnected in same establishment	7.2−5.6	
Wage gap connected−unconnected		6.1−4.8
Observations (millions)	2.7	3.0
Fixed effects:		
Year×establishment	✓	
Year		✓
Job process		✓

Notes: The first model is $wage_{i,e,t} = \delta_0 \mathbb{1}\{\text{Connected}_i(e)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t\geq 1989}\} + \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y\geq 1989} \times \text{Connected}_i(e)\} + \theta x_{i,e,t} + \varsigma_e \times \varsigma_t + u_{i,e,t}$.
The second model is $wage_{i,j(e),m\neq e,\tau\geq t} - wage_{j(e),t} = \delta_0 \mathbb{1}\{\text{Connected}_i(e)\} + \delta_1 \mathbb{1}\{\text{Treated}_e \times \text{Post}_{t\geq 1989}\} + \beta \mathbb{1}\{\text{Treated}_e \times \text{Post}_{y\geq 1989} \times \text{Connected}_i(e)\} + \theta x_{i,e,t} + \varsigma_{j(e)} + \varsigma_t + u_{i,e,t}$. The estimating sample spans 1986 to 1992. Treated establishments are those in the federal government, while untreated are in state governments. Controls include gender, education, age, and other worker characteristics. Standard errors clustered at the establishment level in the first column and job process in the second. We measure wages in multiples of Brazil's federal minimum wage, which binds for all formal jobs in the country.

Table 7: Estimates of Effects on Quality of Job Applicants

Quality of job applicants	Observable				Unobservable			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post×Treated	0.366***				0.487***			
	(0.102)				(0.163)			
Post×Treated×Connected		0.649***	0.281***	0.188**		0.933***	0.494***	0.222**
		(0.247)	(0.280)	(0.291)		(0.341)	(0.191)	(0.095)
Observations (millions)	2.8	1.8	1.8	1.0	2.8	1.8	1.8	1.0
Fixed effects:								
Year	✓	✓			✓	✓		
Establishment	✓	✓			✓	✓		
Year×establishment			✓	✓			✓	✓
Job process				✓				✓

Notes: The estimating sample spans 1986 to 1992. Treated establishments are those in the federal government, while untreated are in state governments. Controls include gender, education, age, and other worker characteristics. Standard errors clustered at the establishment level in the first column and job process in the second. The joint tests measure whether we can fail to reject that every pre-reform estimated effects in a dynamic model are zero; and whether we reject that all after-reform estimates are zero.

Table 8: Estimates of Effects on Quality of Hired Employees

Quality of hired employees	Observable			Unobservable		
	(1)	(2)	(3)	(4)	(5)	(6)
Post×Treated	0.214***			0.426***		
	(0.077)			(0.089)		
Post×Treated×Connected		0.320***	0.198**		0.712***	0.496**
		(0.088)	(0.081)		(0.279)	(0.230)
Observations (thousands)	232	81	81	232	81	81
Fixed effects:						
Year	✓	✓		✓	✓	
Establishment	✓	✓		✓	✓	
Year×establishment			✓			✓

Notes: The estimating sample spans 1986 to 1992. Treated establishments are those in the federal government, while untreated are in state governments. Controls include gender, education, age, and other worker characteristics. Standard errors clustered at the establishment level in the first column and job process in the second. The joint tests measure whether we can fail to reject that every pre-reform estimated effects in a dynamic model are zero; and whether we reject that all after-reform estimates are zero.

Table 9: Nonlinear Effects on Quality of Job Applicants (Mincer residuals)

Applicant belongs to... (within job process)	Top Quality Quartile	Bottom Quality Quartile
Post×Treated×Connected	0.004	−0.091***
	(0.006)	(0.035)
Observations (thousands)	877	
Fixed effects:		
Year×establishment	✓	✓
Job process	✓	✓

Notes: The estimating sample spans 1986 to 1992. Treated establishments are those in the federal government, while untreated are in state governments. Controls include gender, education, age, and other worker characteristics. Standard errors clustered at the establishment level in the first column and job process in the second. The joint tests measure whether we can fail to reject that every pre-reform estimated effects in a dynamic model are zero; and whether we reject that all after-reform estimates are zero.

Table 10: Comparing Quality of Connected Job Applicants and Hires Around the Reform

Panel A — Observed Quality

Quality	Job Applicants	
	Before Reform	After Reform
Unconnected	0.81	0.77
Connected	0.34	1.20

Quality	Hired Workers	
	Before Reform	After Reform
Unconnected	1.01	1.07
Connected	0.98	1.43

Panel B — Unobserved Quality

Quality	Job Applicants	
	Before Reform	After Reform
Unconnected	0.09	0.13
Connected	0.05	0.44

Quality	Hired Workers	
	Before Reform	After Reform
Unconnected	0.63	0.68
Connected	0.57	0.96

Notes: We compare unobserved quality for workers with and without personal connections applying or hired in the federal sector. Quality is measured as private sector wage residuals from a fully saturated Mincer regression (age, education, sector) and municipality and year dummies of employment previous to applying to the public sector job. A personal connection tracks a previously shared a workplace between an employee of the target public sector establishment and the job applicant/hired worker. A number of 1 in the table measures one monthly minimum wage.

Table 11: Effects of On-the-Job Worker Outcomes

Ever...	Separates			Has Pay Suspended			Promoted		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post×Treated	−0.006***			−0.047***			0.051***		
	(0.001)			(0.013)			(0.014)		
Post×Treated×Connected		−0.014***	−0.010***		−0.092**	−0.091*		0.131***	0.149***
		(0.001)	(0.002)		(0.070)	(0.080)		(0.060)	(0.054)
Baseline in federal sector ($t < 1989$):									
%Separation		2							
%Pay suspended					6				
%Promoted								10	
Observations (thousands)	233	233	211	82	82	36	176	176	161
Fixed effects:									
Year	✓	✓		✓	✓		✓	✓	
Establishment	✓	✓		✓	✓		✓	✓	
Year×establishment			✓			✓			✓

Notes: The estimating sample spans 1986 to 1992. Treated establishments are those in the federal government, while untreated are in state governments. Controls include gender, education, age, and other worker characteristics. Standard errors clustered at the establishment level in the first column and job process in the second. The joint tests measure whether we can fail to reject that every pre-reform estimated effects in a dynamic model are zero; and whether we reject that all after-reform estimates are zero.

Table 12: Effects on Establishment Budget Efficiency Measures

Budget Execution...	Shortfall	% Mandatory Spending	%Investment	%Core Activity
% Unconnected Hires (instrumented with Post×Treated)	−0.784*** (0.294)	−0.005** (0.003)	0.019** (0.008)	0.107*** (0.030)
Observations	2,118	2,118	2,118	1,907
F-statistic	121.33	111.07	69.40	108.77
Fixed effects:				
Year	✓	✓	✓	✓
Establishment	✓	✓	✓	✓

Notes: This table shows results for a specification that instruments the share of connected new hires in a public sector establishment with the impartiality reform to study budgetary efficiency in that workplace. Budget efficiency is measured (i) as the gap between the annual budget spent and requested amounts, (ii) % of executed budget allocated to mandatory items (social security, wage bill, and debt service), (iii) % of executed budget in investments, and (iv) % of non-mandatory executed budget ("free cash") on core activities, measured by a correspondence between itemized expense keywords and the sector or activity of the establishment. We only consider cases with shortfall bounded from above by 1 (we drop establishments that obtain further appropriations during the fiscal year and therefore spend "more" than the fiscal year initial budget allocated). The estimating sample spans 1987 to 1993. Standard errors clustered at the establishment level. In the few cases where establishments do not have discretized budgets assigned, we aggregate them up to the employer level.