



**The impact of discretionary hiring on bureaucrats' corruption perceptions,  
satisfaction, and the mishandling of public resources**

**Author**

Andrés Yesid Calderón Amaya

**Thesis**

M.Sc. in Economics

**Advisor**

Mounu Prem

Facultad de Economía  
Maestría en Economía  
Universidad del Rosario

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# The impact of discretionary hiring on bureaucrats' corruption perceptions, satisfaction, and the mishandling of public resources

Andres Calderon<sup>\*</sup>

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This paper examines the impact in a developing country of adopting a system of selection of civil servants through discretionary and temporary contracts on three aspects of the public sector: corruption perceptions of bureaucrats, job satisfaction, and the mishandling of public resources. I study the impact of a legislative reform that incentivized certain Colombian public agencies to hire personnel using the direct-procurement regime. Relying on a *difference-in-differences* design, I find a relative increase in the awareness of nepotism, the influence of political networks, and of money and favors in the selection processes of territorial-order entities, which were the most affected by the reform. Civil servants in these agencies also experienced a differential decrease in job satisfaction, and the institutions were more likely to appear in bulletins of sanctions for mishandling of resources. Household data suggests an actual deterioration of meritocracy, and triple difference analyses suggest that perceptions of unfairness in hiring practices might drive the effects on perceptions and satisfaction.

JEL Codes: D73, J45, M50

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# 1 – Introduction

The growing demand for public goods and services in a developing country requires an improvement in the quality of individuals employed in the public sector, as they are an essential factor explaining government performance (Finan et al., 2015; Besley et al., 2021). This quality, known as “the human capital of the state”, depends on aspects like selection or hiring, promotion and retention, and other incentives to bureaucrats. Two approaches have dominated the debate on the building of civil service. The first proposes recruitment through a competitive system and employee protection from political dismissals. The other one allows politicians to retain discretion in the selection process. Scholars have discussed the advantages and disadvantages of the two systems<sup>1</sup>, and in practice, they both coexist in all governments with different degrees of importance (Evans and Rauch, 1999).

What happens to public agencies and their personnel when discretionary practices in the selection processes increase? The negative effect on meritocracy might raise corruption perceptions, eroding trust among civil servants. This can lead to low job satisfaction and ultimately to an efficiency decline in the public sector. A growing body of research has focused on how nepotism or patronage impact government performance (Riaño, 2022; Xu, 2018; Voth and Xu, 2022). However, scarce data on the incentives of public employees has limited the understanding on the implications of meritocracy and its deterioration on the motivations of civil servants.

In this paper, I study the effect in a developing country of passing from a rigid hiring system in the public sector to another where discretion through noncompetitive and temporary contracts becomes the norm. I specifically look at the impact of a 2000 law that prohibited certain agencies from increasing their operating personnel

1. A branch of the empirical evidence argues that a professional civil service is associated with higher economic growth, lower corruption, and incidence of patronage (Dahlström et al., 2012; Muñoz and Prem, 2022; Ornaghi, 2019). However, this system usually lacks incentive schemes crucial to bureaucratic and political performance (Lazear and Shaw, 2007; Finan et al., 2017; Lewis, 2008).

expenditures in real terms while imposing fewer or no restrictions on the rest of the agencies. Specifically, the law targeted the local (or territorial) executive-branch government entities: *gobernaciones* (for the subnational department level) and *alcaldías* (for the municipality level), which manage resources at the subcountry level and are responsible for the provision of essential public services in the areas of education, health, drinking water, and sanitation (Alesina et al., 2005).

These entities started using short-term contracts that fall into the direct procurement regime (meaning they omit tenders or civil service calls) to circumvent the legislation and the fact that there were no meritocratic processes to recruit local-level government employees before 2020. The result: civil servants in this government sector had a differential increase in the exposure to discretionary hiring practices, relative to those in the control group of agencies (i.e., those of other government branches or at the national level), which had fewer restrictions on hiring staff and benefited from competitive selection processes.

I evaluate the effect of the 2000 law on three aspects of the Colombian public sector: the perceptions of corruption in the hiring practices by public employees, the satisfaction with public employment, and the mishandling of public resources. I hypothesize that this change in the legislation decreased meritocracy in the affected agencies, which eroded trust among bureaucrats (measured by corruption perceptions). Keefer and Vlaicu (2022) find that trustful public employees in Latin America have a higher mission motivation, are more willing to collaborate with colleagues, and are more open to innovation, three factors that determine overall employee performance. I use this evidence to argue that the reform potentially led to decreased job satisfaction and the eventual deterioration of public spending efficiency.

To explore the impact in the first two aspects, I use a *difference-in-differences* (diff-in-diff) design in repeated cross sections that compares the evolution of perceptions of corruption and job satisfaction of public employees that entered territorial (local)-level agencies after 2000, relative to those that entered the national-order

government or agencies outside the executive branch. Using data from a survey that is applied exclusively to staff public employees (and that excludes contractors), I find that public employees in the local governments experienced a differential increase in corruption perceptions after the change in legislation, as measured by their awareness of the influence of personal links (which can be interpreted as a proxy for nepotism), political networks, and money in the selection processes. Depending on the measure, the effect's magnitude represents between 4.5% and 25.2% of the outcomes' averages before the law. The estimated impacts are robust to the inclusion of survey participant controls and fixed effects at the survey year, the cohort of entry, and the sector of the entity levels.

For the second aspect, I use a subsample of public employees (including contractors) in household survey data that includes questions on whether the respondent desires to switch jobs, if she feels that her skills and training are being underused, and if she is satisfied with her job and three additional characteristics: the wage, the contract, and the workday. Civil servants of the territorial level government experienced a differential reduction in all measures of job satisfaction, although the impact on the job itself is inconclusive. The magnitudes of the estimates are substantial and robust, as they represent between 12.69% and 25% of the outcomes' standard deviations before 2000 and do not change when controlling for the employees' sex, age, or education level.

Finally, for the third aspect, I apply the diff-in-diff design to a panel with entity-level measures of sanctions for mishandling of public resources by the Office of the Comptroller General. Using data spanning 1995–2018 period, I find that the probability of appearing in the sanctions list differentially increased for territorial-level agencies of the executive branch, which suggest that the law negatively affected the efficiency of the spending in the treated agencies. The magnitude of the impact is substantially large compared to pre-reform salience of the problem<sup>2</sup>. The evidence

2. Only 6.7% of the agencies appeared in the sanctions data before 2000, so the impact represents about 87% of this fraction and around 23% of the standard deviation (0.25).

for other intensive-margin measures (including the number of appearances or sanctioned individuals) is weaker than for the dichotomous one.

I then investigate two possible mechanisms associated with the differential increase in corruption perceptions and the reduction in job satisfaction. The first one is the actual deterioration of the hiring process in treated agencies, where I corroborate if noncompetitive hires became more common after the change in legislation. Household survey data in Colombia includes a question to employed individuals about the primary way in which they got their jobs, allowing me to compare the evolution of different sources of public employment access after 2000. I find a relative increase in the probability of entering via help from family, friends, and colleagues for those in the territorial-level government, compared to civil servants in other levels or branches of the government. At the same time, I find a reduction in the likelihood of entering via civil service calls or announcements, which are more competitive selection instruments. The magnitudes of both impacts are substantially large: they range from 39% to 41% of pre-reform averages. I interpret the first result as evidence of an increase in nepotism, and the second as an overall erosion of the meritocratic process.

The second mechanism is related to the perception of fairness. I hypothesize that higher exposure to discretionary hiring practices might develop a belief that the selection processes in the public sector are unfair, especially for more educated and experienced public employees (since they have faced higher financial and time costs and probably have higher expectations about the quality of the civil service). Using a triple difference design, I find that the increase in perceptions of corruption after the 2000 change in legislation is higher for those with graduate education relative to those with lower levels of formal training. I also find that more experienced civil servants, measured by their years contributing to a pension, experienced further decreases in their measures of job satisfaction. However, the evidence is only conclusive for satisfaction with job characteristics.

This paper contributes to two branches of the recent body of research on the personnel economics of the state (Finan et al., 2015; Besley et al., 2021). The first one covers the effects of civil service reform. Muñoz and Prem (2022) find that a reform that centralized and improved competition and transparency in the election of public school principals in Chile increased the effectiveness of these “street-level” managers. Moreira and Pérez (2022) study the impacts of an 1883 act in the United States that introduced competitive exams for selecting certain federal employees, including some at the Customs Service agency. They find that this reform reduced turnout and improved the professional background of the hired public servants. However, it did not impact the cost-effectiveness of revenue collection. They argue that this unexpected result is caused by a perverse incentive to increase hiring in excepted, low-paid positions, which play an essential role in the performance of the customs districts. Finally, Riaño (2022) identifies that a 2015 anti-nepotism legislation in Colombia had limited effects as just 13% of the targeted civil servants left the public administration, whereas the remaining percentage reshuffled within entities or did not respond to the reform.

The second branch of the literature studies the impacts of different selection mechanisms on the performance of the public sector. Xu (2018) finds that politically connected governors during the British Empire generated less government revenue, provided more tax exemptions, and supplied less expenditure. Xu (2019) analyzes the long-term effects of this historical phenomenon and claims that modern-day countries exposed to more patronage governors have lower tax-to-GDP rates. In contrast to these studies that rely on cross-country comparisons in the colonial context, Dahis et al. (2022) argue that competitive examinations can effectively screen candidates by focusing on the hiring process of state judges in Brazil.

The contribution of this paper is twofold. First, I study the effects of a civil service reform that affected the complete public administration in a developing country, in contrast to Moreira and Pérez (2022); Muñoz and Prem (2022); Dahis et al. (2022),

and Xu (2018), that focus on specific positions or servants (i.e., customs officers, schools principals, and judges). Data from the Worldwide Bureaucracy Indicators (WWBI) (World Bank, 2022) shows that public administration workers account for almost half of the total public employees, and their importance has been increasing in the last two decades<sup>3</sup>. Second, the literature has also focused on how patronage affects public performance, measured by tax collection, government expenditure, or public schools' quality. However, few studies explore other outcomes, such as corruption perceptions and public employees' motivations, the latter being a determinant of performance itself (Ashraf et al., 2014; Dixit, 2002).

The paper is related to a study investigating the effect of noncompetitive and temporary contracts on the quality of public employment in Colombia. Sanabria Pulido et al. (2019) also use household survey data to explore differences in wage, the number of hours worked, job stability, and satisfaction between those hired using this type of contract and those that are not. They instrument the treatment status by exploiting the year of entry (before or after 2007), and find that public employees with discretionary contracts work fewer hours per week, have a lower monthly wage, lower satisfaction with job characteristics, and lower job stability. They are also more willing to change jobs. I add to these results by exploring two mechanisms behind the impact. I also exploit the differences in the degree of exposure to contracts between entities, which allows looking at their effects on agency-level data where contractors are not identified. This is, for instance, the case of the survey that contains questions on corruption perceptions.

The rest of the paper is organized as follows: section 2 describes the institutional context of the selection processes in the Colombian public sector. Section 3 summarizes the data sources and defines the outcomes of interest. Section 4 outlines the empirical strategy to estimate the effect of the change in legislation on the three aspects of interest, and section 5 reports the main findings and robustness. Finally,

3. The number of public administration workers as a share of total public employees increased from 41.4% in 2000 to 48.07% in 2019, which is the last year in the WWBI.

section 6 discusses the potential mechanisms driving the effect, and section 7 concludes.

## 2 – Institutional Context: Hiring in the Colombian Civil Service

The increasing importance of the Colombian public sector in the economy, as measured by the participation of the government expenses in the GDP, has not been translated into a growth of its civil service. Figure 1 shows the evolution of total expense and the compensation of public employees between 1998 and 2019 in the central (national-level) and territorial (local-level) governments (comprising departments and municipalities). The first variable increased respectively by 55 and 71% during the 2000-2019 period in the two levels of the government. In contrast, payroll grew by 9% in the central government and decreased by 3.5% in the local one. These dynamics have turned Colombia into a country with a small public workforce either when considering the number of civil servants or when using the payroll (Bernal et al., 2017). By April 2021, the country had about 1 million 300 thousand public employees, of which 56% were police officers, soldiers, or teachers.

Two laws have contributed to the personnel shortage problem in the Colombian public sector. In the context of the 1999 economic crisis, the government introduced Law 617 in 2000, targeting territorial order entities. Aiming to improve the financial situation of the local corporations, article 92 of this law stated that personnel costs (which are included in the operating budget of agencies) should grow by less than 90% of the inflation rate in 2005 and could not increase in real terms afterward. This article was removed in 2019.

A similar law for the executive branch of national-order entities was launched two years later. Law 790 of 2002 gave former President Alvaro Uribe the power to suppress entities and positions, and its regulating decree (Decreto Nacional 190 de

2003) established in article 18 that the positions of civil servants retiring by 2006 would be removed. The law also limited the increase in personnel costs to lower than inflation from 2002 to 2005 (article 19). It also prohibited the replacement of these positions with personnel services contracts (article 17)<sup>4</sup>.

To circumvent these two laws, entities in the executive branch (both at the territorial and the national level) have been using personnel services contracts (colloquially known as *Órdenes de Prestación de Servicios*, or OPS) (OECD, 2013). In July 2007, Congress passed Law 1150, which reformed the procurement statute of the Colombian state. This law established that personnel services contracts could be signed via direct procurement and thus are excepted from tenders, public calls, or merit based-competitions. Sanabria Pulido et al. (2019) argue that this change in legislation caused a surge in the use of this type of contract for selecting public employees.

Personnel services contracts are usually financed by investment resources and thus are not part of the operating budget of the entities (Bernal et al., 2017). They operate under different labor regulations, so providers are considered self-employed and they are fully responsible for their retirement contributions, in contrast to staff public employees, where the employer pays 75% of the contributions. They are also characterized by a very short-term duration and high discretion, given that they can be made through direct procurement. Using data from the Colombian electronic system of public procurement (SECOP), I find that half of the OPS contracts signed between 2000 and 2021 lasted four months, and almost 80% belonged to the direct procurement regime. Zuleta and Caro (2020) claim that some of these contracts (usually those signed for less than three months) are used to exchange political favors, while those signed for more than a year correspond to jobs that are crucial for the functioning of the entity.

I use the increasing importance of these contracts for the selection of civil ser-

4. Article 17 also states that in the case that the entity needs to hire through personnel services contracts, the minister or the head of the department will present a report to the Congress.

vants after 2000 to explore whether it caused a change in three aspects of the Colombian public sector: the perceptions of corruption in the hiring practices by public employees, the satisfaction with the public employment, and the mishandling of public resources. The new legislation had differential impacts on enrollment in the entities depending on their level (national versus territorial) and branch. Those out of the executive branch (e.g., in the defense or education sectors) were excluded from the restrictions to hire employees because most have special selection regimes. Agencies of the national-order executive branch faced the restriction of article 17 of Law 790, stating that the removed jobs could not be replaced using personnel-services contracts. They also have disproportionately benefited from meritocratic processes: since 2014, 93% (2,304 out of 2,365) of the meritocratic processes carried out by the Department of the Civil Service (*Departamento de la Función Pública*) were conducted for the national-order agencies.

I argue that entities of the territorial-level executive branch were the most exposed to this change in legislation. Law 617 of 2000 prohibited them from having real growth in payroll for almost 20 years until the removal of article 92 in 2019. They began to benefit from meritocratic processes in 2020, where 38 competitive recruitments were carried out, representing 1.61% of the processes during the 2014-2020 period. However, in contrast to article 17 of law 790 for the national-order entities, they did not face a restriction on the use of personnel services contracts. Law 1150 in 2007 then represented an opportunity for the governments of departments and municipalities to hire labor without violating Law 617. It also allowed them to do it directly and use investment resources instead of the strict operating budget. By 2016, OPS contracts represented almost half of total employment in the territorial-order entities, but only 7.1% of the labor force in national order entities (25% excluding teachers and the public force) (Bernal et al., 2017).

I provide evidence supporting that Law 617 generated a differential increase in the use of discretionary and temporary contracts. I use a *Difference-in-Differences*

model that compares the change in the probability of being hired via this type of contract after 2000 in the executive branch of the territorial government, relative to other government branches and to national-order agencies<sup>5</sup>. Relying on monthly household survey data spanning the period 2010–2018, I estimate the following model in the subset of public employees:

$$DiscContract_{isdct} = \alpha_t + \beta_{sd} + \gamma_c + \delta(LocalEntity_{sd} \times After2000_c) + \varepsilon_{isdct} \quad (1)$$

where  $i, s, d, c,$  and  $t$  respectively indicate the survey participant, the 4-digit economic sector, the department where she works<sup>6</sup>, the year in which she entered her current job, and the period of the survey (composed by month and year). The dependent variable,  $DiscContract_{isdct}$  is a dummy that proxies whether the employee holds a discretionary and temporary contract.  $\delta$  thus measures the differential change in the probability of being hired via this type of contract in treated agencies –executive-branch territorial-order agencies– after 2000, relative to the rest of the public sector.

Table 4 reports the estimation of this parameter for three proxies: being self-employed, fully paying the contributions for retirement, and having a fixed-term contract. The coefficients point out that after Law 617 was passed, the probability of having a fixed-term contract increased by 29.5 percentage points (pp) for employees in treated agencies relative to those at the national level or in other branches of the government. The effect of being self-employed and fully paying retirement contributions is respectively estimated at 33.9 and 31.2 pp. The coefficients are statistically significant and substantially large, given that only 3.5% of the public employees that entered before 2000 have fixed-term contracts, 1.31% fully pay re-

5. Section 3 provides a profound description of the data, including the definition of the treatment groups. Section 4 describes the intuition and technical details of the Diff-in-diff design. Refer to equation 3 for a better description of this identification strategy’s application to the household survey data.

6. Departments in Colombia refer to the principal subnational jurisdictions. The country is made up of thirty two departments and the Capital District (Bogotá)

tirement contributions and 1.12% are self-employed. These results support the idea that the change in legislation indeed generated important discrepancies in how the agencies hire their workforce, with evidence that territorial-order agencies in the executive branch started to over-rely on a selection mechanism that is discretionary and temporary.

## 3 – Data

### 3.1. *Perceptions of corruptions*

I build a repeated cross-section spanning the period 2010–2018 using data from the Surveys of Institutional Environment and Performance (EDI for the national-order entities and EDID for the local-order institutions<sup>7</sup>), designed and collected by the Colombian Official Institute of Statistics (DANE). These surveys are targeted at public employees –contractors are excluded–, and contain questions related to their perceptions on crucial aspects of the entities' performance, such as the transparency in the hiring process or what influences budget construction and execution. DANE uses a stratified random sampling design, whose strata are defined at the entity and hierarchy level of the employee. There are three hierarchical levels: directive, professional, and auxiliary.

To construct the dependent variables, I focus on three questions about the perception of corruption in the hiring process. Before 2016, the surveys asked public employees how much they believed that the selection of contractors or officials of free appointment and removal<sup>8</sup> was based on (i) personal, familiar, or friendship links, (ii) on political influences, (iii) irregular payments and exchange of favors. After 2016, the questions do not make distinctions between positions.

7. In the context of the EDID, local-order institutions refers to the executive branch at the department-level and the government of Bogotá.

8. This type of public employees refers to top officials in the executive sector, such as secretaries, ministers, or deputy ministers.

I create three dummy variables based on the responses to these three questions. After 2016, the variables take the value of one if the employee strongly agrees or agrees with the statement that hiring processes were based on each of the three reasons mentioned above and zero if she disagrees or strongly disagrees with the sentence. For the period 2010-2015, the variables are equal to one if the civil servant strongly agrees or agrees with both questions about selection in the entity (for contractors and officials of free appointment and removal) and equal to zero if she strongly disagrees or disagrees in any of both questions.

Key to the identification strategy, DANE asks public employees about their experience in the agencies. One of the disadvantages of the surveys is that it does not ask respondents their exact tenure in the public entity where they work, but instead asks them to choose from the following set of bins: six months to six years, seven to eleven years, twelve to sixteen years, or more than sixteen years. This information allows me to construct categories of years of entry (characterized by lower and upper bounds), which act as cohorts in the empirical design<sup>9</sup>.

Table 1 displays summary statistics for the sample, composed of 78.2% of survey participants in national entities and 21.8% in territorial entities. 79.7% of the overall respondents entered their agencies after 2000, with statistically-significant differences in this fraction between national and territorial-order agencies. There are also significant differences in outcomes, as average corruption perceptions are higher in national-order agencies than in territorial-order. Territorial entities tend to have a higher share of women respondents and a lower percentage of survey participants with tertiary or graduate education. Besides, surveyed public employees in these agencies tend to be more in auxiliary positions, as the share of directives and professionals is higher in the national order. The fact that all differences are statistically significant suggests that there might be disparities between both levels of

9. The cohort bins are created by subtracting the upper and lower bounds of the answer from the year of the survey. For example, if a survey participant in 2012 answers that she has seven to eleven years of experience in her agency, she will belong to the “2001-2005” cohort

the public sector, including in the selection of employees. Using an empirical strategy that controls (at least for time-invariant characteristics behind these disparities) is therefore crucial for identifying the effects of the hiring processes on corruption outcomes.

### 3.2. *Job satisfaction*

For the outcomes on job satisfaction, I use the *Gran Encuesta Integrada de Hogares (GEIH)* –also conducted by DANE – to construct a repeated cross-section spanning the period 2010-2018. The GEIH is equivalent to the Current Population Survey in the United States and is applied to approximately 20,000 households monthly, mainly to measure labor market statistics.

The survey asks occupied individuals if they desire to switch jobs. I create the dummy outcome “*Switch jobs*”, that takes the value of one if the respondent answers “Yes” to that question, and zero if she answers “No”. If she indicates a desire, then GEIH asks the reasons. The first option is “to increase the use of your skills or training”, allowing me to create a dichotomous variable (referred to as “*Unused skills*” henceforth) equal to one if that is one of the reasons to switch jobs, and zero if it is not or if she does not want to change jobs. Finally, DANE also asks participants four questions about satisfaction with job-related characteristics, namely if they are satisfied with (i) the type of contract they have, (ii) their current job, (iii) the wage and benefits they receive, (iv) and the job’s workday. I construct the “*Satisfaction index*”, ranging from 0 to 4, that sums the number of characteristics the individual is satisfied with. The last satisfaction dependent variable, “*Satisfied with the job*”, equals one if the respondent is satisfied with her current job and zero otherwise.

An advantage of using this survey is that it also contains a question about the time of the employee in her current job, and individuals answer in months<sup>10</sup>. I use this information to construct the year of entry cohorts by subtracting the number

10. The specific question is “For how much time has the person been continuously working in this enterprise, business, industry, firm, or farm?”

of months from the survey period (year-month). In contrast with the cohorts in the corruption perceptions data, the ones in the GEIH are exact.

Finally, the GEIH has two sources of information on the respondent's occupation. The first one is a question that directly asks about it.<sup>11</sup> The microdata also contains the 4-digit International Standard Industrial Classification of All Economic Activities (ISIC) code. I use these and the cohort variables to filter the data and keep only public sector occupied respondents that entered their jobs in 1994 or later. A person works in the public sector if she is in Division 75 of the ISIC third revision (Public administration and defense; compulsory social security) or if she reports being a government worker. The final sample includes around 163,941 individuals.

Table 2 reports the mean and standard deviation of the outcomes and controls whose source is the GEIH. The composition of the sample by treatment status is similar to that of the EDI-EDID surveys: about 20% of the public employees work in the treatment group – they are in the executive branch of the territorial-order agencies – and 79.2% entered after 2000. Employees in the control group (those that work in other branches of the government or the national-order agencies) are generally older, have more years contributing to pension (interpreted as being more experienced), and have more years of education (although they are less likely to have tertiary education). Treated entities have more women, and their employees exhibit lower satisfaction levels. They also report a lower percentage of getting their jobs via calls and a higher rate of getting them via family and friends. These variables will be analyzed in further detail later in the paper.

### 3.3. *Mishandling of public resources*

To complement the data on perceptions of corruption and proxy public spending efficiency, I use information from the quarterly bulletins containing the list of indi-

11. The specific questions asks whether the person is in her current job: a. A private-sector worker, b. A government worker, c. A domestic worker, d. Self-employed, e. Manager or employer, f. Unpaid family worker, g. Unpaid worker in enterprises or businesses of other households, h. Laborer, or i. Others.

viduals and organizations sanctioned for mishandling public funds between 1995 and 2019 in Colombia. These are published by the office of the Comptroller General of Colombia (*Contraloría General de la República, CGR*) and collected by Carreri and Martinez (2022). Notably, they include the affected public entity.

This data allows me to construct a yearly panel (from 1995 to 2019) of entities and a dummy indicating the agency’s appearance in a period’s bulletin. This variable can be interpreted as a proxy for public inefficiency at the agency level. I construct alternative variables, such as the number of appearances of persons (names) and the number of IDs (Colombian ID or tax ID numbers) per agency and year. Appendix A describes these variables in further detail.

Table 3 presents the mean and standard deviation of the outcomes for this panel of 39,550 observations. 81.5% of the agencies in the panel are treated, meaning that they are in the executive branch of the territorial order. Treated agencies usually have fewer sanctions at the extensive (the dummy outcome) and the intensive margins. Unfortunately, there is not a high availability of data on agency characteristics, especially for the pre-treatment period.

## 4 – Empirical Strategy

My identification strategy exploits the changes Law 617 generated in the hiring practices after 2000 and the different degrees of exposition of the public entities to this new regulation. Formally, I apply a *difference-in-differences* design for the three data sources. The first one is for the repeated cross-section on corruption perceptions of public employees (using the EDI & EDID surveys), where I estimate the following model:

$$Y_{iect} = \alpha_t + \beta_e + \gamma_c + \delta(\text{LocalEntity}_e \times \text{After2000}_c) + \varepsilon_{iect} \quad (2)$$

where  $i$  denotes the survey participant,  $t$  the year of the survey,  $e$  the entity and  $c$  the cohort of entry to the public sector.  $Y_{iect}$  is my measure of corruption perception

in the hiring processes – described in section 3 – ,  $LocalEntity_e$  is a dummy that takes the value of one if the public employee works for the executive sector in the territorial order, and  $After2000_c$  takes the value of one if she entered the public sector in 2001 or later, taking into account that the law was passed in October 2000. I include fixed effects at the year of the survey level ( $\alpha_t$ ), the cohort of entry-level ( $\gamma_c$ ), and the entity level ( $\beta_e$ ). Standard errors are clustered at the entity level, given that the order of the entity defines exposure to the new regulation. Clusterization also accounts for the surveys' sampling method, as they were stratified at the entity and hierarchy of the employee level.

As described in Section 3, one of the disadvantages of the surveys is that public employees are not explicitly asked for the year in which they began to work in the entity, but they instead answer their tenure in a set of bins is. Thus, some cohorts contain pre- and post-treatment periods (for example, the 1998-2002 cohort), with no possibility of identifying whether the respondent was hired before or after the introduction of Law 617. I exclude these contaminated cohorts in my main set of results.

I also apply a *diff-in-diffs* design for cohorts of entry on the job satisfaction data from the GEIH survey. I formally estimate the following equation in the sample of civil servants:

$$Y_{isdct} = \alpha_t + \beta_{sd} + \gamma_c + \delta(LocalEntity_{sd} \times After2000_c) + \varepsilon_{isdct} \quad (3)$$

where  $i$  represents the respondent,  $t$  the survey period (year and month),  $c$  the year of entry to the public sector, and  $Y_{isdct}$  are outcomes of job satisfaction defined in Section 3. Since it is not possible to directly identify the agency where the survey participant works, I use the economic sector and the employment location as a proxy for treatment definition. An employee is treated if she works under the ISIC 7512 code (designated to “Executive activities of the public administration”) and

outside of Bogotá, as a proxy of working in the executive sector in the local level <sup>12</sup>. The regression includes fixed effects at the sector (4-digit ISIC code) and department –subnational jurisdiction– level ( $\beta_{sd}$ ), the year of entry-level  $\gamma_c$ , and the survey period  $\alpha_t$ . I cluster the standard errors at the sector and department level, given that treatment is defined at this level.

The third and final application of the design is for the General Comptroller panel of agencies with data on the mishandling of public resources. Using the subscript  $t$  to denote the year of the bulletins and  $e$  to denote the entity, I estimate the following model:

$$Y_{et} = \alpha_t + \beta_e + \delta(LocalEntity_e \times After2000_t) + \varepsilon_{et} \quad (4)$$

$Y_{et}$  is the measure of public spending inefficiency – described in Appendix A –,  $LocalEntity_e$  is a dummy that takes the value of one if the agency belongs to the executive sector in the territorial government order, and  $After2000_t$  takes the value of one for the period 2001 and after. I include fixed effects at the entity level ( $\beta_e$ ) and the year level ( $\alpha_t$ ). Finally,  $\varepsilon_{et}$  is the error term, which I cluster at the entity level given the fact that exposure to the new regulation is defined by order of the entity <sup>13</sup>.

#### 4.1. *Validity of the empirical strategy*

My coefficients of interest,  $\delta$ , capture the change in corruption before and after the introduction of Law 617 in the entities of the territorial order, compared to those of

12. This assumption is possible due to the fact that most national-order agencies operate from Bogotá. For instance, 74% of the entities in the EDI survey have their headquarters in the national capital.

13. I also estimate another specification where instead of including year fixed effects, I control for different intercepts of year and organic class of the agency, which is a classification system that groups agencies into six main categories: administrative corporations (mainly, municipal councils and department assemblies), executive-branch agencies, autonomous entities, judiciary-branch agencies, legislative-branch agencies, and surveillance institutions. This specification allows for comparing the evolution of the outcomes among agencies of the same nature.

the national order, taking into account that the former were more exposed to the change in the legislation than the latter, which allowed them to use hiring through direct and temporary contracts with a higher frequency. For the survey data, they represent the differential change in corruption perceptions and job satisfaction among public employees that entered the civil service before and after 2000 in the most affected group of agencies. The main assumption behind this *difference-in-differences* design is that in the absence of the law, the outcomes would have evolved independently of the entities' order. I partially assess the validity of this parallel-trends assumption by estimating event study specifications, which look like Equation 5 for the Comptroller data:

$$Y_{et} = \alpha_t + \beta_e + \sum_{\tau \in T \neq 2000} \theta_{\tau} (LocalEntity_e \times \mathbb{1}[t = \tau]) + \epsilon_{et} \quad (5)$$

where  $\mathbb{1}[t = \tau]$  are year dummies and  $T$  includes all periods in the sample except 2000, which is the last one before the change in the legislation. The parameters  $\theta_{\tau}$  can be interpreted as the differential change in the outcome of public inefficiency  $Y_{et}$  at all points in time, relative to 2000. For the corruption perceptions outcomes, I estimate the following non-parametric version of equation 2:

$$Y_{iect} = \alpha_t + \beta_e + \gamma_c + \sum_{j \in C} \theta_j (LocalEntity_e \times \mathbb{1}[c = j]) + \epsilon_{iect} \quad (6)$$

where  $\mathbb{1}[c = j]$  are cohort of entry dummies and  $C$  includes all cohorts in the sample except 1996-2000, which is the last cohort before the change in the legislation in 2000. The parameters  $\theta_j$  can be interpreted as the difference in average corruption perceptions between territorial and national entities for employees of cohort  $j$  relative to the last cohort that was not exposed to law 617. Finally, I run a similar model for the repeated cross-section data on job satisfaction:

$$Y_{isdct} = \alpha_s + \beta_{sd} + \gamma_c + \sum_{j \in C} \theta_j (LocalEntity_{sd} \times \mathbb{1}[c = j]) + \epsilon_{isdct} \quad (7)$$

here,  $C$  includes all cohorts in the sample except 2000, and the parameters  $\theta_j$  can be interpreted as the difference in average job satisfaction between those that work in the executive public sector out of Bogotá and those in other sectors or in the capital, for employees of cohort  $j$  relative to the excluded cohort. I formally test the hypotheses that  $\theta_\tau = 0$  for years 1995-1999 in equation 5,  $\theta_j = 0$ , for the cohorts before 2000 (“1994-1997” and “1995-1999”) in equation 6, and  $\theta_j = 0$  for the cohorts 1994-1999 in the job satisfaction data (equation 7).

Since both national and territorial entities were exposed to discretionary contracts before and after Law 617, but the latter have been more affected by the change in legislation, it would be ideal to estimate the impact using the *fuzzy difference-in-differences* design suggested by de Chaisemartin and D’Haultfœuille (2017). However, I lack data on the number of these contracts and their importance in public-sector employment before 2011, impeding me from computing the Wald-DID or any other estimator.

## 5 – Results

This section provides evidence of the effect of the change in legislation on perceptions of corruption, job satisfaction, and the mishandling of public resources. I start by presenting the difference-in-differences estimates (those of equations 2, 3, and 4). I then assess the validity of the main identifying assumption (equations 5, 6, and 7) and conclude by discussing the robustness of the main results.

### 5.1. Main findings

In table 5, I report the estimated coefficients of equation 2 for the three outcomes of perceived corruption. In the first column, the dependent variable refers to the dummy variable taking the value of one if the public employee strongly agrees or agrees that the hiring process was based on personal, familial, or friendship links. In contrast, the statements in the second and third columns indicate the influence

of politicians, or money and the exchange of favors. I include agency, survey year, and category of entry fixed effects in all specifications and show standard errors clustered at the entity level in parentheses.

Relative to the public employees in entities of national order, those who work at the local level had higher corruption perceptions of the personnel selection processes after law 617, as measured by the three outcomes. Accounting for entity, cohort of entry and year of survey heterogeneity, territorial corporations experienced a 7.8 percentage-points (pp) relative increase in the average probability of their employees believing that hiring practices were based on personal, familial, or friendship networks. Similarly, the relative escalation on perceptions of political influence or the use of irregular payments and exchange of favors are respectively computed at 9.7pp and 3.56pp. These magnitudes are equivalent to 25.2%, 24.8%, and 4.5% of the outcomes' means before the law was implemented, and to 16%, 19.9%, and 8.9% of their standard deviations.

Table 6 presents the estimates of equation 3 (job satisfaction data). Civil servants working in treated entities that entered after the change in legislation increased their desire to switch jobs by 5.2 percentage points on average, relative to public employees in other government sectors or those in the capital (as a proxy of working in the national order). The desire to switch jobs to increase the use of their skills or training also increased by 3.3pp. At the same time, there are reductions in the satisfaction index (by around -0.18 units), and job satisfaction (by 0.6pp), although the latter is not statistically significant. The rise in the first dependent variable represents 114% of the variable's mean for pre-treatment cohorts, whereas the other coefficients account for 100%, 6.43%, and 0.94% of their respective outcomes' mean before the change in legislation. The magnitudes are equivalent to 25%, 18.53%, 12.69%, and 1.25% of the variables' standard deviation for employees that entered before 2000.

Finally, Table 7 contains the estimated coefficients for the Comptroller's data on the mishandling of public resources (equation 4). Odd columns present the basic

specifications with agency and year fixed effects, and even columns add organic class and year fixed effects, which allow for better comparisons between agencies of the same nature<sup>14</sup>. Relative to national-order entities or those out of the executive branch, territorial-order agencies experienced an increase in the probability of appearing in the Comptroller's bulletins in a given year by 6 to 10 percentage points after the change in legislation, depending on whether the model includes year or class and year fixed effects. The effect represents 23.6% of the variable's standard deviation in the pre-treatment period for the year fixed-effects specification and 40.6% for the class and year fixed-effects model. There are also positive estimates for the transformations of the number of appearances in the bulletins, number of persons, and number of IDs. However, the coefficients are statistically significant (at  $\alpha = 0.05$ ) in the last two outcomes only when including class and year fixed effects.

## 5.2. Identifying assumption

In this subsection, I assess the validity of the *difference-in-differences* design by presenting the estimation results of equations 6, 7 and 5. Figure 2 reports the coefficients for the corruption perceptions outcomes, with those cohorts contaminated with pre and post-2000 colored in gray. I find that before 2000, all the estimated  $\theta_j$  are close to zero and not statistically significant under the 95% interval of confidence. The overall F-test p-values are 0.135, 0.144, and 0.161 for the personal networks, political networks, and money and favors influence, respectively, providing evidence of parallel pre-trends for all outcomes using  $\alpha = 0.05$  for this joint test.

Figure 3 presents the estimates of the non-parametric specification of dynamic effects for the job satisfaction outcomes (Equation 7). Panels a and b, which refer to the switching jobs outcomes, show the most substantial evidence against pre-

14. There are six main organic classes for Colombian public entities: administrative corporations (mainly, municipal councils and department assemblies), executive-branch agencies, autonomous entities, judiciary-branch agencies, legislative-branch agencies, and surveillance institutions)

trends before the change in legislation (with p-values of 0.708 and 0.745 for the joint hypothesis F test of all pre-treatment coefficients equaling 0). Although the satisfaction index and the job satisfaction dependent variables have one coefficient –  $\theta_{1995}$  for the former and  $\theta_{1999}$  for the latter – rejecting the null hypothesis of being non-significant under 95% confidence interval, the results for the joint hypothesis F tests suggest there is not enough evidence suggesting that these satisfaction measures evolved differently for treatment and control agencies before the reform (the p-values are 0.48 and 0.161).

For the Comptroller data on the mishandling of public resources, Figure 4 reports the estimates for Equation 5. In addition to the variables in Table 7, Figure B1 shows the dynamic specification estimates for the measures without the inverse hyperbolic sine transformation. All outcomes present evidence for the joint hypothesis test for the absence of pre-trends before 2000, as the p-values are higher than 0.1. However, it is worth denoting that Figure B1 allows seeing that the treatment effects are not robust to the functional form for the number of persons and the number of IDs, since there are more positive coefficients when transforming these measures.

This set of results predominantly points out the absence of differential pre-trends before the change in legislation in 2000 for my three outcomes of interest. This partially validates the use of the *difference-in-difference* design and supports my main result, namely that the promotion of discretionary contracts through law 617 caused a differential increase in the perceptions of corruption in the hiring process of public employees.

### 5.3. Robustness checks

#### 5.3.1 Model specification

Despite accounting for differences among agencies, survey periods, and cohorts, equations 2 & 3 lack the inclusion of other factors that might have an impact on the perceptions of corruptions and on satisfaction with job characteristics. This is

partly caused by the fact that the EDI and EDID surveys contain few questions on individual and entity characteristics. The GEIH corrects this issue for the satisfaction outcomes, given that it is a labor market instrument with a section dedicated to personal features, allowing me to add individual-level controls to the difference-in-difference design. Formally, I estimate the following models:

$$Y_{iect} = \alpha_t + \beta_e + \gamma_c + \delta(LocalEntity_e \times After2000_c) + \zeta X_{iect} + \varepsilon_{iect} \quad (8)$$

for the outcomes of corruption perceptions, and:

$$Y_{isdct} = \alpha_t + \beta_{sd} + \gamma_c + \delta(LocalEntity_{sd} \times After2000_c) + \zeta X_{isdct} + \varepsilon_{isdct} \quad (9)$$

for the job satisfaction data.  $X_{iect}$  is a matrix of dummies indicating whether the survey participant is a woman and her position in the agency (with three possible options: directive, professional, and auxiliary). The EDI and EDID survey started asking for the public employee's education level in 2016. However, there are no individuals in the pre-treatment cohorts for this or the subsequent waves (2017 and 2018).  $X_{isdct}$  also contains the sex dummies but adds the respondent's age and the number of years in education.

Table B1 reports the estimates for the coefficient of interest ( $\delta$ ) under equation 8 in columns 2, 4, and 6. Columns 1, 3, and 5 show the main specification's coefficients that do not include controls (those reported in Table 5). The magnitudes of the effects are reduced by around 2 percentage points. However, they remain statistically significant in the outcomes on the influence of personal and political networks. The outcome on the influence of irregular payments and favors, which is reduced by 1.7 percentage points, loses statistical significance (its p-value in the new specification is 0.136).

Table B2 presents the estimates for the outcomes on job satisfaction, with a similar structure to the results for perceptions of corruption. All coefficients remain

unchanged when including controls, except for the satisfaction index, whose magnitude increases by 1pp. There are also no changes in the statistical significance, suggesting that the impacts are robust for this aspect.

### 5.3.2 Influential observations

I test that the results are not driven by one particular observation. Figure B2 in the appendix plots the *difference-in-differences* estimates for corruption perceptions outcomes (i.e., those in Table 5), when excluding the departments where the entities are located one-by-one. The magnitude of the effects and their significance are similar across subsamples. The only exception is the exclusion of Bogotá when the dependent variable is the perceptions of hiring based on money or exchange of favors. The coefficient is reduced by about 1.3 percentage points, and the new p-value is 0.139. However, the increase in the confidence intervals is partly caused by the importance of the capital in the national-order government (74% of the agencies of this level have their headquarters in Bogotá) and in the overall sample (60% of the respondents work there).

Figure B3 repeats the analysis in the outcomes of job satisfaction. The estimated impacts remain significant at the 5%, and with similar magnitudes for both dependent variables on the desire to switch jobs and the satisfaction index. Finally, Figure B4 shows the application of the one-department-at-a-time exclusion for the data on the mishandling of public resources (without including class and year fixed effects). The results are similar to those in columns (1), (3), (5), and (7) of Table 7.

## 6 – Mechanisms

In this section, I explore two potential mechanisms behind the positive effect of being more exposed to the change in legislation on the perceptions of corruption and the reduction in job satisfaction outcomes in treated entities: the actual increase

in nepotism and the deterioration of the image of fairness and competitiveness in the public sector. I will refer to the latter as the “fairness” channel.

I first test that the reform actually promoted non-competitive practices in the hiring processes of treated agencies. The GEIH asks occupied respondents the primary way in which they got their jobs, with seven possible options, two of which are of interest: (1) by asking for help from family, friends, or colleagues, and (2) via calls or job announcements. I respectively interpret these as proxies of nepotistic and competitive approaches to select personnel. I apply the *difference-in-difference* design used in equation 3 to explore how the probability of being hired in one way or another changed after the change in legislation. I formally estimate the following model in the sample of public employees:

$$Y_{isdct} = \alpha_t + \beta_{sd} + \gamma_c + \delta(LocalEntity_{sd} \times After2000_c) + \varepsilon_{isdct} \quad (10)$$

where  $Y_{isdct}$  are two dummies. The first one indicates whether the respondent entered the agency mainly by a call or job announcement, and the second one whether she entered mainly by asking for help from family, friends, or colleagues. The definition of the treatment variables remains unchanged.

Table 8 reports the estimates. Columns 2 and 4 expand the specification with individual-level controls: a sex dummy, the number of years of education, and the respondent’s age. The coefficients indicate that relative to public employees in untreated entities, those that entered territorial order in the executive branch after the change in legislation experienced a decrease in the likelihood of entering via calls or job announcements by 20 percentage points. Meanwhile, there was a ten percentage points increase in the probability of the help of family, friends, and colleagues being the primary means to getting their jobs. These magnitudes are economically significant, equivalent to 39% of the pre-period mean (41% of the standard deviation) for the calls dummy and around 41% (23% of the S.D) for the family and friends variable. This evidence and the results on corruption perceptions suggest that nepo-

tistic practices indeed increase after the change in legislation, and public employees notice it.

For the second mechanism, I hypothesize that higher exposure to discretionary and temporary contracts after the change in legislation caused a relative increase in the image that the selection processes in the public sector are unfair, which might have an impact on corruption perceptions and job satisfaction. Civil servants from older cohorts (those that entered before 2000) and from the national order might have developed a stronger sense of competitiveness in the hiring practices, whereas those that entered territorial-order agencies after law 617 might have a stronger belief of unfairness. This hypothesis becomes more reasonable given that they might have entered into one of the few competitive processes for their agencies and are still more exposed to discretion.

Consistent with this hypothesis, the relative increase in corruption perceptions and the decrease in job satisfaction should be higher for more educated and experienced public employees. Not only did they face higher financial and time costs, but they were also more likely to obtain stable, well-paid public sector jobs through competitive recruitment processes. They are, therefore, more likely to be disappointed by the conditions of employment associated with discretionary, non-competitive recruitment processes. I formally test this hypothesis using a triple-difference design. I estimate the following model using the corruption perceptions data, which only contains the education level of the survey respondent:

$$\begin{aligned}
 Y_{iect} = & \alpha_t + \beta_e + \gamma_c + \theta(LocalEntity_e \times After2000_c \times Graduate_{iect}) \\
 & + \delta_1(LocalEntity_e \times After2000_c) + \delta_2(Graduate_{iect} \times After2000_c) \quad (11) \\
 & + \delta_3(Graduate_{iect} \times LocalEntity_e) + \varepsilon_{iect}
 \end{aligned}$$

where  $Graduate_{iect}$  is a dummy taking the value of 1 if the public employee has graduate education (which in Colombia also includes the *especialización*, a postgrad-

uate diploma that usually does not involve research). Unfortunately, the education question was introduced in the 2016 wave of the EDI and EDID, which does not contain data on clean pre-treatment cohorts. I take advantage of the fact that the positive and significant effects on corruption perceptions start in the “2001-2005” cohort (and more stable in the “2004-2009” cohort, as Figure 2 shows) and redefine  $After2000_c$ , so it takes the value of 0 for those that entered in the “2000-2004” window – the first cohort available in the 2016 surveys –, and 1 if they entered in “2001-2005” or after.

The data on job satisfaction does not contain this problem. The GEIH survey includes questions about the respondent’s education level and experience. I estimate the following triple-difference design models, the first one to explore heterogeneous treatment effects by education, and the second by experience:

$$\begin{aligned}
Y_{isdct} = & \alpha_t + \beta_{sd} + \gamma_c + \theta(LocalEntity_{sd} \times After2000_c \times TertiaryEduc_{isdct}) \\
& + \delta_1(LocalEntity_{sd} \times After2000_c) + \delta_2(TertiaryEduc_{isdct} \times After2000_c) \\
& + \delta_3(TertiaryEduc_{isdct} \times LocalEntity_{sd}) + \zeta X_{isdct} + \varepsilon_{isdct}
\end{aligned} \tag{12}$$

$$\begin{aligned}
Y_{isdct} = & \alpha_t + \beta_{sd} + \gamma_c + \theta(LocalEntity_{sd} \times After2000_c \times Experienced_{isdct}) \\
& + \delta_1(LocalEntity_{sd} \times After2000_c) + \delta_2(Experienced_{isdct} \times After2000_c) \\
& + \delta_3(Experienced_{isdct} \times LocalEntity_{sd}) + \zeta X_{isdct} + \varepsilon_{isdct}
\end{aligned} \tag{13}$$

where  $TertiaryEduc_{isdct}$  is a dummy that equals one if the survey participant has tertiary education (a bachelor’s or technical degree) and zero if she does not. The GEIH does not directly ask about the employee’s experience, so I construct a proxy with the number of years contributing to a retirement pension. The variable  $Experienced_{isdct}$

takes the value of 1 if the respondent has contributed to pension for seven or more years, which is the sample median. All regressions control for the sex, age, and years of education of the individual.

Table 9 reports the estimates of equation 11. The triple-difference coefficient is positive in the three outcomes, meaning that the perceptions of corruption grew more after the legislation change for the most educated public employees. Although this evidence partly supports the fairness hypothesis, the  $\theta$  is only statistically different from 0 in the outcome of the political network (with a p-value of 0.056). The effect is not significant in the other two dependent variables ( $p=0.144$  for the personal networks and 0.484 for the irregular payment and favors).

For the job satisfaction outcomes, Tables 10 and 11 show the estimated heterogeneous treatment effects respectively by education (Equation 12) and experience (Equation 13). In line with the fairness hypothesis, the triple difference interaction with education is positive for the desire to switch jobs because their skills and training are being underused, and negative for the satisfaction index. However, they are not statistically significant. More robust evidence for the hypothesis comes from experience. The first two columns indicate that the “switching jobs” measures differentially increased in more experienced individuals. At the same time, they also had a relative reduction in satisfaction (although the triple interaction is only significant at the 5% for the satisfaction index). Considering that the *difference-in-difference* estimator is negative for the first two outcomes and positive for the last two, it means that the satisfaction outcomes decreased after the legislation change only for those with an experience above the median, and the rise in the desire to switch jobs is only present in this group as well.

These results suggest that the increase in corruption perceptions found in Section 5 was based on the deterioration of the hiring practices in the treated entities, given that after the reform, more employees entered using the help of family, friends, and colleagues. This has negatively impacted the fairness image of the public sector,

as the effects of the change in legislation (both in perceptions of corruption and job satisfaction) are concentrated in the more competent individuals (those with higher levels of education and experience).

## 7 – Conclusion

This article explores the impact of being exposed to more discretionary hiring practices on three aspects of the public sector: corruption perceptions, job satisfaction, and the mishandling of public resources, being the first two relatively unexplored in the literature. I exploit a change in the Colombian legislation that prohibited territorial-level executive government agencies from having real growth in their official (less discretionary) personnel expenditure after 2000, incentivizing them to hire civil servants using highly discretionary and temporary contracts. Previous studies suggest that such an intervention could deteriorate meritocracy in the affected entities, which has the potential to undermine trust between their public employees. Following research on the relationship between trust, mission motivation, and performance, I hypothesize that reform negatively affected job satisfaction and public efficiency.

Using a *difference-in-differences* in cohorts design, I find a relative increase in the awareness of nepotism, the influence of political networks, and the role of unofficial payments and favors in the selection processes of territorial-order agencies after the reform. Depending on the corruption perceptions measure, the effects' magnitudes represent between 4.5% and 25.2% of the outcomes' averages before the law. Civil servants in affected agencies also experienced a differential increase in the desire to switch jobs, and the perceptions that their skills and training are being underused. The reform also negatively impacted job satisfaction. Finally, *diff-in-diff* estimates applied to panel data on sanctions for mishandling public resources suggest that this phenomenon augmented in affected entities after 2000, although the evidence

is weak.

I then explore if a deterioration of meritocracy indeed drives the increase in corruption perceptions. I find that civil servants that entered the affected agencies experienced an increase in the probability of enrolling thanks to help from family, friends, and colleagues, while the likelihood of being hired via calls or job announcements decreased. Finally, I hypothesize that this policy worsened the sense of fairness in selection processes in public employment. I document a differential increase in corruption perceptions in highly educated public employees and a relative decrease in job satisfaction outcomes for those with high experience. The law's impact should be the largest on this group of workers since they have faced higher financial and time costs and probably have higher expectations of the need for a competitive civil service.

These results show that policies aiming at reducing expenditure in public employment can have negative consequences if agencies continue to face the need for civil servants to provide public goods and services. Despite having a reasonable objective (improving the finances of local governments), Colombian legislation opened the opportunity for the discretionary selection of public employees. Furthermore, aspects such as job satisfaction and corruption perceptions should be taken into account by policymakers, given their potential to affect overall performance in the public sector.

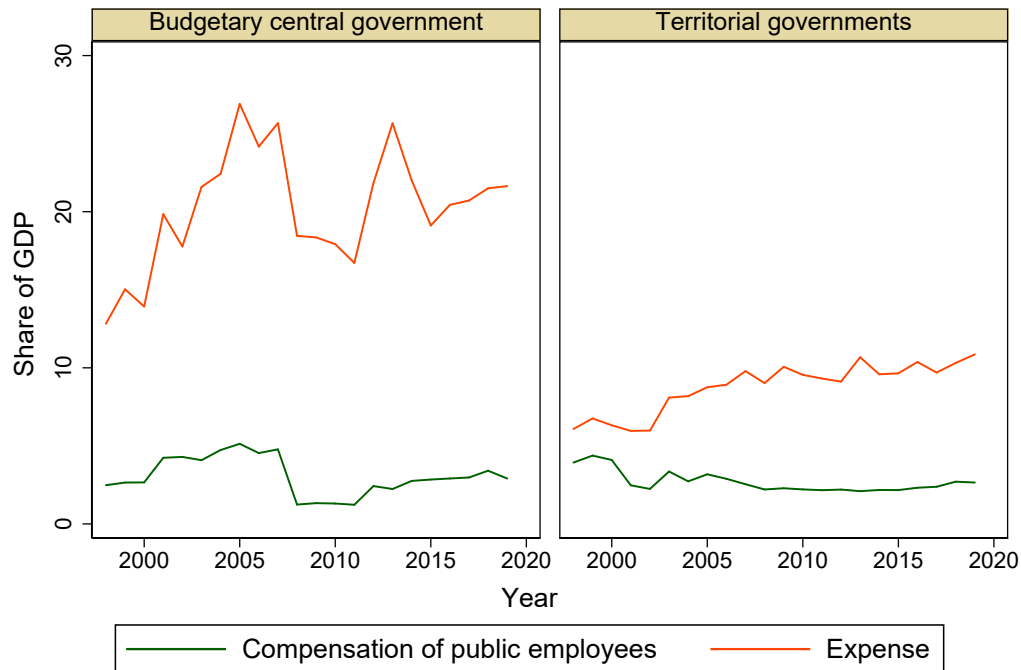
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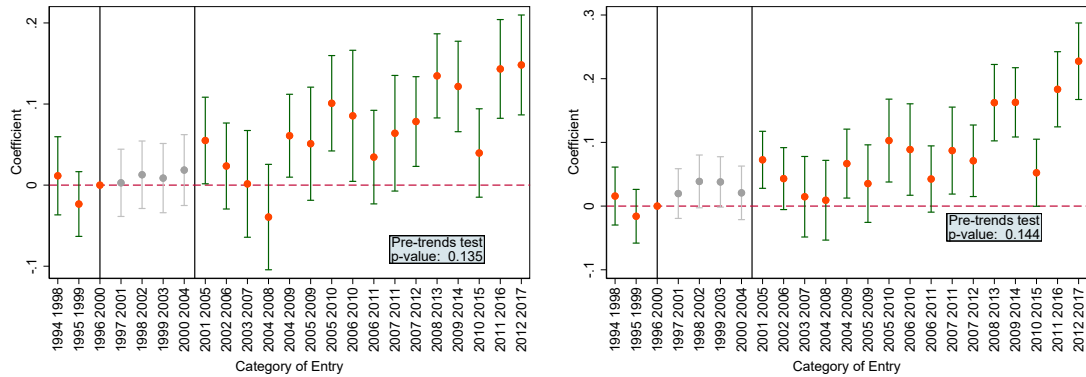
**Figure 1 – Evolution of total expense and payroll in the Colombian public sector, 1998–2019**



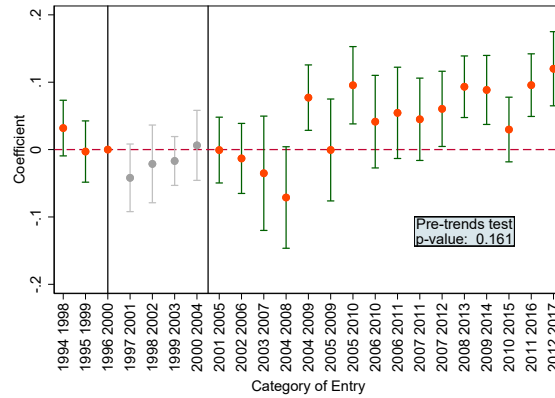
Graphs by Government sector

**Notes:** This figure presents the evolution of total expense and the compensation of employees in the Colombian public sector as a share of GDP. Public employee compensation includes wages, salaries, and the employer’s social contributions. Budgetary central government encompasses the fundamental activities of the national executive, legislative, and judiciary powers and is usually covered by the main (or general) budget. The category of “Territorial governments” (for the local level) includes department and municipality governments (*gobernaciones* and *alcaldías* in the Colombian case). Source: IMF (2021). Government Finance Statistics.

**Figure 2 – Dynamic effects of the change in legislation on corruption perceptions**



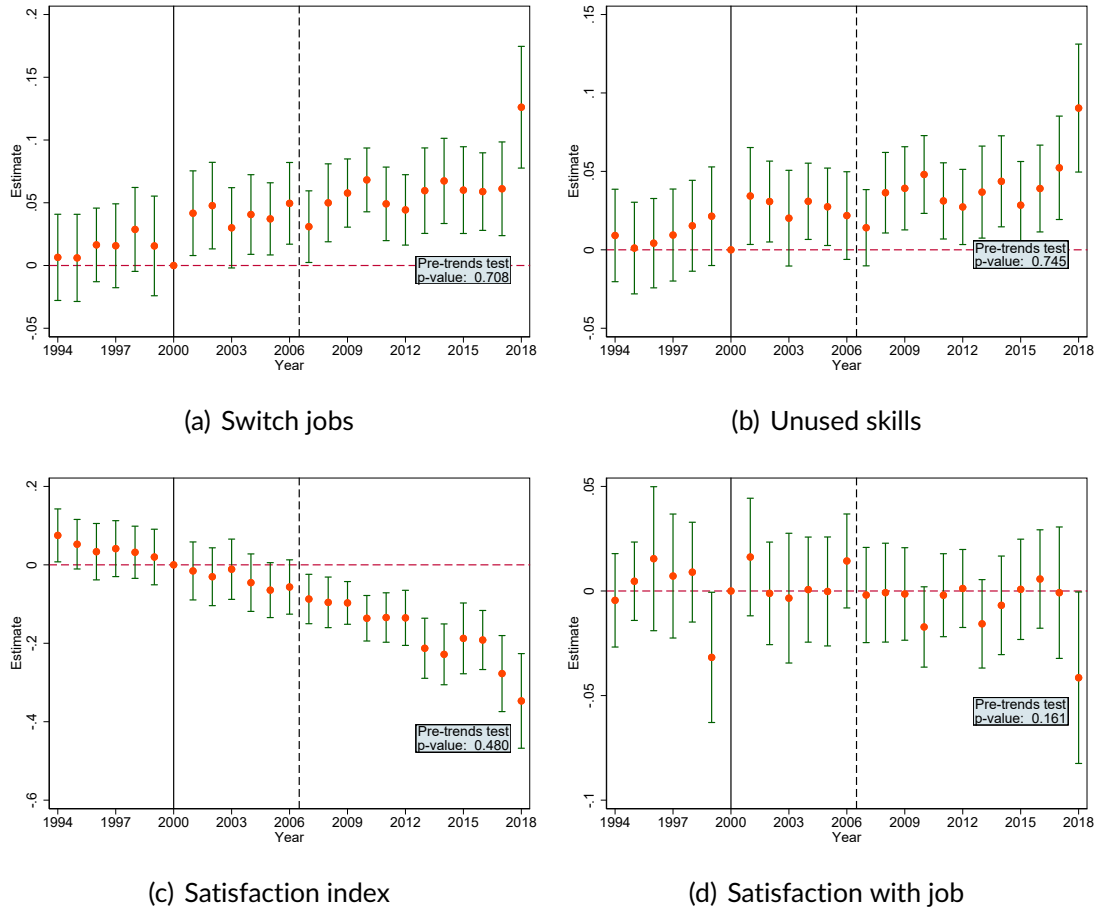
(a) Hiring based on personal, familial, or friend-ship links (personal networks) (b) Hiring based on political influences (political networks)



(c) Hiring based on money or exchange of favors (money)

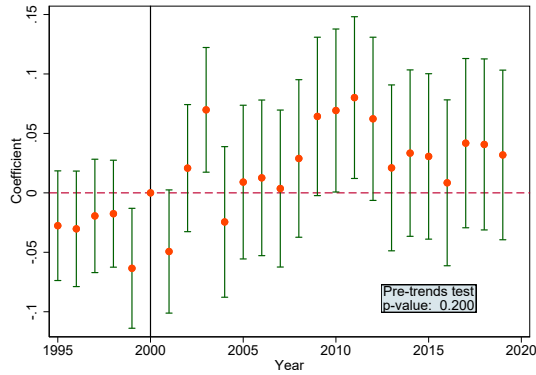
**Notes:** This figure presents the estimated coefficients from the dynamic specification established in equation 6 on each of the three outcome variables. Hiring based on personal, familial, or friendship links is a dummy equal to one if the public employee strongly agrees or agrees with the statement that those factors corrupted personnel selection. The creation of the rest of the variables followed the same process. I present the point estimates and their intervals of confidence at 95%. Standard errors are clustered at the agency level. The gray estimates belong to cohorts contaminated with pre and post-treatment periods.

**Figure 3 – Dynamic effects of the change in legislation on job satisfaction outcomes**

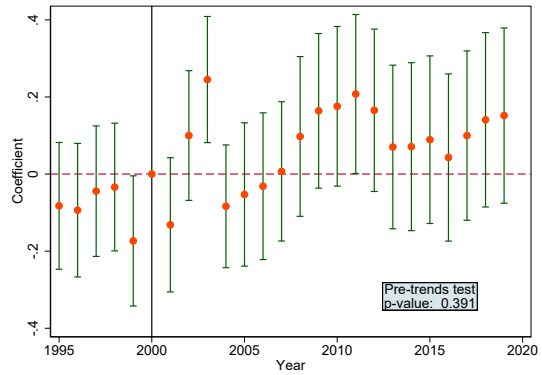


**Notes:** This figure reports the estimated coefficients from the dynamic specification established in equation 7 for different measures of job satisfaction. “Switch jobs” is a dummy equal to 1 if the individual reports desiring to change her current job, and 0 if she does not. “Unused skills” is a dichotomous variable that takes the value of 1 if the respondent wants to switch jobs “to increase the use of her skills or training”, and 0 if this is not a reason or if she does not want to change jobs. The satisfaction index is a discrete variable ranging from 0 to 4, which indicates the number of job characteristics the survey participant is satisfied with (from the following options: (i) the type of contract she has, (ii) her current job, (iii) the wage and benefits she receives, (iv) and the job's workday). “Satisfied with job” equals 1 if the civil servant indicates being satisfied with her job, and 0 otherwise. I present point estimates and their intervals of confidence at 95%. Standard errors are clustered at the sector (4-digit ISIC code) and department level.

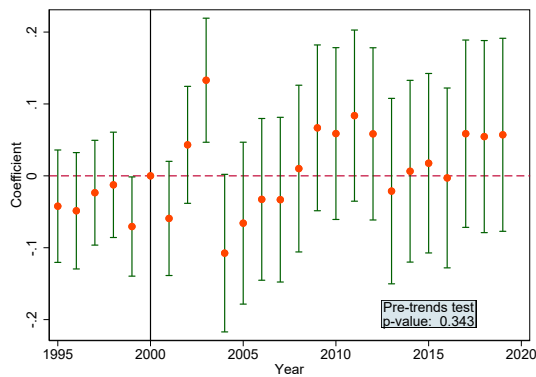
**Figure 4 – Dynamic effect of the change in legislation on the mishandling of public resources**



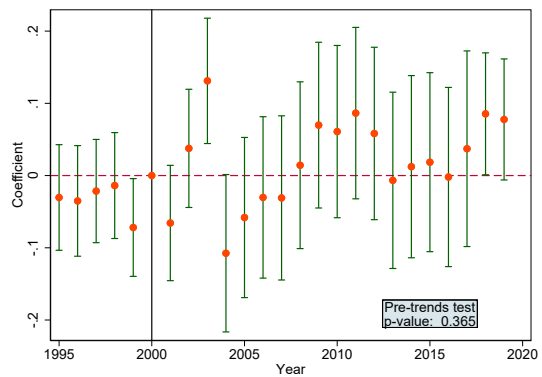
(a) D(Any sanction)



(b) Arcsinh(Appearances)



(c) Arcsinh(People)



(d) Arcsinh(No. IDs)

**Notes:** This figure reports the estimated coefficients from the dynamic specification established in equation 5 for different measures the mishandling of public resources. “D(Any Sanction)” is a dummy taking the value of 1 if the agency appears in the Comptroller reports at any given year. The number of persons is defined by the number of raw names that appear for an entity quarterly during any given year. The number of IDs is the count of Colombian or tax ID numbers in the entity-year cell. All variables are winsorized at the percentiles 1 and 99. I present point estimates and their intervals of confidence at 95%. Standard errors are clustered at the agency level.

**Table 1 – Summary statistics - Corruptions perceptions survey**

Variable	Overall	Entity order		Difference p-value
		National	Territorial	
<i>Outcomes</i>				
Contracts based on personal network	0.469 (0.001)	0.488 (0.001)	0.402 (0.003)	0.000
Contracts based on political influences	0.479 (0.001)	0.518 (0.001)	0.347 (0.002)	0.000
Contracts based on money and favors	0.758 (0.001)	0.780 (0.001)	0.680 (0.003)	0.000
<i>Covariates</i>				
Woman	0.549 (0.001)	0.543 (0.001)	0.571 (0.002)	0.000
Directive position	0.138 (0.001)	0.144 (0.001)	0.116 (0.001)	0.000
Professional position	0.619 (0.001)	0.623 (0.001)	0.607 (0.002)	0.000
D(Tertiary Education)	0.979 (0.000)	0.980 (0.000)	0.974 (0.001)	0.000
D(Graduate education)	0.205 (0.001)	0.212 (0.001)	0.179 (0.002)	0.000
<i>Treatment variables</i>				
After 2000	0.797 (0.001)	0.805 (0.001)	0.766 (0.002)	0.000
Local Entity	0.218 (0.001)			
Observations	247,505	193,612	53,893	
No. of Entities	285	240	45	

**Notes:** This table reports summary statistics at the individual (survey participant) level for the survey's periods of study (from 2010 to 2018). *Contracts based on personal networks* is a dummy variable equal to one if the civic employee strongly agrees or agrees with the statement that the overall hiring practices in her entity were based on personal, familial, or friendship links. *Contracts based on political networks* equals 1 if she strongly agrees or agrees that the hiring practices were based on political influences, and *Contracts based on money and favors*=1 in the case she strongly agrees or agrees that the selection process was based on irregular payments and exchange of favors. *Treated Entity* is also a dummy that equals 1 if the public employee works for an entity of the territorial order, and *Entry After 2007* takes the value of 1 if she entered in 2007 or later. Standard errors are presented in parentheses.

**Table 2 – Summary statistics - sample of public employees from the *Gran Encuesta Integrada de Hogares (GEIH)***

Variable	Entity treatment status			Difference p-value
	Overall	Control	Treated	
<i>Outcomes</i>				
Switch jobs	0.112 (0.001)	0.092 (0.001)	0.189 (0.002)	0.000
Unused skills	0.082 (0.001)	0.068 (0.001)	0.137 (0.002)	0.000
Satisfaction index	2.798 (0.003)	2.864 (0.004)	2.545 (0.008)	0.000
D(Satisfied with the job)	0.677 (0.001)	0.688 (0.001)	0.635 (0.003)	0.000
D(Self-Employed)	0.153 (0.001)	0.078 (0.001)	0.438 (0.003)	0.000
D(Full-Retirement Contributions)	0.143 (0.001)	0.076 (0.001)	0.406 (0.003)	0.000
D(Fixed-term contract)	0.280 (0.001)	0.207 (0.001)	0.562 (0.003)	0.000
D(Got job via calls)	0.476 (0.001)	0.511 (0.001)	0.259 (0.003)	0.000
D(Got job via family & friends)	0.265 (0.001)	0.239 (0.001)	0.426 (0.004)	0.000
<i>Covariates</i>				
Female	0.490 (0.001)	0.487 (0.001)	0.501 (0.003)	0.000
Age	38.680 (0.026)	38.597 (0.029)	38.997 (0.059)	0.000
Years of education	14.737 (0.008)	14.791 (0.009)	14.529 (0.019)	0.000
Years contributing to pension	11.873 (0.029)	12.172 (0.031)	10.701 (0.068)	0.000
D(Tertiary education)	0.775 (0.001)	0.774 (0.001)	0.779 (0.002)	0.088
<i>Treatment variables</i>				
D(Entered after 2000)	0.792 (0.001)	0.766 (0.001)	0.890 (0.002)	0.000
Treated individuals	0.207 (0.001)			
Maximum Observations	163,941	129,940	34,001	

**Notes:** This table reports summary statistics at the individual (survey participant) level for the sample of public employees in all *Gran Encuesta Integrada de Hogares* periods of study (from January 2010 to 2018). "Full-Retirement Contr." is a dummy that equals 1 if the individual answers that she fully pays for her retirement contributions and 0 if her employer totally or partially pays these contributions or if she does not contribute. Treated individuals are those working in the ISIC Revision 3 "7514" code and out of Bogotá, as a proxy of working in the executive sector in the territorial-order agencies. Standard errors are presented in parentheses.

**Table 3 – Summary statistics - Mishandling of public resources data**

Variable	Overall	Entity treatment status		Difference p-value
		Control	Treated	
<i>Outcomes</i>				
Had at least one sanction	0.367 (0.002)	0.384 (0.006)	0.363 (0.003)	0.001
Appearances (Windsorized)	4.855 (0.055)	6.030 (0.155)	4.588 (0.058)	0.000
Number of sanctioned people (Windsorized)	1.154 (0.012)	1.420 (0.034)	1.093 (0.012)	0.000
Number of sanctioned IDs (Windsorized)	1.009 (0.010)	1.229 (0.029)	0.960 (0.011)	0.000
<i>Treatment variables</i>				
After 2000	0.800 (0.002)	0.800 (0.005)	0.800 (0.002)	1.000
Local Entity	0.815 (0.002)			
Maximum Observations	39,550	7,325	32,225	
Number of Entities	1,582	293	1,289	

**Notes:** This table reports summary statistics at the agency-year level for the Comptroller General Panel on the mishandling of public resources. I(Territorial order agency) is a dummy variable that takes the value of 1 if an agency belongs to the executive branch in the territorial order and 0 if it is in the national order or the legislative or judiciary branch. The outcomes are described in Appendix A. Standard errors are presented in parentheses.

**Table 4 – Estimates from a difference-in-difference design of the effect of the change in legislation on proxies for discretionary and temporary contracts (only public employees)**

	(1) Self-Employed	(2) Full-Retirement Contr.	(3) Fixed-term Contract
Local Entity × After 2000	0.339*** (0.016)	0.312*** (0.016)	0.295*** (0.016)
Observations	158,689	152,495	156,508
No. of Clusters	1,037	1,027	1,033
SectorXdep F.E	Yes	Yes	Yes
Year of entry F.E	Yes	Yes	Yes
Period of Survey F.E	Yes	Yes	Yes
Mean DV	.0112	.0131	.0356
S.D DV	.105	.114	.185

**Notes:** This table presents the estimation of  $\delta$  from Equation 1. Treated individuals are those working in the ISIC Revision 3 “7514” code and out of Bogotá, as a proxy of working in the executive sector in the territorial-order agencies. All dependent variables are dummies. “Full-Retirement Contr.” is a dummy that equals 1 if the individual answers that she fully pays for her retirement contributions and 0 if her employer totally or partially pays these contributions or if she does not contribute. Standard errors clustered at the sector (ISIC 4-digit ISIC code) and department level are presented in parentheses. \* is significant at the 10% level, \*\* is significant at the 5% level, \*\*\* is significant at the 1% level.

**Table 5 – Estimates from the diff-in-diff design of the effect of the change in legislation on corruptions perceptions using category of year of entry fixed effects**

	(1)	(2)	(3)
	Personal networks	Political networks	Money
Local Entity × After 2000	0.078*** (0.016)	0.097*** (0.016)	0.036** (0.014)
Observations	128,961	127,067	107,269
No. of Entities	281	281	281
Entity F.E	Yes	Yes	Yes
Category of Entry F.E	Yes	Yes	Yes
Survey F.E	Yes	Yes	Yes
Mean DV	.381	.391	.786
S.D DV	.486	.488	.41

**Notes:** This table presents the estimation results for the main specification (equation 2). “Personal networks” is a dummy variable equal to one if the civic employee strongly agrees or agrees with the statement that the overall hiring practices in her entity were based on personal, familiar, or friendship links. Political networks equals 1 if she strongly agrees or agrees that the hiring practices were based on political influences, and Money=1 in the case she strongly agrees or agrees that the selection process was based on irregular payments and exchange of favors. Treated is also a dummy that equals 1 if the public employee works for an entity of the territorial order, and After 2000 takes the value of 1 if she entered in 2000 or later. Standard errors clustered at the entity level are presented in parentheses. \* is significant at the 10% level, \*\* is significant at the 5% level, \*\*\* is significant at the 1% level.

**Table 6 – Estimates from the difference-in-difference design of the effect of the change in legislation on job satisfaction outcomes**

	(1)	(2)	(3)	(4)
	Switch jobs	Unused skills	Satisfaction index	Satisfied with Job
Local Entity × After 2000	0.052*** (0.008)	0.033*** (0.006)	-0.179*** (0.022)	-0.006 (0.005)
Observations	158,689	158,689	157,788	158,689
No. of Clusters	1,037	1,037	1,036	1,037
SectorXdep F.E	Yes	Yes	Yes	Yes
Year of entry F.E	Yes	Yes	Yes	Yes
Period of Survey F.E	Yes	Yes	Yes	Yes
Mean DV	.0455	.0329	2.78	.636
S.D DV	.208	.178	1.41	.481

**Notes:** This table presents the estimation of  $\delta$  from Equation 3. Treated individuals are those working in the ISIC Revision 3 “7514” code and out of Bogotá, as a proxy of working in the executive sector in the local-level agencies. “Switch jobs” is a dummy equal to 1 if the individual reports desiring to change her current job, and 0 if she does not. “Unused skills” is a dichotomous variable that takes the value of 1 if the respondent wants to switch jobs “to increase the use of her skills or training”, and 0 if this is not a reason or if she does not want to change jobs. The satisfaction index is a discrete variable ranging from 0 to 4, which indicates the number of job characteristics the survey participant is satisfied with (from the following options: (i) the type of contract she has, (ii) her current job, (iii) the wage and benefits she receives, (iv) and the job’s workday). “Satisfied with job” equals 1 if the civil servant indicates being satisfied with her job, and 0 otherwise. Standard errors clustered at the sector (ISIC 4-digit ISIC code) and department level are presented in parentheses. \* is significant at the 10% level, \*\* is significant at the 5% level, \*\*\* is significant at the 1% level.

**Table 7 – Estimates from difference-in-difference design on the agency panel with outcomes related to the mishandling of public resources**

	D(Has Sanctions)		Appearances (Asinh)		No. People (Asinh)		No. IDs (Asinh)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local Entity × After 2000	0.059*** (0.020)	0.099*** (0.027)	0.162** (0.065)	0.281*** (0.086)	0.056 (0.040)	0.136** (0.054)	0.054 (0.037)	0.125** (0.050)
Observations	39,550	36,700	39,550	36,700	39,550	36,700	39,550	36,700
Agency F.E	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E	Yes	No	Yes	No	Yes	No	Yes	No
ClassXyear F.E	No	Yes	No	Yes	No	Yes	No	Yes
Mean DV	.0673	.0636	.243	.227	.101	.0923	.0971	.0892
S.D DV	.25	.244	.946	.912	.414	.391	.4	.377

**Notes:** This table presents the estimation of  $\delta$  from Equation 4 for different measures the mishandling of public resources. "D(Any Sanction)" is a dummy taking the value of 1 if the agency appears in the Comptroller reports at any given year. Number of persons is defined by the number of raw names that appear for an entity in the quarterly levels during any given year. Number of IDs is the count of Colombian ID or tax ID numbers in the entity-year cell. Standard errors clustered at the agency level in parentheses. \* is significant at the 10% level, \*\* is significant at the 5% level, \*\*\* is significant at the 1% level.

**Table 8 – Estimates from difference-in-difference design of the effect of the change in legislation on the means to get an employment**

	Got the job via			
	(1) Calls	(2) Calls (Ind C)	(3) Family & Friends	(4) Family & Friends (Ind C)
Local Entity × After 2000	-0.206*** (0.020)	-0.208*** (0.019)	0.102*** (0.014)	0.103*** (0.013)
Observations	133,459	133,457	133,459	133,457
No. of Clusters	1,036	1,036	1,036	1,036
SectorXdep F.E	Yes	Yes	Yes	Yes
Year of entry F.E	Yes	Yes	Yes	Yes
Period of Survey F.E	Yes	Yes	Yes	Yes
Ind. Controls	No	Yes	No	Yes
Mean DV	.527	.527	.244	.244
S.D DV	.499	.499	.429	.429

**Notes:** This table presents the estimation of  $\delta$  from Equation 10, including individual controls in even columns (namely, a sex dummy, the number of years of education and the age of the respondent). Treated individuals are those working in the ISIC Revision 3 “7514” code and out of Bogotá, as a proxy of working in the executive sector in the territorial-order agencies. All dependent variables are dummies, built from a survey question where the individual answers the principal way she got her employment. “Got the job via family & friends” takes the value of 1 if the respondent answers that she asked for help to relatives, friends, or colleagues to get her actual job, and 0 if she selected one of the other options (described in the appendix). Standard errors clustered at the sector (ISIC 4-digit ISIC code) and department level are presented in parentheses. \* is significant at the 10% level, \*\* is significant at the 5% level, \*\*\* is significant at the 1% level.

**Table 9 – Estimates of heterogeneous effects to the new legislation by the education level of the public employee**

	(1)	(2)	(3)
	Personal networks	Political networks	Money
Local Entity × After 2000 × Graduate	0.035 (0.024)	0.042* (0.022)	0.022 (0.031)
Graduate × After 2000	0.030 (0.022)	0.060*** (0.021)	0.033 (0.031)
Graduate × After 2000	0.021 (0.014)	0.022 (0.014)	-0.007 (0.012)
Graduate × Local Entity	-0.037 (0.025)	-0.041* (0.025)	-0.021 (0.032)
Observations	55,040	54,097	45,280
No. of Entities	271	271	271
Entity F.E	Yes	Yes	Yes
Category of Entry F.E	Yes	Yes	Yes
Survey F.E	Yes	Yes	Yes
Mean DV	.454	.446	.693
S.D DV	.498	.497	.461

**Notes:** This table presents the estimation results for the triple difference model (Equation 11) where I interact the DD estimator with a dummy that indicates whether the survey participant has graduate education. “Personal networks” is a dummy variable equal to one if the civic employee strongly agrees or agrees with the statement that the overall hiring practices in her entity were based on personal, familial, or friendship links. “Political networks” equals 1 if she strongly agrees or agrees that the hiring practices were based on political influences, and Money=1 in the case she strongly agrees or agrees that the selection process was based on irregular payments and exchange of favors. Treated is also a dummy that equals 1 if the public employee works for an entity of the territorial order, and After 2000 takes the value of 1 if she entered in 2001 or later. Standard errors clustered at the sector of entity level are presented in parentheses. \* is significant at the 10% level, \*\* is significant at the 5% level, \*\*\* is significant at the 1% level.

**Table 10 – Heterogeneous treatment effects by education (job satisfaction outcomes)**

	(1) Switch jobs	(2) Unused skills	(3) Satisfaction index	(4) Satisfied with job
Tertiary Educ. × Local Entity × After 2000	-0.005 (0.011)	0.007 (0.008)	-0.008 (0.025)	0.013 (0.010)
Local Entity × After 2000	0.055*** (0.011)	0.027*** (0.008)	-0.173*** (0.033)	-0.016* (0.009)
Tertiary Educ. × After 2000	-0.017*** (0.004)	-0.005 (0.003)	-0.001 (0.014)	0.001 (0.004)
Tertiary Educ. × Local Entity	0.008 (0.010)	0.013* (0.007)	-0.008 (0.029)	-0.011 (0.010)
Observations	158,684	158,684	157,783	158,684
No. of Clusters	1,037	1,037	1,036	1,037
SectorXdep F.E	Yes	Yes	Yes	Yes
Year of entry F.E	Yes	Yes	Yes	Yes
Period of Survey F.E	Yes	Yes	Yes	Yes
Ind. Controls	Yes	Yes	Yes	Yes
Mean DV	.0455	.0329	2.78	.636
S.D DV	.208	.178	1.41	.481

**Notes:** This table presents the estimation results for the triple difference model (equation 12) where I interact the DD estimator with a dummy that indicates whether the individual has tertiary education. "Switch jobs" is a dummy equal to 1 if the individual reports desiring to change her current job, and 0 if she does not. "Unused skills" is a dichotomous variable that takes the value of 1 if the respondent wants to switch jobs "to increase the use of her skills or training", and 0 if this is not a reason or if she does not want to change jobs. The satisfaction index is a discrete variable ranging from 0 to 4, which indicates the number of job characteristics the survey participant is satisfied with (from the following options: (i) the type of contract she has, (ii) her current job, (iii) the wage and benefits she receives, (iv) and the job's workday). "Satisfied with job" equals 1 if the civil servant indicates being satisfied with her job, and 0 otherwise. All regressions control for sex, age, and for the "Tertiary education" dummy itself. Standard errors clustered at the sector (ISIC 4-digit ISIC code) and department level are presented in parentheses. \* is significant at the 10% level, \*\* is significant at the 5% level, \*\*\* is significant at the 1% level.

**Table 11 – Heterogeneous treatment effects by experience (job satisfaction outcomes)**

	(1) Switch jobs	(2) Unused skills	(3) Satisfaction index	(4) Satisfied with job
Experienced × Local Entity × After 2000	0.096 (0.095)	0.071 (0.079)	-0.262** (0.106)	-0.047 (0.047)
Local Entity × After 2000	-0.056 (0.094)	-0.047 (0.078)	0.123 (0.105)	0.045 (0.046)
Experienced × After 2000	0.042** (0.019)	0.032** (0.016)	-0.130*** (0.049)	-0.019 (0.015)
Experienced × Local Entity	-0.118 (0.093)	-0.087 (0.077)	0.347*** (0.102)	0.054 (0.045)
Observations	152,492	152,492	151,982	152,492
No. of Clusters	1,027	1,027	1,026	1,027
SectorXdep F.E	Yes	Yes	Yes	Yes
Year of entry F.E	Yes	Yes	Yes	Yes
Period of Survey F.E	Yes	Yes	Yes	Yes
Ind. Controls	Yes	Yes	Yes	Yes
Mean DV	.0454	.033	2.78	.636
S.D DV	.208	.179	1.41	.481

**Notes:** This table presents the estimation results for the triple difference model (equation 13) where I interact the DD estimator with a dummy that indicates whether the individual has contributed to pension above the median of the sample, as a proxy for experience. "Switch jobs" is a dummy equal to 1 if the individual reports desiring to change her current job, and 0 if she does not. "Unused skills" is a dichotomous variable that takes the value of 1 if the respondent wants to switch jobs "to increase the use of her skills or training", and 0 if this is not a reason or if she does not want to change jobs. The satisfaction index is a discrete variable ranging from 0 to 4, which indicates the number of job characteristics the survey participant is satisfied with (from the following options: (i) the type of contract she has, (ii) her current job, (iii) the wage and benefits she receives, (iv) and the job's workday). "Satisfied with job" equals 1 if the civil servant indicates being satisfied with her job, and 0 otherwise. All regressions control for sex, age, education years, and the "Experienced" dummy itself. Standard errors clustered at the sector (ISIC 4-digit ISIC code) and department level are presented in parentheses. \* is significant at the 10% level, \*\* is significant at the 5% level, \*\*\* is significant at the 1% level.

## A – Data Appendix

### A.1. Description of the Comptroller General's data on the mishandling of public resources

The Comptroller General of Colombia publishes quarterly bulletins with the names and IDs of individuals or enterprises sanctioned for the mishandling of public resources and that have not met their sanctions' obligations<sup>15</sup>. Carreri and Martinez (2022) digitized these bulletins from 1995 to 2019, which also contain the name of the affected entity. I use exact and string-distance-based matching to get the tax ID codes of the affected entities from the directories of public entities of the Colombian Official Institute of Statistics (DANE) and the Department of the Civil Service (*Función Pública*)<sup>16</sup>. The final raw input data is a compilation of the quarterly bulletins with the name and ID of the sanctioned individual or enterprise, the year and number of the bulletin, and the tax ID of the affected entity.

I collapse this data at the entity (formerly, its tax ID) and year level to create a balanced panel of agencies from 1995 to 2019 containing the following outcomes:

- D(Has Sanctions): A dummy variable indicating that the entity has at least one observation in any of the quarterly bulletins of a given year (i.e., it is mentioned as an affected entity at least once). The creation of a balanced panel is crucial for this variable. This variable can be interpreted as the extensive margin measure of mishandling of public resources.
- Appearances: This variable is created by counting the number of observations an entity appears as being affected in all the bulletins for a given year.

15. These individuals and enterprises are called “fiscal responsible” (*responsables fiscales* in Spanish)

16. For the string-distance-based matching I select the entities in the DANE and *Función Pública* directories whose names are the closest to a given agency's name in the Comptroller General's data. I compute the string distance based on the Jaccard distance between q-gram profiles (with  $q = 3$ ). I do a manual revision that excludes 71% of the string-distance matches. In the end, I match 52.89% of the observations of the bulletins' raw string data. Observations where it was not possible to get the affected entity's tax ID (i.e., the remaining 47.1%) were excluded from the analysis.

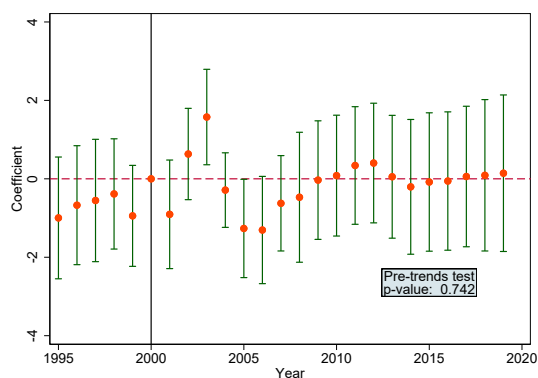
- Number of sanctioned people: The creation of this variable follows a process where I first collapse the data at the entity-year-name of the sanctioned individual/enterprise level. After having one observation per individual/enterprise-entity-year, I count the names of individuals or enterprises by entity and year.
- Number of sanctioned IDs: The creation of this variable follows a similar process to that in the previous variable, but instead, I count the number of tax (or personal) IDs of sanctioned enterprises (or individuals) by entity and year.

The last three variables can be interpreted as intensive-margin measures of the mishandling of public resources. To account for possible outliers, I windsorized these outcomes to the percentiles 1 and 99 of the overall sample and transformed them using the inverse hyperbolic sine. The analyses of the paper present both the windsorized and the transformed versions of these measures.

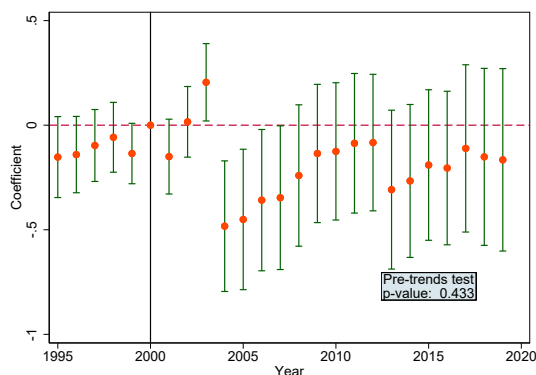
Finally, I get the treatment variable (whether the entity is from the executive branch of territorial-order governments) from the DANE and *Función Pública* directories. I define a dummy  $\mathbb{1}[Territorial]$  equal to 1 if the agency is in the Municipal, District, or Department Order in the DANE dataset –which contains a string column named “order”–, or in the Territorial Order of *Función Pública*’s directory. The dummy takes the value of 0 if the agency belongs to the national order or is not from the executive branch. From the DANE directory, I also get the organic class of the agency, with six possible categories: administrative corporations (mainly municipal councils and department assemblies), executive-branch agencies, autonomous entities, judiciary-branch agencies, legislative-branch agencies, and surveillance institutions).

## B – Additional Results

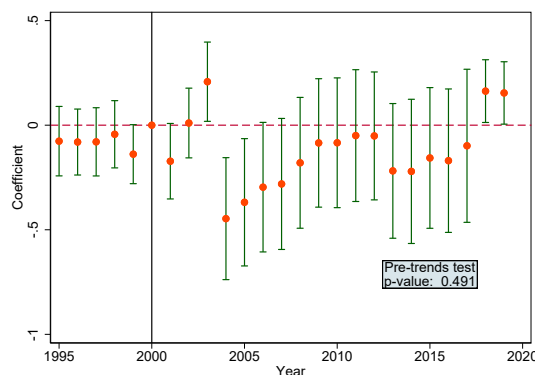
Figure B1 – Dynamic effects of the change in legislation on mishandling of public resources - outcomes without transformation



(a) Number of appearances



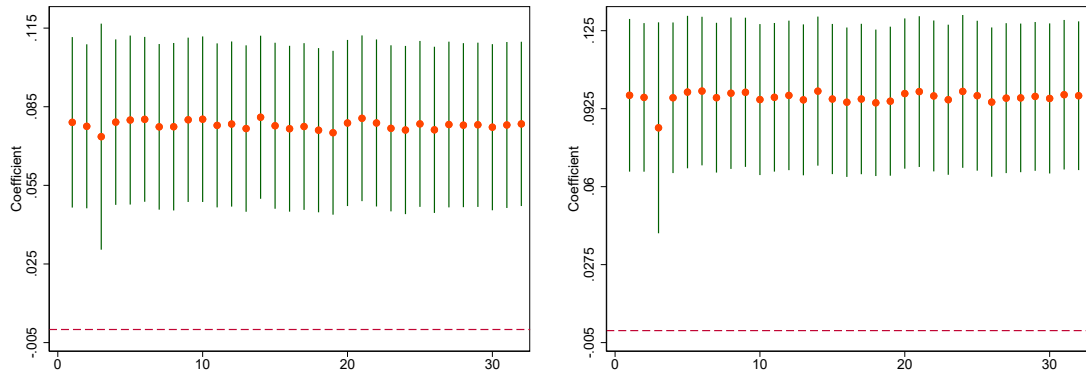
(b) Number of persons



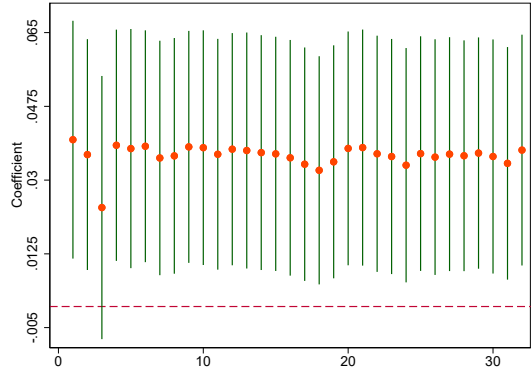
(c) Number of IDs

**Notes:** This figure reports the estimated coefficients from the dynamic specification established in equation 5 for different measures the mishandling of public resources, without being transformed using the inverse hyperbolic sine. The number of persons is defined by the number of raw names that appear for an entity quarterly during any given year. The number of IDs is the count of Colombian or tax ID numbers in the entity-year cell. All variables are winsorized at the percentiles 1 and 99. I present point estimates and their intervals of confidence at 95%. Standard errors are clustered at the agency level.

**Figure B2 – Department exclusion in corruption perceptions' models**



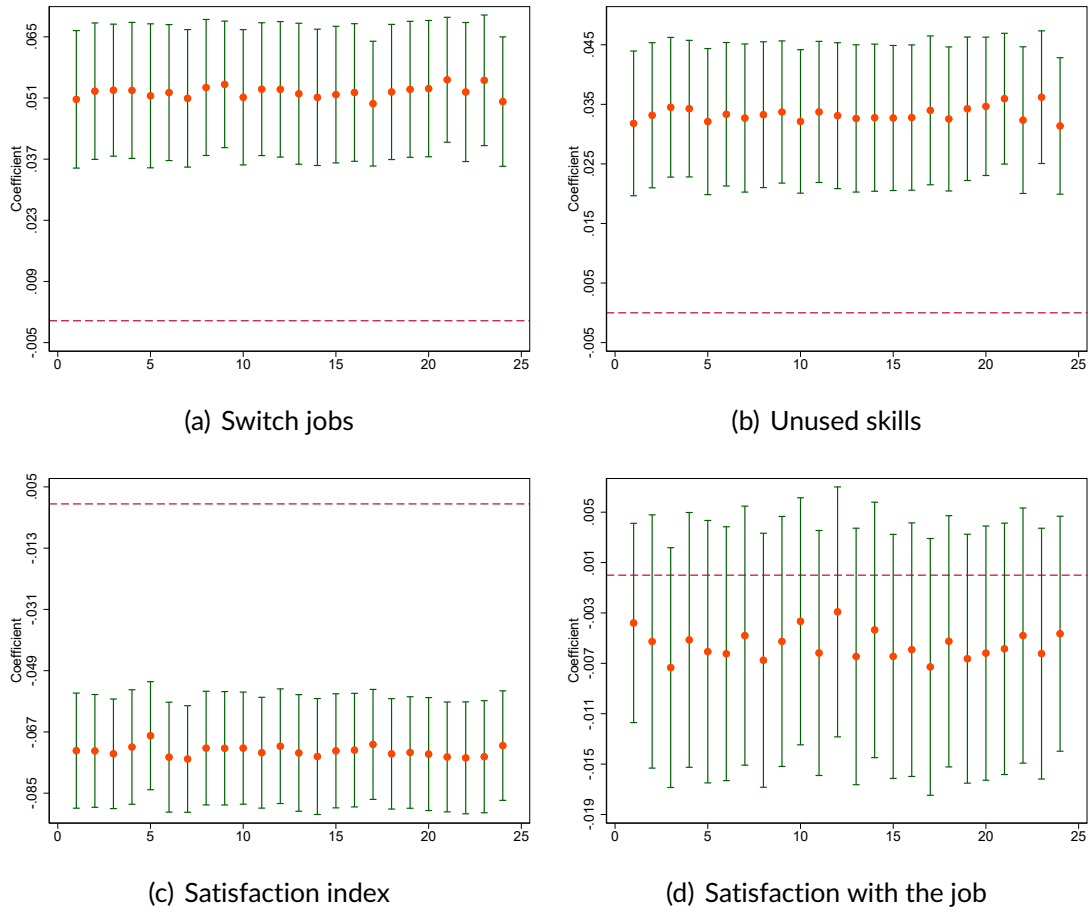
(a) Hiring based on personal, familial, or friend-ship links (personal networks) (b) Hiring based on political influences (political networks)



(c) Hiring based on money or exchange of favors (money)

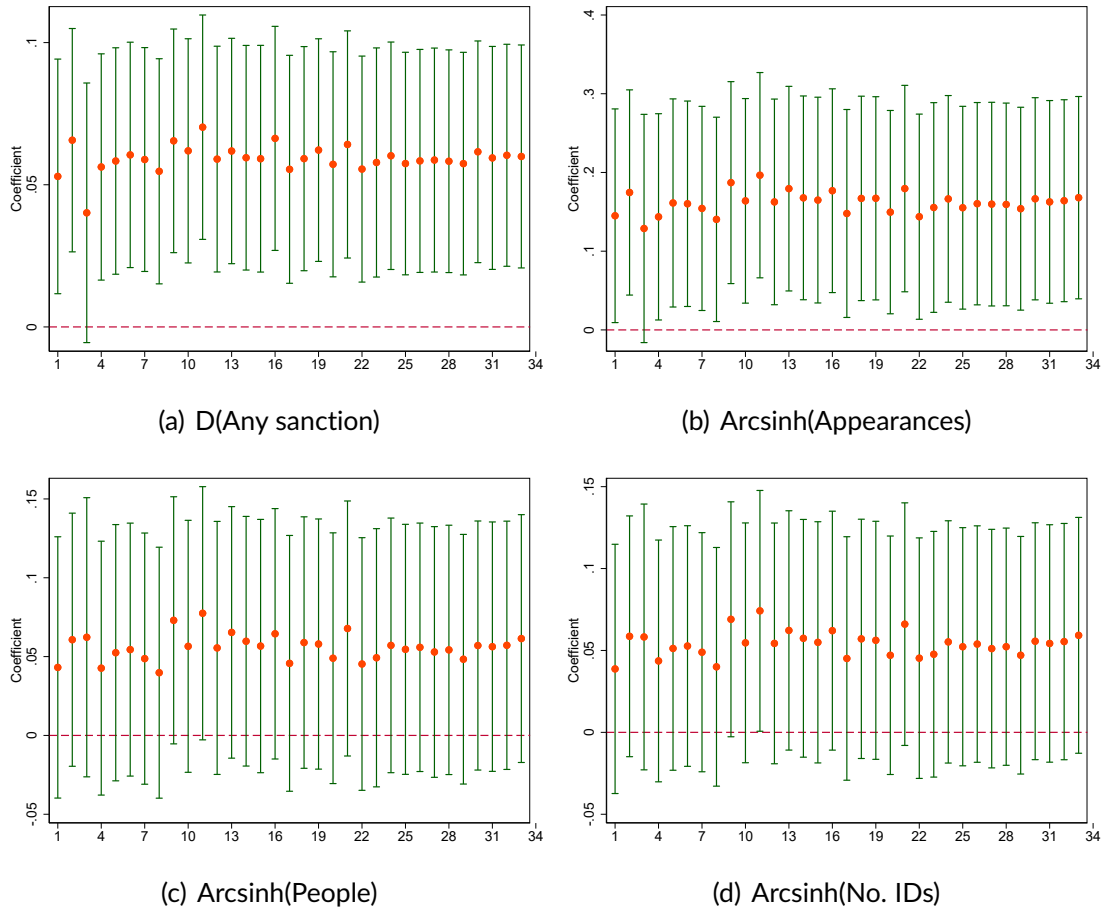
**Notes:** This figure reports the *difference-in-differences* estimates for the corruption perceptions outcomes ( $\delta$  in Equation 2), excluding one department at a time. The x-axis indicates the excluded department. I present point estimates and their intervals of confidence at 95%. Standard errors are clustered at the entity level.

Figure B3 – Department exclusion in satisfaction models



**Notes:** This figure reports the *difference-in-differences* estimates for the outcomes of satisfaction with job characteristics ( $\delta$  in Equation 9), excluding one department at a time. The x-axis indicates the excluded department. All regressions control for sex, the number of years of education, and the age of the respondent. I present point estimates and their intervals of confidence at 95%. Standard errors clustered at the sector (ISIC 4-digit ISIC code) and department level are presented in parentheses.

Figure B4 – Department exclusion in models of mishandling of public resources



**Notes:** This figure reports the *difference-in-differences* estimates for the outcomes of mishandling of public resources ( $\delta$  in Equation 9), excluding one department at a time. The x-axis indicates the excluded department. I present point estimates and their intervals of confidence at 95%. Standard errors are clustered at the entity level.

**Table B1 – Estimates from difference-in-difference design using category of year of entry fixed effects and individual-level controls**

	Personal networks		Political networks		Money	
	(1) Complete sample	(2) Ind. Controls	(3) Complete sample	(4) Ind. Controls	(5) Complete sample	(6) Ind. Controls
Local Entity × After 2000	0.078*** (0.016)	0.052*** (0.015)	0.097*** (0.016)	0.076*** (0.015)	0.036** (0.014)	0.019 (0.013)
Observations	128,961	128,961	127,067	127,067	107,269	107,269
No. of Entities	281	281	281	281	281	281
R-squared	.0866	.12	.14	.172	.0644	.081
Entity F.E	Yes	Yes	Yes	Yes	Yes	Yes
Category of Entry F.E	Yes	Yes	Yes	Yes	Yes	Yes
Survey F.E	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes
Mean DV	.381	.381	.391	.391	.786	.786
S.D DV	.486	.486	.488	.488	.41	.41

**Notes:** This table presents the estimation results for the expanded specification (equation 8). "Personal networks" is a dummy variable equal to one if the civic employee strongly agrees or agrees with the statement that the overall hiring practices in her entity were based on personal, familial, or friendship links. "Political networks" equals 1 if she strongly agrees or agrees that the hiring practices were based on political influences, and Money=1 in the case she strongly agrees or agrees that the selection process was based on irregular payments and exchange of favors. Treated is also a dummy that equals 1 if the public employee works for an entity of the territorial order, and After 2000 takes the value of 1 if she entered in 2001 or later. Standard errors clustered at the sector of entity level are presented in parentheses. \* is significant at the 10% level, \*\* is significant at the 5% level, \*\*\* is significant at the 1% level.

**Table B2 – Estimates from the difference-in-difference design of the change in legislation on job satisfaction outcomes (with individual controls)**

	Switch jobs		Unused skills		Satisfaction index		Satisfied with job	
	(1) Baseline	(2) Ind C	(3) Baseline	(4) Ind C	(5) Baseline	(6) Ind C	(7) Baseline	(8) Ind C
Local Entity × After 2000	0.052*** (0.008)	0.052*** (0.008)	0.033*** (0.006)	0.033*** (0.006)	-0.179*** (0.022)	-0.180*** (0.022)	-0.006 (0.005)	-0.006 (0.005)
Observations	158,689	158,684	158,689	158,684	157,788	157,783	158,689	158,684
No. of Clusters	1,037	1,037	1,037	1,037	1,036	1,036	1,037	1,037
SectorXdep F.E	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of entry F.E	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period of Survey F.E	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind. Controls	No	Yes	No	Yes	No	Yes	No	Yes
Mean DV	.0455	.0455	.0329	.0329	2.78	2.78	.636	.636
S.D DV	.208	.208	.178	.178	1.41	1.41	.481	.481

**Notes:** This table presents the estimation of  $\delta$  from Equation 9, which includes individual controls in even columns (namely, a sex dummy, the number of years of education and the age of the respondent). Treated individuals are those working in the ISIC Revision 3 “7514” code and out of Bogotá, as a proxy of working in the executive sector in the territorial-order agencies. “Switch jobs” is a dummy equal to 1 if the individual reports desiring to change her current job, and 0 if she does not. “Unused skills” is a dichotomous variable that takes the value of 1 if the respondent wants to switch jobs “to increase the use of her skills or training”, and 0 if this is not a reason or if she does not want to change jobs. The satisfaction index is a discrete variable ranging from 0 to 4, which indicates the number of job characteristics the survey participant is satisfied with (from the following options: (i) the type of contract she has, (ii) her current job, (iii) the wage and benefits she receives, (iv) and the job’s workday). “Satisfied with job” equals 1 if the civil servant indicates being satisfied with her job, and 0 otherwise. Standard errors clustered at the sector (ISIC 4-digit ISIC code) and department level are presented in parentheses. \* is significant at the 10% level, \*\* is significant at the 5% level, \*\*\* is significant at the 1% level.