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

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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 *diffence-in-differences* design, I find a relative increase in the awareness of nepotism, the influence of political networks, and the role 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 satisfaction with job characteristics, 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 propose that an image of unfairness in hiring practices might drive the effects on perceptions and satisfaction.

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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. 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).

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. Despite the constitutional principle of meritocracy to select and retain public servants, Colombian legislation has incentivized short-term contracts that allow recruiters to omit tenders or civil service calls. I specifically look at the impact of a 2000 law that prohibited executive-branch government agencies at the territorial level from having real growth in their personnel expenditures.

I argue that this change in legislation had differential impacts on the hiring practices of public entities depending on their legal nature, which caused the executive branch of the territorial order government to be the most exposed to discretionary hiring practices (meaning that the use of temporary contracts to select bureaucrats

had the highest growth in these agencies). They started using these contracts to circumvent the law and the fact that there were no meritocratic processes to recruit territorial-level government employees before 2020. Additionally, a procurement reform in 2007 established that this type of contract does not require competitive processes as they fall into the direct procurement regime, further increasing their discretionary feature.

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 between bureaucrats. 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 decreased job satisfaction and deteriorated 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 the satisfaction with job characteristics of public employees that entered territorial-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 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 household survey data that includes questions about satisfaction with the contract, the wage, the workday, and the job itself. Civil servants of the territorial level government (including contractors) experienced a differential reduction in all measures of job satisfaction, although the impact on the job itself is inconclusive. The most considerable effect is satisfaction with the contract (with a relative decrease of 8.4 percentage points), followed by the wage (7.3 percentage points). These magnitudes respectively represent 8.6% and 12.7% of the averages before 2000 and do not substantially 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 deferentially 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. Although the magnitude of the impact might seem small (as it is estimated at around six percentage points), it is substantially large when compared to pre-reform salience of the problem[1]. The evidence 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 entries became more salient after the change in legislation. Household survey data in Colombia includes a question to occupied individuals about the primary way in which they got their jobs, which allows me to compare

1. Only 6.7% of the agencies appeared in the sanctions data before 2000, so the impact present about 87% of this average, and around 23% of the standard deviation

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 calls or job announcements. 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 satisfaction with job characteristics, although the evidence is only conclusive for wage satisfaction. More educated workers were also the ones where the reduction in wage and contract satisfaction was larger, but these results are weak and disparate[2].

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 (2021) study the impacts of an 1883 act in the

2. The coefficients were not statistically significant, and they suggest that the reduction in the satisfaction with the job and the workday was smaller for respondents with higher education levels

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 (2021) 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) find that politically connected governors during the British Empire generated less public 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. (2021) 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 national-level civil service reform that promoted temporary and direct hiring in a developing country, in contrast to Moreira and Pérez (2021); Muñoz and Prem (2022); Dahis et al. (2021), and Xu (2018), that focus on specific positions or servants (i.e., customs officers, schools principals, and judges). 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 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 exposition 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 the questions on corruptions 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, 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 and territorial 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 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 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 Úribe 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)[3].

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

3. 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.

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 functioning budget of the entities (Bernal et al., 2017). 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 servants 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 contests 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 functioning 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 [4]. Relying on monthly household survey data spanning the period 2010–2018, I estimate the following model in the subset of public employees:

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

where i,s,d,c,and t respectively indicate the survey participant, the 4-digit economic sector, the department where she works, the year in which she entered her current job, and the period of the survey (composed by month and year). The dependent variable, $DiscCrontract_{isdct}$ is a dummy that proxies whether the employee holds

4. 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.

a discretionary and temporary contract. δ 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 retirement 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[5]), 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

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

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[6] 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.

I construct 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 they do not respond for a specific time in the public entity, but they 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[7].

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

7. The cohort bins are constructed by subtracting the upper and lower bounds of the answer from

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 orders of 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. Satisfaction with job-related characteristics

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 surveys contain four questions about satisfaction with job-related characteristics, which are applied to occupied individuals. They respectively ask if the person is satisfied with (i) the type of contract she has, (ii) her current job, (iii) the wage and benefits she receives, (iii) and the job's workday. As all of them are "Closed" (they can only be answered with a Yes or a No), I construct four dummies indicating that

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 respondent is satisfied with that specific feature.

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 [8]. I use this information to construct the year of entry cohorts by subtracting the number 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.[9] 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 getting their jobs via

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

9. 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.

calls and a higher percentage 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, I use information from the quarterly bulletins containing the list of individuals and organizations sanctioned for the mishandling of 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 (2021). 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:

$$Y_{iect} = \alpha_t + \beta_e + \gamma_c + \delta(LocalEntity_e \times After2000_c) + \varepsilon_{iect} \quad (2)$$

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 (α_t), the cohort of entry-level (γ_c), and the entity level (β_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 what their time of experience in a set of bins is. Thus, some cohorts contain pre- and post-treatment periods (for example, the 1998-2002 cohort). 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:

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

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 identify the agency where the survey partici-

pant 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 out of Bogotá, as a proxy of working in the executive sector in the territorial order. The regression includes fixed effects at the sector (4-digit ISIC code) and department level (β_{sd}), the year of entry-level γ_c, and the survey period α_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:

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

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 order, and $After2000_t$ takes the value of one for the period 2001 and after. I include fixed effects at the entity level (β_e) and the year level (α_t). Finally, ε_{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 [10].

4.1. Validity of the empirical strategy

My coefficients of interest, δ, capture the change in corruption before and after the introduction of Law 617 in the entities of the territorial order, compared to those of 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 rep-

10. I also estimate another specification where instead of including year fixed effects, I control for different intercepts of year and class of the agency. This specification allows for comparing the evolution of the outcomes among agencies of the same class.

resent 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:

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

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 θ_τ 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:

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

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 θ_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:

$$Y_{isdct} = \alpha_s + \beta_{sd} + \gamma_c + \sum_{j \in C} \theta_j (Treated_{sd} \times \mathbb{1}[c = j]) + \varepsilon_{isct} \tag{7}$$

here, C includes all cohorts in the sample except 2000, and the parameters θ_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, in the second and third columns the statements respectively indicate the influence of politicians, or of money and exchange of favors. I include entity, 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 decreased satisfaction with their contracts by 8.4 percentage points on average, relative to public employees in other sectors or those in the capital (as a proxy of working in the national order). There are also wage, workday, and overall job satisfaction reductions, by 7.3pp, 1.6pp, and 0.6pp, respectively, although the last one is not statistically significant. The decrease in contract satisfaction represents 8.7% of the variable's mean for the pre-treatment cohorts, whereas the other coefficients account for 12,8%, 2.7%, and 0.9% of their respective outcomes' mean before the change in legislation. The magnitudes are equivalent to 49.7%, 14.8%, 3.2%, and 1.3% 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 two-way fixed effects specifications, and even columns add class and year fixed effects, which allow for better comparisons between agencies of the same nature [11]. Rela-

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

tive 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 θ_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 c and d, which refer respectively to satisfaction with the wage and the workday, show the strongest evidence against pre-trends before the change in legislation (with p-values of 0.871 and 0.932 for the joint hypothesis F test of all coefficients equaling 0). Panel b (overall job satisfaction) brings weaker evidence for the parallel-trends assumption with a p-value of 0.161, although it reaffirms negative but non-significant effects found

tities, judiciary-branch agencies, legislative-branch agencies, and surveillance institutions)

in table 6 for this variable. Finally, panel a, which refers to satisfaction with the contract, brings weak evidence for the identifying assumption, with a p-value of a joint hypothesis test for null pre-trends of 0.045, and one coefficient – θ_{1995} – rejecting the null hypothesis of being non-significant under a 95% confidence interval.

I complement the design of dynamic effects with a parametric test for the existence of differential trends in the measures of satisfaction before the implementation of Law 617 (from cohorts 1994 to 2000). I do so by interacting a linear trend for cohorts with the treatment dummy that proxies if the survey respondent works for a territorial-order agency of the executive branch of the government, and check if the associated coefficient is statistically significant[12].

Table B3 reports the estimated coefficient for the four satisfaction measures. Odd columns present specifications that do not control for individual-level characteristics, whereas the models in even columns do. All the coefficients are small (representing 0.17% to 0.51% of the dependent variables' averages depending on the outcome) and not statistically significant under a significance level of 0.05. These results suggest that there were no differential trends in job satisfaction between public employees working for territorial executive-branch agencies and those in other branches or the national government.

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, panels b, d, and f report 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 this plot allows seeing that the post-treatment effects are not robust to the functional form of the outcomes for the number of persons and the number of IDs, since

12. I run the following specification for each measure of satisfaction: $Y_{isdct} = \alpha_s + \beta_{sd} + \gamma LinearTrend_c + \delta(Treated_{sd} \times LinearTrend_c) + \varepsilon_{isct}$ The parameter of interest, δ, shows whether the job satisfaction outcomes followed differential linear trends in territorial-order executive-branch entities relative to those in the national level or other branches.

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:

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

for the outcomes of corruption perceptions, and:

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

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 (δ) 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. There is no reduction in the magnitudes when including controls, except for wage satisfaction (which is reduced by 1 pp). There are also no changes in the statistical significance, suggesting that the impacts are robust for this aspect.

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 noncompetitive 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 selected in one way or another changed after the change in legislation. I formally estimate the following model in the sample of public employees:

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

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 nepotistic 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 outcomes should be higher for more educated and experienced public employees. They had to face higher financial and time costs, making the perception of unfairness higher when exposed to discretionary hiring for contractors. 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:

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

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 postgraduate 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:

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

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

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 contain a direct question on the employee's experience, so I construct a proxy with the number of years contributing to pension. The variable $Experienced_{isdct}$ takes the value of 1 if the respondent has contributed 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 θ 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 coefficients are negative for the satisfaction with the contract and the wage. However, they are not statistically significant (the smallest p-value is for contract satisfaction: 0.19). More robust evidence for the hypothesis comes from the experience, with all coefficients being negative. Considering that the *difference-in-difference* estimator is positive, it means that the satisfaction outcomes decreased after the legislation change only for those with an experience above the median. Nonetheless, the triple interaction is only significant for satisfaction with the wage (p-value=0.1).

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 en-

tities, 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 decrease in all measures of job satisfaction, with the highest drop in contract satisfaction (8.4 percentage points on average, equivalent to 8.6% of the pre-period mean). 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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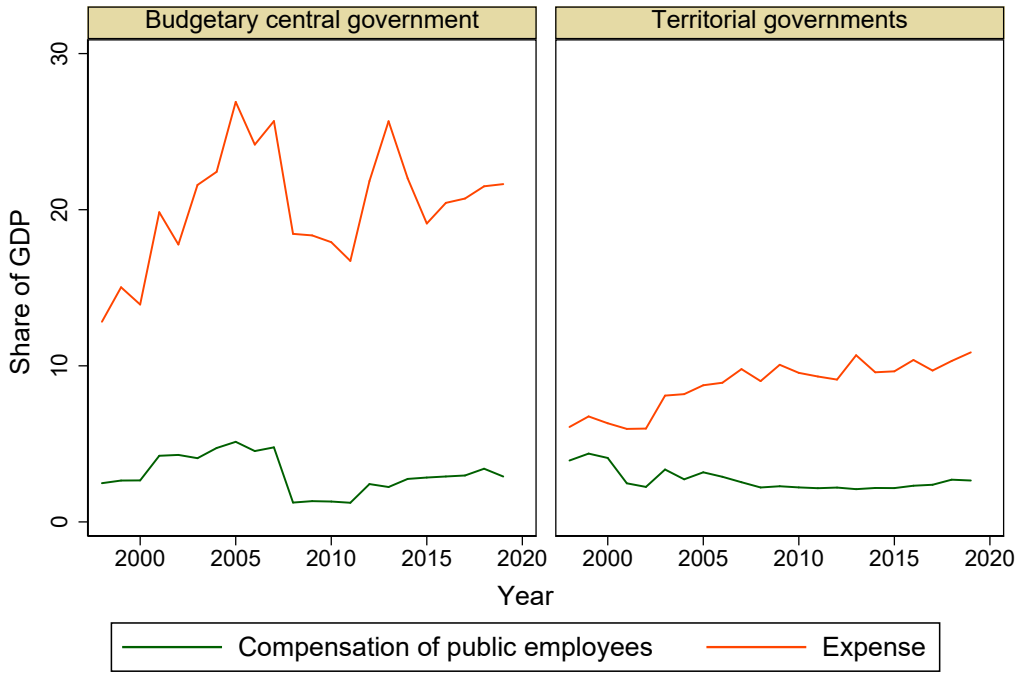
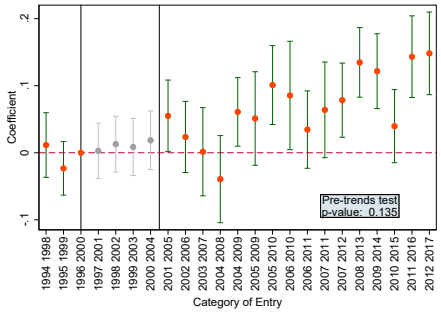
Figure 1 — Evolution of total expense and payroll in the Colombian public sector, 1998–2019

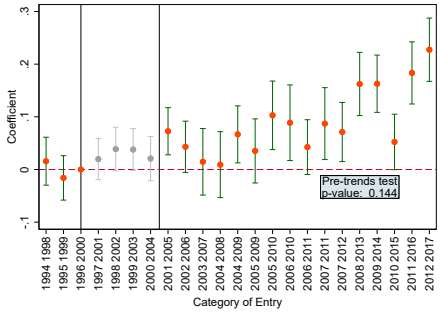


Notes: This figure presents the evolution of total expense and the compensation of employees in the Colombian public sector as a share of GDP. Public employee compensation includes wages, salaries, and the employer's social contributions. Budgetary central government encompasses the fundamental activities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

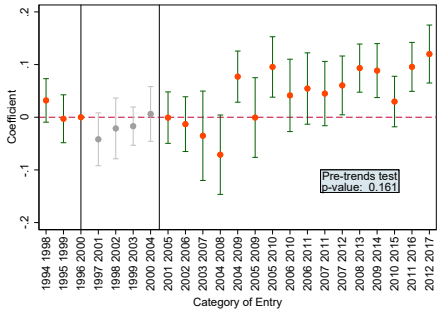
Figure 2 — Dynamic effects of the change in legislation on corruption perceptions



(a) Hiring based on personal, familial, or friendship links (personal networks)



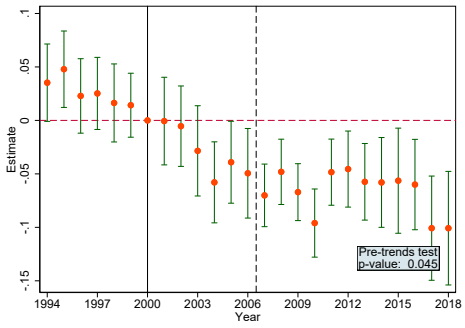
(b) Hiring based on political influences (political networks)



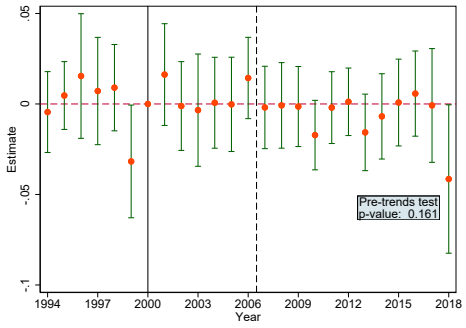
(c) Hiring based on money or exchange of favors (money)

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.

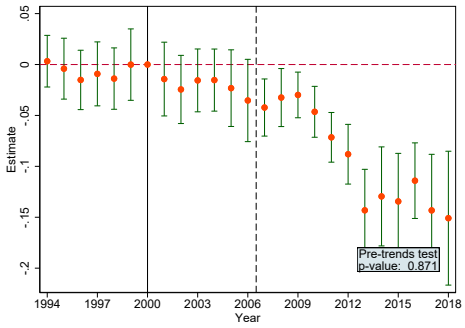
Figure 3 — Dynamic effects of the change in legislation on job satisfaction outcomes



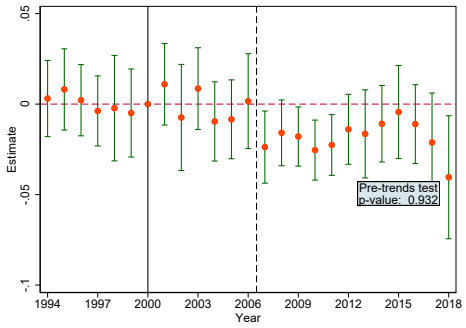
(a) Satisfaction with the contract



(b) Satisfaction with the job



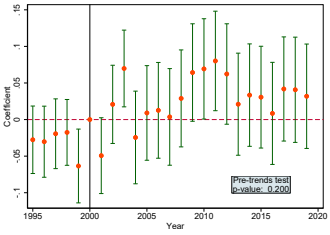
(c) Satisfaction with the wage



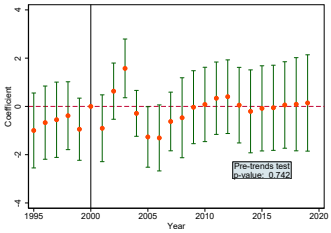
(d) Satisfaction with the workday

Notes: This figure reports the estimated coefficients from the dynamic specification established in equation 7 for different measures of job satisfaction. All the dependent variables are dummies built from survey questions where individuals respond if they are satisfied or not with the specific job dimensions. "Satisfaction with the wage" is a dummy equal to 1 if the individual is satisfied with the benefits she receives, which include the wage (but might comprise other elements). 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.

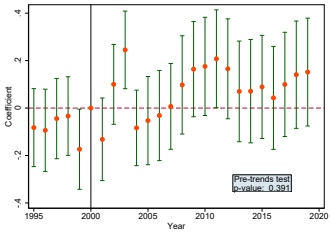
Figure 4 — Dynamic effect of the change in legislation on the mishandling of public resources



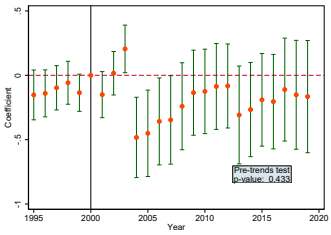
(a) D(Any sanction)



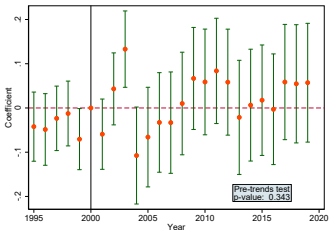
(b) Number of appearances



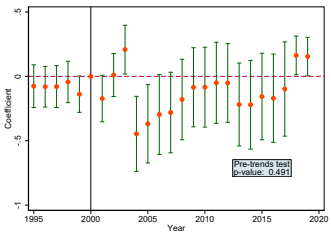
(c) Arcsinh(Appearances)



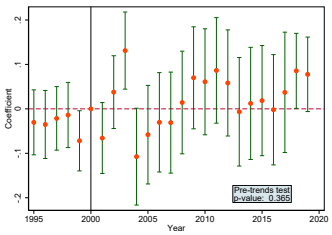
(d) Number of persons



(e) Arcsinh(People)



(f) Number of IDs



(g) 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 windsorized 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
		National	Territorial	p-value
Outcomes				
Contracts based on personal network	0.469	0.488	0.402	0.000
	(0.001)	(0.001)	(0.003)	
Contracts based on political influences	0.479	0.518	0.347	0.000
	(0.001)	(0.001)	(0.002)	
Contracts based on money and favors	0.758	0.780	0.680	0.000
	(0.001)	(0.001)	(0.003)	
Covariates				
Woman	0.549	0.543	0.571	0.000
	(0.001)	(0.001)	(0.002)	
Directive position	0.138	0.144	0.116	0.000
	(0.001)	(0.001)	(0.001)	
Professional position	0.619	0.623	0.607	0.000
	(0.001)	(0.001)	(0.002)	
D(Tertiary Education)	0.979	0.980	0.974	0.000
	(0.000)	(0.000)	(0.001)	
D(Graduate education)	0.205	0.212	0.179	0.000
	(0.001)	(0.001)	(0.002)	
Treatment variables				
After 2000	0.797	0.805	0.766	0.000
	(0.001)	(0.001)	(0.002)	
Treated 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	Overall	Entity treatment status Control	Treated	Difference p-value
Outcomes				
D(Satisfied with the contract)	0.897	0.919	0.812	0.000
	(0.001)	(0.001)	(0.002)	
D(Satisfied with the job)	0.677	0.688	0.635	0.000
	(0.001)	(0.001)	(0.003)	
D(Satisfied with the wage)	0.571	0.598	0.469	0.000
	(0.001)	(0.001)	(0.003)	
D(Satisfied with the workday)	0.648	0.654	0.625	0.000
	(0.001)	(0.001)	(0.003)	
D(Self-Employed)	0.153	0.078	0.438	0.000
	(0.001)	(0.001)	(0.003)	
D(Full-Retirement Contributions)	0.143	0.076	0.406	0.000
	(0.001)	(0.001)	(0.003)	
D(Fixed-term contract)	0.280	0.207	0.562	0.000
	(0.001)	(0.001)	(0.003)	
D(Got job via calls)	0.476	0.511	0.259	0.000
	(0.001)	(0.001)	(0.003)	
D(Got job via family & friends)	0.265	0.239	0.426	0.000
	(0.001)	(0.001)	(0.004)	
Covariates				
Female	0.490	0.487	0.501	0.000
	(0.001)	(0.001)	(0.003)	
Age	38.680	38.597	38.997	0.000
	(0.026)	(0.029)	(0.059)	
Years of education	14.737	14.791	14.529	0.000
	(0.008)	(0.009)	(0.019)	
Years contributing to pension	11.873	12.172	10.701	0.000
	(0.029)	(0.031)	(0.068)	
D(Tertiary education)	0.775	0.774	0.779	0.088
	(0.001)	(0.001)	(0.002)	
Treatment variables				
D(Entered after 2000)	0.792	0.766	0.890	0.000
	(0.001)	(0.001)	(0.002)	
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). All dependent variables are dummies. "Satisfaction with the wage" is a dummy equal to 1 if the individual is satisfied with the benefits she receives, which include the wage (but might comprise other elements). "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
		Control	Treated	p-value
Outcomes				
Had at least one sanction	0.367	0.384	0.363	0.001
	(0.002)	(0.006)	(0.003)	
Appearances (Windsorized)	4.855	6.030	4.588	0.000
	(0.055)	(0.155)	(0.058)	
Number of sanctioned people (Windsorized)	1.154	1.420	1.093	0.000
	(0.012)	(0.034)	(0.012)	
Number of sanctioned IDs (Windsorized)	1.009	1.229	0.960	0.000
	(0.010)	(0.029)	(0.011)	
Treatment variables				
I(After 2000)	0.800	0.800	0.800	1.000
	(0.002)	(0.005)	(0.002)	
I(Territorial order agency)	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
Treated × After 2000	0.339***	0.312***	0.295***
	(0.016)	(0.016)	(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 δ 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) Personal networks	(2) Political networks	(3) Money
Local Entity X After 2000	0.078***	0.097***	0.036**
	(0.016)	(0.016)	(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

	Satisfaction with			
	(1) Contract	(2) Job	(3) Wage	(4) Workday
Treated × After 2000	-0.084***	-0.006	-0.073***	-0.016***
	(0.011)	(0.005)	(0.008)	(0.005)
Observations	157,788	158,689	158,689	158,689
No. of Clusters	1,036	1,037	1,037	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	.97	.636	.572	.6
S.D DV	.169	.481	.495	.49

Notes: This table presents the estimation of δ 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 territorial-order agencies. All dependent variables are dummies. "Satisfaction with the wage" is a dummy equal to 1 if the individual is satisfied with the benefits she receives, which include the wage (but might comprise other elements). 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)
I (Territorial $\times$ After 2000)	0.059***	0.099***	0.162**	0.281***	0.056	0.136**	0.054	0.125**
	(0.020)	(0.027)	(0.065)	(0.086)	(0.040)	(0.054)	(0.037)	(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 δ 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)
Treated × After 2000	-0.206***	-0.208***	0.102***	0.103***
	(0.020)	(0.019)	(0.014)	(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 δ 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) Personal networks	(2) Political networks	(3) Money
Local Entity × After 2000 × Graduate	0.035	0.042*	0.022
	(0.024)	(0.022)	(0.031)
Graduate × After 2000	0.030	0.060***	0.033
	(0.022)	(0.021)	(0.031)
Graduate × After 2000	0.021	0.022	-0.007
	(0.014)	(0.014)	(0.012)
Graduate × Local Entity	-0.037	-0.041*	-0.021
	(0.025)	(0.025)	(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)

	Satisfaction with			
	(1) Contract	(2) Job	(3) Wage	(4) Workday
Tertiary Educ. × Treated × After 2000	-0.017	0.013	-0.005	0.004
	(0.013)	(0.010)	(0.012)	(0.008)
Treated × After 2000	-0.071***	-0.016*	-0.069***	-0.019**
	(0.016)	(0.009)	(0.013)	(0.009)
Tertiary Educ. × After 2000	-0.007*	0.001	-0.001	0.006
	(0.004)	(0.004)	(0.005)	(0.005)
Tertiary Educ. × Treated	0.013	-0.011	-0.010	-0.001
	(0.011)	(0.010)	(0.014)	(0.009)
Observations	157,783	158,684	158,684	158,684
No. of Clusters	1,036	1,037	1,037	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	.97	.636	.572	.6
S.D DV	.169	.481	.495	.49

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. All dependent variables are dummies. "Satisfaction with the wage" is a dummy equal to 1 if the individual is satisfied with the benefits she receives, which include the wage (but might comprise other elements). 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)

	Satisfaction with			
	(1) Contract	(2) Job	(3) Wage	(4) Workday
Experienced × Treated × After 2000	-0.081	-0.047	-0.101*	-0.044
	(0.061)	(0.047)	(0.062)	(0.042)
Treated × After 2000	0.012	0.045	0.045	0.031
	(0.063)	(0.046)	(0.061)	(0.041)
Experienced × After 2000	-0.049**	-0.019	-0.045**	-0.019
	(0.020)	(0.015)	(0.021)	(0.015)
Experienced × Treated	0.113*	0.054	0.140**	0.050
	(0.062)	(0.045)	(0.063)	(0.041)
Observations	151,982	152,492	152,492	152,492
No. of Clusters	1,026	1,027	1,027	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	.971	.636	.572	.6
S.D DV	.168	.481	.495	.49

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. All dependent variables are dummies. "Satisfaction with the wage" is a dummy equal to 1 if the individual is satisfied with the benefits she receives, which include the wage (but might comprise other elements). 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 [13]. Carreri and Martinez (2021) 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*)[14]. 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.

13. These individuals and enterprises are called "fiscal responsible" (*responsables fiscales* in Spanish)

14. 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

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 X After 2000	0.078***	0.052***	0.097***	0.076***	0.036**	0.019
	(0.016)	(0.015)	(0.016)	(0.015)	(0.014)	(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)

	Satisfaction with							
	(1) Contract	(2) Contract (Ind C)	(3) Job	(4) Job (Ind C)	(5) Wage	(6) Wage (Ind C)	(7) Workday	(8) Workday (Ind C)
Treated × After 2000	-0.084***	-0.084***	-0.006	-0.006	-0.073***	-0.072***	-0.016***	-0.016***
	(0.011)	(0.011)	(0.005)	(0.005)	(0.008)	(0.008)	(0.005)	(0.005)
Observations	157,788	157,783	158,689	158,684	158,689	158,684	158,689	158,684
No. of Clusters	1,036	1,036	1,037	1,037	1,037	1,037	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	.97	.97	.636	.636	.572	.572	.6	.6
S.D DV	.169	.169	.481	.481	.495	.495	.49	.49

Notes: This table presents the estimation of δ 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. All dependent variables are dummies. "Satisfaction with the wage" is a dummy equal to 1 if the individual is satisfied with the benefits she receives, which include the wage (but might comprise other elements). 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 B3 — Differential linear trends in satisfaction with job characteristics between territorial and national entities before law 617

	Satisfaction with							
	(1) Contract	(2) Contract (Ind C)	(3) Job	(4) Job (Ind C)	(5) Wage	(6) Wage (Ind C)	(7) Workday	(8) Workday (Ind C)
Linear trend × Treated	-0.005*	-0.005*	-0.001	-0.001	0.001	0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Linear trend	-0.003***	-0.003***	-0.003***	-0.003***	-0.005***	-0.005***	-0.003***	-0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	32,728	32,726	32,822	32,820	32,822	32,820	32,822	32,820
No. of Clusters	644	644	645	645	645	645	645	645
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	.97	.97	.637	.637	.572	.572	.6	.6
S.D DV	.169	.169	.481	.481	.495	.495	.49	.49

Notes: This table presents a test for differential trends in the period before Law 617 was implemented (cohorts 1994 to 2000). The variable of interest is an interaction of a linear trend of the cohorts of entry to the public sector –i.e., a variable equal to 1 for 1994, and equal to 6 for 2000– and the treatment status, which takes the value of 1 if the person works under the ISIC Revision 3 "7514" code and out of Bogotá, as a proxy of being employed in the executive sector in territorial-order agencies. The variable equals 0 if she works in Bogotá or in other government sectors. All dependent variables are dummies. "Satisfaction with the wage" is a dummy equal to 1 if the individual is satisfied with the benefits she receives, which include the wage (but might comprise other elements). Individual controls (Ind C) include a sex dummy, the number of years of education, and the respondent's age. 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.