BUREAUCRATIC NEPOTISM*

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This paper provides the first systematic empirical examination of bureaucratic nepotism and anti-nepotism legislation using data from an entire modern bureaucracy. By linking confidential information on family ties and administrative employer–employee records for the universe of civil servants in Colombia, I uncover three sets of new empirical findings. First, using a novel methodology of family network reconstruction, I provide evidence of the pervasiveness of close family connections in public administration and demonstrate its negative relationship with the performance of public sector agencies. Second, by further exploiting within-bureaucrat variation in family connections generated by the turnover of top *unelected* bureaucrats, I demonstrate that family connections to managers and supervisors distort the allocation and compensation of public sector workers. Third, I evaluate an anti-nepotism legislation reform by exploiting a sharp discontinuity in the family connections restricted by this law. I prove its limited effectiveness and show how bureaucrats strategically responded to it.

Keywords: Favoritism, Nepotism, Bureaucracy, Public Sector Managers

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1 Introduction

Bureaucratic nepotism —the practice of favoring relatives when conferring jobs and promotions by *unelected* bureaucrats— is one of the most chronic and hard-to-identify pathologies within public administrations around the world (World Bank, 2020; Meyer-Sahling et al., 2018). It directly affects the allocation and compensation of public sector workers, which are both critical determinants of state capacity (Bandiera et al., 2021; Finan et al., 2017; Besley et al., 2022; Xu, 2018). Although most countries have implemented civil service reforms aimed to eradicate it (Mulcahy, 2015; Grindle, 2012), the perception of favoritism by government officials in these countries remains high, and complaints about this practice within public organizations are recurring. Identifying the actual magnitude of this phenomenon, why it has been so persistent, and what are its consequences in modern bureaucracies is fundamental for strengthening state capabilities worldwide.

As with many other forms of favoritism in the public sector, the ultimate impact of bureaucratic nepotism is theoretically ambiguous (Chandrasekhar et al., 2020; Bramoullé & Goyal, 2016; Bramoullé & Huremovic, 2018; Alger & Weibull, 2010; Prendergast & Topel, 1996). On the one hand, *unelected* top bureaucrats could use their discretionary power and family networks to reduce informational frictions and screen for more qualified and motivated government employees. On the other hand, nepotistic bureaucrats could substitute competent individuals with less capable family connections, which could decrease government effectiveness.[1]

Despite plenty of anecdotal accounts and qualitative evidence on the role of bureaucratic nepotism (Meyer-Sahling et al., 2018), systematic empirical evidence of its extent, operation, and effects is scarce. By contrast, several studies have investigated the role of political connections (Brugues et al., 2023; Colonnelli et al., 2020; Brassiolo et al., 2020; Iyer & Mani, 2012; Fisman, 2001), political dynasties (Dal-Bó et al., 2009; Querubin, 2016; George, 2020), and family connections to *politicians* (rather than bureaucrats) in determining private and public employment outcomes (Fafchamps & Labonne, 2017; Folke et al., 2017; Gagliarducci & Manacorda, 2020; Cruz et al., 2017). The study of nepotism in bureaucracies has proven challenging due to the lack of comprehensive data on family connections, performance, and career paths of bureaucrats *within* the public sector.

In this paper, I contribute to our understanding of bureaucratic nepotism by studying its extent, functioning, and consequences within an *entire* public administration. The empirical analysis focuses on Colombia and the role that

[1] Cross-country evidence suggests that favoritism in the public sector can result in ineffective governance. *See, e.g.*, Appendix Figure A-1, Evans & Rauch (1999) & Cornell et al. (2020).

public sector managers and supervisors had in allocating and compensating middle- and lower-tier workers from 2011 to 2017. Colombia provides an ideal laboratory to study this phenomenon because despite having a career civil service system since 1991 — where qualifications and seniority determine pay raises and promotions — public sector managers and advisors still retain a lot of discretion in determining public employment outcomes. In fact, as in many other Low and Middle-income Countries (LMICs), public sector managers in Colombia not only outnumber politicians in the public sector but also oversee more task assignments, promote and recommend more bureaucrats to leadership positions, and intervene directly in selecting temporary contractors (IDB, 2014).

Bureaucratic nepotism is extremely challenging to detect. Ideally, one needs to observe not only family connections between public sector workers but also their whole career progression within the public administration. The latter must specify exactly when hirings, promotions, and pay raises occur since the manifestation of this form of favoritism is inherently dependent on the timing of the events and the asymmetry of power between the individuals involved. For example, the mere presence of two family members in the same institution does not directly prove the existence of nepotistic practices. People may find romantic partners in the workplace or select into the same institutions for a variety of reasons. Additionally, to identify nepotism econometrically, one requires variation in family connections that is arguably exogenous to the evolution of employment outcomes. Finally, since one of the ultimate goals of studying nepotism from an economics perspective is to assess its potential distortive effects, one also needs to observe meaningful and comparable measures of performance to evaluate its implications for public sector outcomes and citizens' welfare.

To overcome these empirical challenges, I leverage fine-grained administrative data tracing the *universe* of civil servants in Colombia over 7 years. I collect and combine detailed biographical information from curricula vitae (CVs), employer–employee records, and the mandatory but confidential disclosure of family ties in the first-degree of consanguinity and affinity of every worker in the public administration.[2] I use this de-anonymized and annually updated data to reconstruct the full career paths of 1,083,714 public servants and their extended family networks, linking more than 2,400,000 individuals via predetermined consanguinity and affinity ties. I complement this information with agency-specific indices of institutional performance and information on the historical and contemporaneous

[2]These degrees correspond to reporting parents, children, and spouse. To guide the reader, Figure A-2 presents the mapping between degrees of consanguinity and family relationships.

presence of misconduct at the individual level, which includes disciplinary, criminal, and fiscal investigations and sanctions.

I use two sources of identifying variation in the empirical strategy. First, I leverage the timing of top bureaucrat turnovers to evaluate how changes in family connections to public sector managers and advisors impact the allocation and compensation of public sector workers. Second, I exploit a sharp discontinuity in Colombia's 2015 anti-nepotism legislation reform that prohibited public sector employees from appointing, promoting, nominating, or contracting relatives up to 4 degrees of consanguinity within public sector organizations. Both sources of variation allow me to study the ultimate impact of family connections to top bureaucrats before and after enforcing a comprehensive anti-nepotism law.

The empirical analysis proceeds in three steps. In the first part of the paper, I document four new empirical facts about family connections in the public administration that highlight: (1) the pervasiveness of close family connections within the public sector and (2) the likely presence of illegal (and misreported) family ties. More importantly, I use data on agency-specific indices of institutional performance to document a negative and robust correlation between the presence of close family connections and agency performance. I establish that a one-standard-deviation increase in the number of family connections below 4 degrees of consanguinity within agencies is robustly associated with a 0.24-standard-deviation decrease in overall performance.

In the second part of the paper, I quantify the nepotistic returns of family ties to top *unelected* bureaucrats. Using within-bureaucrat variation in family connections generated by the turnover of these influential bureaucrats, I demonstrate that, on average, a public sector worker is 40% more likely to be hierarchically promoted – compared to the sample mean – and receives a 2–5% increase in salary after becoming family connected to a top manager or advisor. I show that these returns materialize by benefiting connected workers within the same institution where top bureaucrats are working rather than by allocating family members to higher-paid agencies. Moreover, these effects are concentrated among family connections of 2 to 5 degrees of consanguinity (e.g., brothers, uncles, and cousins) rather than among parents, children, or spouses of top bureaucrats (connections that each institution's human resources department effectively audits).

I argue that these effects are most likely driven by allocating family members to better-paid contracts, temporary promotions to leadership positions, and temporarily filling vacancies that were in the process of being assigned via meritocratic examinations. Consistent with these mechanisms, I demonstrate that the prospects of connected bureaucrats are closely linked to the fate of their relatives who are top bureaucrats. Following the exit of managers and advisors, connected bureaucrats experience a significant reduction in total earnings and are significantly less likely to be promoted, which initially offsets the effects of having had a family connection to a top bureaucrat in the past.

Next, I examine the consequences of this favoritism on the type of workforce that is promoted. Building on Voth & Xu (2021) and Benson et al. (2019), I evaluate the decision process that top bureaucrats face every period when deciding whom to promote. I calculate the differences in bureaucrats' pre-promotion characteristics between promoted and passed-over workers and their relationship with family connectedness to top bureaucrats. I show that managers promote better-qualified individuals in general but also that they are likely to overlook these qualifications when promoting their family members. On average, promoted workers tend to have more education, more public sector experience, and fewer records of misconduct. However, those differences either disappear or completely reverse when promotees are family-connected to top bureaucrats. Therefore, this distortion is consistent with the pure extraction of private rents instead of better screening of workers via family networks. These results are based on the reconstruction of choice-sets of candidates, which allows me to restrict the comparisons only among workers within the same public sector agency, choice period, hierarchical position, and seniority level.

In the final part of the paper, I evaluate Colombia's anti-nepotism legislation reform of 2015 (Art 2. Act 02 of 2015). Using a difference-in-differences identification strategy and exploiting a sharp discontinuity in the set of family ties restricted by the reform, I examine the degree to which enforcing a more stringent anti-nepotism law can stop the spread of kin favoritism. Although the reform reduced the number of illegal connections by almost 15%, it did not improve the quality of the promoted workforce, the overall performance of public sector agencies, or prevent kin favoritism from happening. I argue that the reform was ultimately ineffective because top bureaucrats strategically responded to the policy change by changing the margin of influence from hierarchical promotions (covered by the law) to pay raises (not covered) and because middle and lower-tier bureaucrats simply reshuffled posts after the new policy came into effect.

Taken together, these findings provide the first systematic empirical examination of bureaucratic nepotism and anti-nepotism legislation in an entire modern bureaucracy. In doing so, this paper relates and contributes to multiple strands of the political economy and development economics literature.

This paper speaks to the literature on the personnel economics of the state and the importance of well-functioning bureaucracies for economic development (Finan et al., 2017; Besley et al., 2022). Prior work has explored the role of pecuniary and non-pecuniary incentives in the selection, allocation, and performance of public sector workers and how such incentives influence state capacity (Dal-Bó et al., 2013; Bertrand et al., 2019; Ashraf et al., 2014, 2020; Akhtari et al., 2021; Colonnelli et al., 2020; Xu, 2018; Bandiera et al., 2023; Deserrano et al., 2021; Bergeron et al., 2022). The paper contributes to this literature by providing systematic empirical evidence of bureaucratic nepotism —a newly defined concept in this paper— and how it can shape the allocation and compensation of public sector workers, the quality of the promoted workforce, and its relationship with public sector performance. Relative to the rest of the literature, the paper focuses on favoritism by bureaucrats who face different incentives, accountability mechanisms, and job security than politicians (Alesina & Tabellini, 2007, 2008; Spenkuch et al., 2023).

To the best of my knowledge, this is the first empirical study of nepotism exercised by *unelected* public sector managers that: (1) covers the *universe* of public sector workers within a country and (2) does *not* rely on any proxies of family connections. The family network reconstruction uses administrative data and national identification numbers to perfectly identify family linkages between workers at all levels of a modern bureaucracy. The literature on powerful connections has used nobility, elite associations, and other proxies of family ties such as shared last names, tax codes, birthplaces (home ties), or ethnicity to determine family connections (Jia et al., 2015; Brassiolo et al., 2021; Durante et al., 2011). Yet all of these proxies tend to overestimate individuals' actual relatedness or confound other dimensions of social connectedness with actual kinship. Moreover, some of these proxies over-sample from elitist networks that do not capture the extent of relatedness across different socioeconomic groups present in modern public sector organizations. My measure of connectedness via blood relationships is predetermined to public employment outcomes and allows me to distinguish between the intensive and extensive margins of family relatedness using well-defined consanguinity degrees of separation.

This paper also contributes to the debate of rules vs. discretion in the allocation of public sector talent (Moreira & Perez, 2022b, 2022a; Deserranno et al., 2022; Aman-Rana, 2022; Deserrano et al., 2021; Estrada, 2019; Jia et al., 2015) and documents the negative selection effect of nepotism exercised by public sector managers. Consequently, this paper also speaks to the recent and growing literature on the importance of managers and their practices within the public sector (Fenizia, 2022; Best et al., 2019; Rasul & Rogger, 2018). More specifically, it adds to this literature by showing *how* managers' family incentives could lead to severe distortions in the allocation of workers in the public administration, especially in developing countries where family ties are strong (Cox & Fafchamps, 2007; Alesina & Giuliano, 2014) and civil service systems are weak (Meyer-Sahling et al., 2018; IDB, 2014; Grindle, 2010).

This paper also contributes to the literature on the misallocation of jobs and corruption in the public sector (Olken & Pande, 2012; Weaver, 2021) by quantifying a hard-to-identify illegal behavior and exposing the difficulties to eradicate it via public sector reforms due to the strategic response of bureaucrats facing those changes. In this regard, this paper also relates to the Forensic Economics literature (Zitzewitz, 2012) by being the first to determine the effectiveness of anti-nepotism legislation based on *consanguinity degrees* widely adopted across countries influenced by the Canon law and Roman civil law.

Finally, this paper relates more broadly to the literature on social incentives within organizations (Ashraf & Bandiera, 2018; Bandiera et al., 2023) and to the labor economics literature on social networks (Eliason et al., 2022; Kramarz & Skans, 2014), job referrals (Burks et al., 2015; Schmutte, 2015), and kin favoritism (Gagliarducci & Manacorda, 2020; Pellegrino & Zingales, 2018) primarily concentrated in the study of these phenomena in private sector organizations (Bandiera et al., 2010, 2009, 2005), specific public sector agencies (Brassiolo et al., 2021; Durante et al., 2011), or among frontline workers (Bandiera et al., 2023). The paper contributes a novel empirical methodology of family network reconstruction that is exportable to other contexts and examines this ubiquitous and understudied form of favoritism across *all* levels of the public sector hierarchy.

The remainder of the paper is organized as follows. Section 2 briefly describes the Colombian institutional context. Section 3 presents the administrative data, including the reconstruction algorithm for bureaucrats' family networks and full career paths. Section 4 documents four data facts on family connections, while Section 5 estimates the returns of family ties to top bureaucrats and examines the qualifications of those receiving such returns. Section 6 evaluates Colombia's 2015 anti-nepotism law and bureaucrats' strategic responses. Section 7 explores the differences and relevance of bureaucratic nepotism compared to other forms of favoritism in the public sector, and Section 8 concludes.

2 Institutional background

Colombia's public sector employs roughly 1.2 million individuals. According to the Administrative Department of the Civil Service, bureaucrats, teachers, and frontline providers account for 70% of these jobs while the remaining 30% constitute active members of the police and military forces. In this paper, I focus on bureaucrats in all branches of the government, including *contractors* and *civil servants* at all hierarchical levels (managers, advisors, professionals, technicians, and clerical workers) and administrative units (national and local).

2.1 Job allocations

There are three ways to become a public servant in Colombia. The first path involves a merit-based system where individuals undergo civil service examinations. Upon passing, these workers are integrated into the official career system. The second avenue is through direct hiring for specific consultancy roles or positions. Unlike the first path, this does not require examinations but instead focuses on the expertise and educational qualifications of workers. However, contractors hired this way do not enjoy the same benefits as career system employees; they lack job stability, cannot unionize, and often have contracts lasting less than two years without renewal guarantees. The final pathway is through elections or direct appointments for high-influence roles, termed "*cargos de libre nombramiento y remocion*" (free appointment and dismissal). Managers and advisors who have direct influence over the process of hiring and promoting other bureaucrats hold these positions. The turnover of these top bureaucrats depends on lateral moves, the discretion of government officials, lateral moves, electoral cycles, and workers' mandatory and voluntary retirement.

2.2 Discretionary appointments and promotions

Two institutional features make Colombia an ideal case study of nepotism in the public sector. First, while most entry-level positions are allocated via exams and educational qualifications, by 2017 —the last year in my sample— less than 50% of *total* public sector jobs were obtained via meritocratic examinations. Abuse of direct hiring and parallel payrolls based on temporary positions and contracts was and remains widespread: most of the selection and promotion of bureaucrats occurs via discretionary appointments. Most public sector agencies have a larger proportion of contractors than workers in the official career system, and those on short-term contracts are often in charge of core public sector activities for years.

The second institutional feature is that the allocation of jobs through meritocratic processes is extremely slow and applies only to the *recruitment* of public servants – not to their promotion or compensation. Bureaucrats who seek a promotion can only apply to entry-level vacancies available in their institution, where they compete with workers inside and outside the organization in processes that take several months. Thus multiple positions, even those that were obtained meritocratically, must be temporally filled by provisional appointees (*encargados* or *provisionales*) selected directly by immediate superiors. Moreover, the ultimate decisions related to temporary leadership positions and coordination tasks – that usually come with temporary bonuses and leadership premia – are not regulated by any meritocratic process.

Therefore, it is difficult for civil servants to move up the career ladder without the favor of top bureaucrats (managers and advisors). Hierarchical promotions in the public sector are rare – representing fewer than 4% of the career transitions each year – and depend on either (1) a fixed pay grade scheme based on experience and education or (2) the direct influence of powerful connections.

2.3 Anti-nepotism laws and the constitutional amendment of 2015

As in many other countries, it is illegal to appoint relatives in the public sector in Colombia. The punishment is severe: firing both parties, fines, and imprisonment for 5 to 12 years. Article 126 of the original 1991 Constitution states that Civil servants may not appoint as employees, individuals to whom they are kin up to *the fourth degree of consanguinity.*

De jure, the auditing of these 4 degrees of consanguinity must occur during the hiring or promotion of any public sector worker. Each organization's human resources (HR) office and the office of internal oversight are in charge of this process. They approve and verify the mandatory reports of family connections filed as part of employees' annual conflict of interest reports and investigate any potential conflict directly identified by them or through any allegation made to the office. Every disciplinary investigation must be reported to the Attorney General's office. If it results in a disciplinary sanction, the Attorney General's office is in charge of investigating and sanctioning the bureaucrats involved.

De facto, however, the auditing is based only on the confidential disclosure of family members in the first degree of consanguinity and affinity. This feature has restricted the auditing scope to immediate family connections and limited the inspection of family ties to siblings, nephews, grandparents, uncles, cousins, and beyond to cases for which a formal complaint of corruption was filed with the internal oversight office.

Between 2013 and 2014 various scandals involving multiple members of the judiciary system and the Attorney General's office revealed loopholes in the 1991 anti-nepotism law and its enforcement. The subjective interpretation of the article left open the possibility of indirect hiring and promotions. Powerful bureaucrats could nominate their relatives to selection committees or suggest that other managers appoint them. They could also appoint relatives just before leaving office who could later re-appoint them through other indirect mechanisms. Moreover, the law was sometimes interpreted to apply only to employees in the official career system and not to temporary contractors.

As part of other constitutional reforms, and partially motivated by these scandals, Congress approved a constitutional amendment that modified the original constraints of the 1991 constitution. Legislative Act 02 of 2015 modified Article 126 of the original anti-nepotism law as follows:

> Civil servants may not, in the exercise of their functions, *nominate, propose or contract* people within their kinship *up to the fourth degree of consanguinity* [...] or with whom they are linked by marriage or permanent union. Furthermore, they will not be able *to nominate or propose as civil servants, nor celebrate state contracts with*, people that have intervened in their postulation or designation.

3 Data construction

Identifying nepotism in the public sector is extremely challenging. Ideally, one requires data on family connections between public sector workers and their career progressions in the public administration. This paper builds on a large-scale consolidation and digitization of multiple administrative datasets and a novel family network reconstruction methodology that overcomes these empirical challenges. This section describes these data sources and how I map bureaucrats' full career paths and extended family networks.

3.1 Panel data on public employment outcomes

I collect and combine employer–employee records and detailed biographical information from three administrative datasets to reconstruct bureaucrats' career paths over time, including any work in the private sector.

First, I analyze data from more than one million civil servants' CVs included in mandatory annual reports to the *Sistema de Informacion y Gestion del Empleo Publico* (SIGEP). The system includes data on the demographics, education level, work experience, and pay grades of all state workers except the military, police, and elected officials. The information uploaded onto the system is declared under oath and must pass a rigorous verification process from each organization's HR office before a worker is hired or can renew or sign a new contract (Appendix Figure A-4 contains a screenshot of the front-end of the system). Official documents such as diplomas and proof of experience remain in the system as PDF attachments that the Department of Civil Service can check at any time. I classified each of the 9,417,400 job spells listed on these CVs as either public or private sector. For the former, I further code the jobs' location, government agency, and hierarchical level. Since I have access to the non-anonymized version of this data, I also have information on all bureaucrats' full names, sex, date and place of birth, and national identification numbers (*cedulas de ciudadania*).

Second, I complement the job-spell data using information from all contractors hired by all public sector institutions. I use more than 6,345,000 contract records from the *Sistema Electronico para la Contratacion Publica* (SECOP), the public procurement information system established by the Colombian central purchasing body (*Colombia Compra Eficiente*), to digitize all transactions made by the country's public entities. I use institutions' unique IDs and contractors' national identification numbers to (1) verify and expand the job-spell data in the CVs for those who were contractors at some point during their careers and (2) fill in any data gaps for workers who reported being contractors in the public sector but did not specify enough details on their CVs to classify their employment.

Third, I incorporate information on total earnings from the *Planilla Integrada de Liquidacion de Aportes al sistema de seguridad social* (PILA), an employer–employee dataset containing detailed information on formal employment and total earnings.

The resulting dataset is a balanced half-yearly[3] panel dataset of 15,151,823 observations containing information on the full career paths of 1,083,714 individuals who worked as public servants from 2011 to 2017.[4] The sample is restricted to individuals aged 18–59 in 2011. This creates a balanced panel of N = 13,984,555 observations and n_b = 1,000,112 bureaucrats. I further divide these observations into two groups ($n_b = n_{top} + n_{ntop}$): those who are or become top bureaucrats (managers or advisors) at some point in their careers (n_{top} = 175,792) and those

[3]Since most hiring and promotions occur at the beginning of the fiscal year and most contracts are for 6 or 12 months, I defined the panel's time unit as a half-year.

[4]My analysis is limited to this period because of data access limitations. I had family connection data only until 2017, and web platform changes in 2017 made further data collection very challenging.

who do not (n_{ntop} = 824,320).[5]

Appendix Tables A-1 and A-2 present key descriptive statistics, while Appendix Figure A-3 plots the hierarchical composition of public sector jobs over time. Top bureaucrats (managers and advisors) represent 13% of the public labor force and are, on average, more educated than non-top bureaucrats. Nearly half (49%) hold a post-graduate degree, compared to the 18% of those who never become top bureaucrats. Bureaucrats enter the public sector at an average age of 29, and were 34 years old at the beginning of 2011. There is considerable variation in public and private sector experience and dispersion in wages. However, promotions and job separations are rare accounting for 3.3% and 3% of all transitions over time. Therefore, the hierarchical composition is very stable over time. Around 40% of public servants are contractors, and there has been a significant increase in the participation of professionals from 22% in 2011 to 33% in 2017.

3.2 Family network reconstruction

To uncover the hidden family networks within the public sector, I exploit confidential information on family connections in the first degree of consanguinity and affinity that all civil servants must report to the Administrative Department of the Public Service when they are hired. This report must be updated annually and include each family member's national identification number, full name, gender, and date of birth, regardless of their labor force participation or sector of employment. Crucially, the system allows the *addition* of family members, but not their elimination. Reporting a family member creates a permanent link between the bureaucrat and their relative that persists even after divorce or the death of family members.

I map the family network in two steps, summarized in Appendix Figure A-5. I first make an undirected network representation of each bureaucrat's family members using the annual reports of family ties: nodes identify individuals, and edges symbolize dyadic family links of 1 degree of consanguinity. Each connected component represents a family. I combine these clusters *within each year* based on the national identification numbers and the entire set of demographics from all reported and filing individuals. I use the demographic information to correct typos in the national identification numbers and merge nodes representing the same individual. I use multiple record deduplication algorithms for this process as well as the *Networkx* python package to create and combine the family networks. I recover or simplify 28,343 family linkages, which generates 1,068,750 family clusters

[5]While I restrict the sample to all non-top bureaucrats, there are still workers in middle- and lower-tier managerial positions that are not responsible for recruitment or appointments.

containing 2,464,868 individuals. I refer to this graph of connected components as the Official Data since it is what human resources could potentially observe each year using the reports.

In the second step, I combine these clusters *over time*. This key step allows me to identify connections that were not observable in any of the year-specific snapshots. This procedure tempers the concern that new bureaucrats strategically misreport family members who are (or were) part of the public administration and who therefore could generate a conflict of interest. In this step, I recover 796,349 family linkages. The resulting graph, which I call the Real Network, identifies 761,231 families (or connected components) containing a total of 2,446,904 individuals.

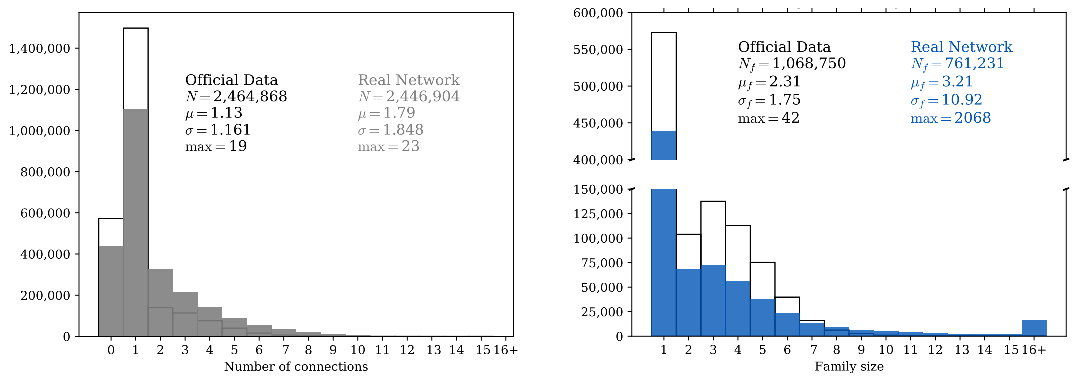
This final dataset of family connections has two important features. First, family network topologies are fixed once they are reconstructed. No nodes disappear or are added during the empirical analysis, and no connections are created or destroyed over time because blood connections are predetermined. Since connections involving spouses could be endogenous to public sector outcomes, in Appendix B I describe how this could potentially affect my results and how I address this concern. Second, nodes can have two mutually exclusive states in each period: they are either bureaucrats or non-bureaucrats. I can, therefore, determine the degree of consanguinity between any pair of bureaucrats within a family using Dijkstra's shortest path algorithm. More importantly, I can calculate the degree of separation (consanguinity) between nodes with different states or characteristics at any point in time.

Panel A of Figure 1 presents the distribution of connections per node before and after the second step of the reconstruction. On average, the matching algorithm adds one connection per node, and most of the recovered connections come from individuals who initially reported one or no connections. This critical step reveals many additional extended family connections. Panel B of Figure 1 displays the distribution of family sizes (number of nodes per connected component) before and after the second step. Even though I am adding, on average, just one additional family member per cluster, the distribution of family sizes shifts sharply to the right. Appendix Figure A-6 shows, for example, how the largest family network reconstructed based on the Official Data significantly differs in shape and size from the most extensive family in the Real Network.

Lower bound of the total number of family connections The proposed algorithm provides a lower bound on the total number of bureaucrat–bureaucrat

Figure 1: Distribution of family connections and family sizes before and after the second step of the family network reconstruction

Panel A — Connections per node *Panel B — Distribution of family sizes*



Notes: Panel A displays the distribution and summary statistics of the number of connections per node within families using the raw official data (hollow histogram) and after the reconstruction of the real family network (gray histogram). Panel B displays the distribution and summary statistics of the family sizes (number of members per family) using the raw official data (hollow histogram) and after the reconstruction of the family network (blue histogram).

connections. Since the data generation process uses bureaucrats as seeds of the network sampling of family trees, there are fewer links that I can fully recover even under truthful reporting. For example, I will not be able to determine the relationship between two cousins (4 degrees of consanguinity apart) if none of their parents – who are at 2 degrees of consanguinity from each other — ever worked in the public sector. However, if any of the grandparents of both cousins was ever a public servant, I can recover all of the intermediate connections (see Appendix Figure A-2 to understand why). Therefore the density of bureaucrats within the family, and how comprehensive their reporting of family ties is, determines how many family connections the algorithm ultimately detects.

Appendix A employs a simulation analysis to estimate how much of a known family network this method can recover under different (1) fractions of ever-bureaucrats in the family and (2) percentages of truthfulness in reporting. Rough calculations using these simulations and observed characteristics in my sample indicate that the algorithm recovers 14.65% to 27.22% of the bureaucrat-to-bureaucrat connections. Appendix C also discusses the potential measurement error introduced in the baseline specifications due to this restriction and its implications for my estimates following the classic result by Chandrasekhar & Lewis (2016).

3.3 Performance indicators

Any meaningful measure of performance in the public sector must be comparable across agencies, workers, and positions, and must be relevant to each institution's ultimate goal. I leverage three novel data sources to construct such an indicator for agencies and individuals.

First, I gather official information on records of individual misconduct by scraping the online version of the *Sistema de Informacion de Registro de Sanciones y Causas de Inhabilidad*, created by the Office of the Inspector General to record all prosecutions and investigations it carries out against public officials. This data includes disciplinary code violations, involvement in corruption, forced dismissals, suspensions, disciplinary warnings, fines, reprimands, arrests, and forced termination of employment contracts. I use this information to create time-varying indicators of any report of misconduct and active impediments. While these measures do not speak directly to bureaucrats' productivity, they capture an important dimension of the quality of the labor force – the integrity and eligibility of public servants to fulfill their public sector duties. While all public sector positions require a clean record before entering a new position, once an employee is hired, these records are rarely used to assess their eligibility for promotions or wage increases.

Second, I use information on agencies' performance from the publicly available *Medicion del desempeno Institucional* (MDI) database, which contains annual reports of achievement for more than 3,800 public sector agencies. This database, managed by the Administrative Department of the Public Service, uses questionnaires given to the most important authorities within each institution to rate the organization's ability to provide public goods and services based on multiple indicators of administrative capacity. I focus my analysis on the agency's overall performance index in 2016, scored between 0 and 100.

Finally, to complement and externally validate the previous index, I use independent information collected by Transparency International (TI) through the *Indice de Transparencia de las Entidades Publicas* for the 251 most representative public sector agencies in Colombia. I use reports from 2014 and 2016 that, like the MDI, rank agencies from 0 to 100 based on their transparency and institutional capability.

4 Stylized facts

I begin the analysis by documenting four empirical facts about the presence of family connections in the public administration.

Table 1: Family network reconstruction: Distribution of network edges

	Distribution of edges in the raw data	Distribution of network edges uncovered in... Step 1	Step 2	Distribution of edges in the Real Network
	(1)	(2)	(3)	(4)
Total edges	1,397,096	-	-	2,191,264
Total edges uncovered	-	28,343	796,349	-
Type of family connection				
Ever-bureaucrat to Ever-bureaucrat	3.25%	30.32%	26.48%	12.08%
Ever-bureaucrat to Relative never bureaucrat	96.75%	69.42%	73.52%	87.92%

Notes: The table displays the family connection distribution by link type before and after applying the family network reconstruction algorithm. "Ever-bureaucrat" denotes those who were bureaucrats at any point from 2011 to 2017. For a detailed breakdown of the reconstruction steps, see Section 3.2. Column 4's total excludes 30,524 duplicates corrected during the first two steps. Total linkages found: 824,692. Total linkages between Ever-bureaucrats: 219,478.

4.1 Fact I – Recovered linkages and potential misreporting

Table 1 summarizes the percentage of family edges recovered after each step of the reconstruction algorithm described in Section 3.2, as well as the distribution of family connections before and after this procedure. I find that about 26% (219,478 of 824,692) of the linkages identified are from connections between family members in the first degree of consanguinity and affinity who are, or become, public servants between 2011 and 2017. This means that more than a quarter of the recovered linkages at degree 1 in my sample occurred between children and parents or between couples who could have been part of an illegal connection and who potentially misreported family members. These linkages uncovered by my algorithm were not visible to individual agencies' human resources departments, which are required to check for potential conflicts of interest each year.

4.2 Fact II – Pervasiveness of family connections

How frequent are family connections within the public administration? Figure 2 displays the fraction of bureaucrats for which I can identify a family connection within the public administration. It indicates whether the connection is within the same institution or not and whether it includes a top bureaucrat (manager or advisor). Around 38% of bureaucrats have a relative in the public administration, and 18% have a family connection to a top bureaucrat. More importantly, about 11% of bureaucrats have a family connection in the institution where they work (2–3% of those links involve a top bureaucrat).

Although I am not aware of any study of the private or public sector that allows me to benchmark these results, according to de la Mata et al. (2022, p. 40), occupational persistence in Latin America and the Caribbean (the percentage of workers with the same occupation as their parents) is 30%, compared to 20–25% for Europe and North America. Corak & Piraino (2011) finds that only 6–8% of workers share occupations with their parents in the Canadian public sector, while Dal-Bó et al. (2009) documents that 7.3% of federal public sector workers in the US have a parent who has worked in the public administration.

4.3 Fact III – Small degree of consanguinity between bureaucrats

Once they occur, how close are family connections within the public administration? To answer this question, I calculate the average path length between public sector workers within each family, and its average over all the periods in which at least two family members were working in the public sector at the same time. In particular, for each family f, I compute:

$$C_f = \frac{1}{T}\sum_{t=1}^{T}\frac{1}{N_t(N_t-1)}\sum_{i\neq j} d(i,j)\cdot \mathbb{1}(i \text{ and } j \text{ are bureaucrats at } t), \tag{1}$$

where $d(i,j)$ is the shortest path length (in degrees of consanguinity) between individuals i and j, N_t is the number of individuals who are bureaucrats in family f at time t, and T denotes the number of periods in which there are at least two bureaucrats in the family.

I find that the average consanguinity degree between bureaucrats is small, about 2.61, and that the distribution of these average path lengths is mostly concentrated below 4 degrees. This implies that when family connections occur over time, they tend to be between very close family members. Appendix Figure A-8 presents the distribution of the average degree of consanguinity between bureaucrats across all families in my sample.

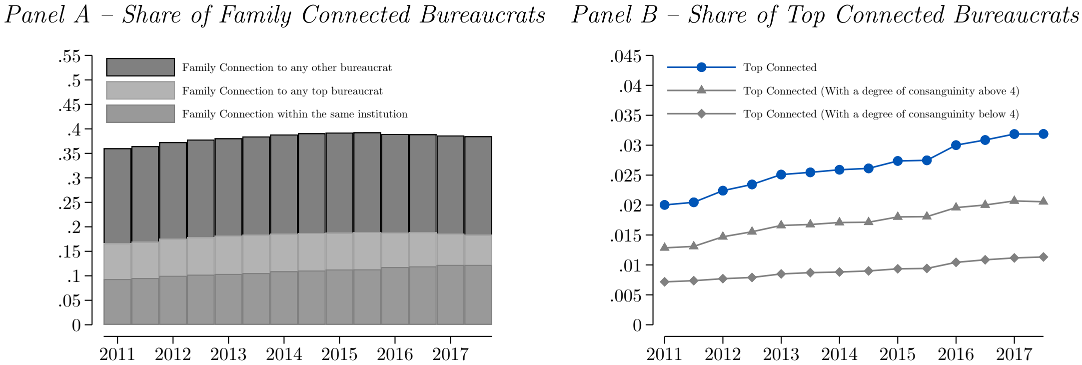
4.4 Fact IV – The potential costs of family connections

Table 2 Panel A reports the beta coefficients of the partial correlation between the overall performance index of agencies in 2016 and the number of family connections up to 4 degrees of consanguinity according to the following econometric specification:

$$\text{Index}_k = \rho_0 \cdot \left(\frac{\text{CloseTies}_k}{\text{Employees}_k}\right) + \rho_1 \cdot \text{Employees}_k + \rho_2 \cdot \text{CloseTies}_k + \gamma_{n(k)} + \epsilon_k, \tag{2}$$

where k indexes agencies, Employees_k is the total number of individuals working at institution k per 1,000 employees, and CloseTies_k is the total number of family connections up to 4 degrees of consanguinity. $\gamma_{n(k)}$ is a full set of fixed effects depending on the level of an agency's aggregation $n(k)$.

Figure 2: Shares of family-connected bureaucrats



Notes: Panel A presents the share of bureaucrats with family connections to any other bureaucrat, to a top bureaucrat (i.e., manager or advisor), and to any other bureaucrat within the same institution. Panel B presents the share of Top Connected bureaucrats, i.e., the share of bureaucrats with a family connection to a manager or advisor within the same agency they work in. It differentiates the share depending on whether the connections are above or below four degrees of consanguinity.

Regardless of the degree of centralization by function (centralized, decentralized, mixed), administration level (national, regional, local), government branch (executive, legislative, judiciary, oversight and control, autonomous), or legal nature of the agency (ministry, administrative department, assembly, *alcaldia*, *personeria*, *gobernacion*, public service firm, control agency, public sector company), I find that larger shares of close family connections are associated with lower levels of institutional performance.

Since government reports may overstate their own progress, I estimate identical specifications in Panel B using TI's independent assessment of the overall performance of most representative agencies in the public sector in 2014 and 2016. Even though the sample of institutions covered by TI is significantly smaller and the coefficients of interest are – as expected – much larger, the qualitative results hold both across panels and specifications. The last column of Panel B shows that a one-standard-deviation increase in the number of close family connections is robustly associated with a 0.24-standard-deviation decrease in the performance index, even after controlling for a full set of time fixed effects and all levels of aggregation.

Table 2: Agency performance and the presence of close family connections

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent variable is agency performance index based on government data						
Share of connections below	−0.0728	−0.0678	−0.0225	−0.0838	−0.0469	−0.0356
4 degrees of consanguinity	(0.0120)	(0.0122)	(0.0122)	(0.0128)	(0.0124)	(0.0124)
Observations	3,853	3,853	3,853	3,853	3,853	3,853
R-squared	0.2183	0.2206	0.2765	0.2677	0.3552	0.3747
Panel B: Dependent variable is agency performance index based on Transparency International data						
Share of connections below	−0.2902	−0.3017	−0.2257	−0.3181	−0.2384	−0.2377
4 degrees of consanguinity	(0.0564)	(0.0579)	(0.0612)	(0.0556)	(0.0803)	(0.0834)
- Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	419	419	419	419	419	419
Agencies	251	251	251	251	251	251
R-squared	0.1733	0.1802	0.1879	0.2153	0.2755	0.2994
Fixed effects applying to all panels						
- Degree of centralization	-	Yes	-	-	-	Yes
- Administrative level	-	-	Yes	-	-	Yes
- Government branch	-	-	-	Yes	-	Yes
- Legal nature	-	-	-	-	Yes	Yes

Notes: Panel A uses agency-level data from the MDI database in 2016. Panel B uses agency-year data from the Transparency International ITEP database for 2014 and 2016. Share of connections measured per 1,000 employees. Columns control for total number of employees and total number of family connections. The table displays standardized coefficients with robust standard errors in parenthesis.

Highlights of the empirical facts Taken together, these four stylized facts demonstrate the pervasiveness of family connections within Colombia's public sector and the likely presence of illegal – and strategically misreported – connections according to the recent anti-nepotism legislation. The strong negative relationship documented between the presence of close family connections and the performance of public sector institutions provides a first approximation of the potential costs of nepotism and further motivation for the analysis that follows.

5 Estimating the returns to bureaucratic nepotism

In this section I estimate the average nepotistic return of family connections to top *unelected* bureaucrats in terms of total earnings and promotion probabilities. I investigate whether middle- and lower-tier bureaucrats who *become* family connected to public sector managers or advisors receive a career premium. This is a key check for analyzing nepotistic behavior.

5.1 Empirical strategy

The identification strategy in this subsection exploits quasi-experimental variation in family connections generated by the turnover of top bureaucrats across public sector agencies. For bureaucrat i in family f and time t, I estimate:

$$E_{i,t} = \theta_i + \delta_t + \eta \cdot \text{TopConnected}_{f(i),t} + \boldsymbol{X'_{i,t}\Phi} + \xi_{i,t} \tag{3}$$

where $E_{i,t}$ represents public employment outcomes such as total earnings or hierarchical promotion, and $\text{TopConnected}_{f(i),t}$ is a dummy variable that equals 1 if worker i from family f has a family connection to a top manager or advisor at time t. By including bureaucrat fixed effects θ_i, I only exploit within-bureaucrat variation in family connections triggered by the turnover of top bureaucrats. These effects also allow me to control for any unobserved individual characteristics related to family connectedness, such as inherited or innate ability, family background, initial public service motivation, occupation, and any other individual time-invariant preference that could directly affect public employment outcomes.The identification of my parameter of interest, η, then comes from bureaucrats who experienced changes in family connections to top bureaucrats during their careers.

Since most salaries and promotions in the public sector depend deterministically on years of experience and education levels, I control for $\boldsymbol{X_{i,t}}$, a vector of individual time-varying controls including the public and private sector experience of worker i since her entry into the labor force, which are allowed to flexibly evolve over time according to the level of education. Since managerial turnovers occur across multiple agencies and top bureaucrats are more likely to influence outcomes in the agency they work in, the preferred specification is:

$$E_{i,t} = \theta_i + \delta_t + \gamma_{k(i,t)} + \eta \cdot \text{TopConnected}_{f(i),k(i,t),t} + \boldsymbol{X'_{i,t}\Phi} + \xi_{i,t}, \tag{4}$$

where $\gamma_{k(i,t)}$ represents a complete set of agency fixed effects, controlling for all time-invariant characteristics that affect both connectedness and labor market outcomes. These include agencies' organizational structure, the geographic location of institutions, and agency-specific pay grades or compensation schemes. By including time fixed effects δ_t, I address the concern that unobserved and aggregate common shocks such as general elections, national reforms, or macroeconomic policies can explain the relationship between public employment outcomes and family connections to top bureaucrats. Lastly, $\xi_{i,t}$ represents the error term, which I cluster at the bureaucrat-agency level or the dyadic family-agency level corresponding to the effective sources of identifying variation. To simplify the notation in subsequent sections, I define $\text{TopConnected}_{f(i),k(i,t),t} \equiv B^{top}_{f,k,t}$.

5.1.1 Main identification assumptions and key threats to identification

To identify my parameter of interest, I do not need to assume that top bureaucrat turnovers occur at random. To consistently estimate η for each outcome of interest, the econometric specification in Equation 4 requires that across agencies and $\forall t \geq 2$,

$$(5) \quad \mathbb{E}[E_{i,t}(0) - E_{i,t-1}(0)|\boldsymbol{X_{it}}, B^{top}_{f,k,t} = 1] = \mathbb{E}[E_{i,t}(0) - E_{i,t-1}(0)|\boldsymbol{X_{it}}, B^{top}_{f,k,t} = 0].$$

In other words, it requires that labor market outcomes would have exhibited parallel trends in the absence of those connections. This condition ultimately requires that there are no additional unobserved time-varying, individual-specific characteristics correlated with family connectedness that could have explained individuals' changes in labor market outcomes over time.

Since consanguinity relationships pre-determine family connectedness, and the turnover of managers and advisors generates cross-sectional variation in family connections to *all bureaucrats* within each agency, it is unlikely that an unobserved, individual-specific factor could violate this condition without affecting other bureaucrats within the agency. Nevertheless, I can validate the plausibility of the assumption by estimating, for each outcome of interest, the following more demanding and fully dynamic event-study specification:

$$(6) \; E_{i,t} = \theta_i + \delta_t + \gamma_k + \eta_{-5} \sum_{\ell \leq -5} B^{top}_{f,k,\ell} + \sum_{\ell=-4, \ell \neq 2}^{4} \eta_\ell \cdot B^{top}_{f,k,\ell} + \eta_5 \sum_{\ell \geq 5} B^{top}_{f,k,\ell} + \boldsymbol{X'_{i,t}} \boldsymbol{\Phi} + \xi_{i,t},$$

where η_ℓ captures the effect of a family connection to a top bureaucrat ℓ periods before or after a managerial turnover creates a change in family connectedness. This specification allows me to directly test for the presence of pre-trends in labor market outcomes and examine the dynamic effects of obtaining a family connection to a top bureaucrat.

Two key additional assumptions are implicit in the empirical models described above. The first is that the treatment effects of family connections to top bureaucrats are homogeneous across individuals and agencies, and the second is that the effects of gaining and losing a connection are almost symmetrical over time. Recent work in the applied microeconomics and econometrics literatures (Goodman-Bacon, 2021; Sun & Abraham, 2021; de Chaisemartin & D'Haultfœuille,

2020) has demonstrated how violating these implicit assumptions may lead to highly biased estimates and misleading tests for the parallel-trends assumption. Specifically, they could incorrectly test the absence of pre-trends in event studies since the contamination caused by the treatment heterogeneity can lead to estimates that are non-zero in the absence of pre-trends, or zero in the presence of pre-trends. In the presence of treatment heterogeneity, ordinary least squares estimators of Equations 4 and 6 – *even after partialling out agency fixed effects* – could lead to non-significant or even negative average treatment effects, when all individual-specific effects are positive and significant. The key reason behind this identification issue is that under treatment heterogeneity and non-staggered treatment adoption, two-way fixed effects (TWFE) regressions end up using already-treated units or switchers as controls. These comparisons create a mechanical negative weights problem when the final average treatment effect is calculated that in turn produces biased estimates of the true coefficients of interest.

I use two strategies to address these important identification concerns. First, I estimate Equation 4 focusing on the treatment of ever having a connection to a top bureaucrat – *by construction a staggered treatment* – and include as pure controls individuals who have never been family connected to a top bureaucrat over time. This approach tempers the concern that using TWFE would end up using the set of switchers as controls, but also captures the idea that the first connection to a top bureaucrat could structurally change civil servants' long-term career prospects. Using the same sample, I employ the estimates of Equation 6 to test the parallel-trends assumption under this setup and its corrected event-study versions based on the Sun & Abraham (2021) estimator.

Second, I account for the non-staggered nature of family connectedness and embrace the potential asymmetry between gaining and losing a top bureaucrat connection. I follow de Chaisemartin & D'Haultfœuille (2020, 2022) and report their proposed and corrected difference-in-differences (DID) estimators that account for both treatment heterogeneity and treatment reversals. These exercises allow me to consistently estimate the parameter of interest and test whether connected bureaucrats' career prospects are closely linked to the fate of their relatives as top bureaucrats (i.e., test whether workers who lose connections stop receiving those nepotistic premia).

5.2 Empirical results

5.2.1 Total earnings

Columns 1–3 in Table 3 report how having a family connection to a top bureaucrat affects the log of total earnings. Column 1 shows that individuals who end up having a family connection to these bureaucrats receive, on average, a positive and significant wage premium of 3.74%. This increase in wages is not explained by individual-specific characteristics or by common shocks affecting all public sector workers. In Column 2, I present the augmented specification that controls for time-varying private and public sector experience based on each worker's highest level of education. I find that even after controlling for these supposedly unique determinants of earnings in the public sector, a family connection to a top bureaucrat is associated with an average salary premium of 3.03%.

Table 3: Returns to family ties to top unelected bureaucrats

Dependent variable:	Total Earnings			Hierarchical Promotion		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean dep. variable	9.22	9.22	9.22	0.033	0.033	0.033
Top Connected	0.03740	0.03032	0.03047	0.01468	0.01437	0.01345
	(0.00579)	(0.00576)	(0.00565)	(0.00105)	(0.00105)	(0.00105)
Time-varying controls						
- Private experience		Yes	Yes		Yes	Yes
- Public experience		Yes	Yes		Yes	Yes
Fixed effects						
- Bureaucrat FE	Yes	Yes	Yes	Yes	Yes	Yes
- Time FE	Yes	Yes	Yes	Yes	Yes	Yes
- Agency FE			Yes			Yes
Observations	6,390,201	6,390,201	6,390,117	6,390,201	6,390,201	6,390,117
Bureaucrats	722,375	722,375	722,366	722,375	722,375	722,366
R-squared	0.73122	0.73208	0.74049	0.10877	0.10887	0.11358

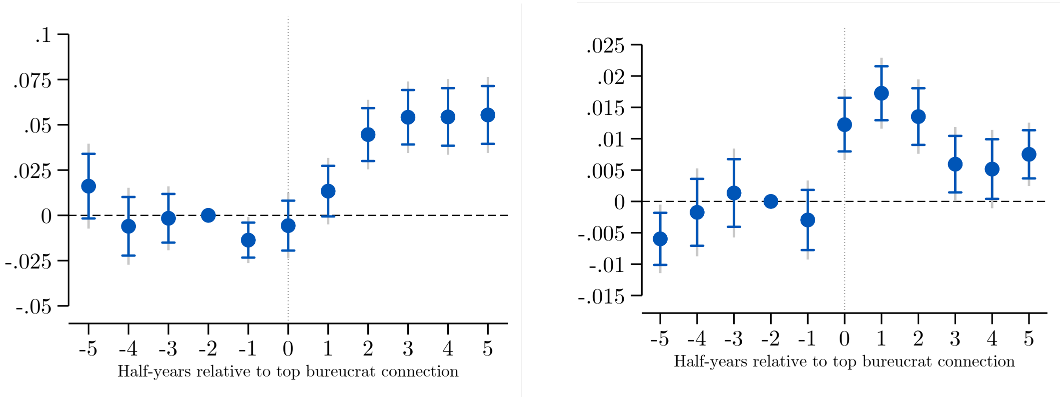
Notes: The unit of observation is bureaucrat-time. Top connected equals one if the bureaucrat has had a family connection to a manager or advisor within the governmental agency he/she is working in. Total earnings refers to the inverse hyperbolic sine of the wage in thousand Colombian pesos. Private and public experience varying by level of education l are included as follows $\sum_{l \in E}$ experience $\times$ $\mathbb{1}$(education= l). Standard errors clustered at the dyadic family-agency level in parentheses.

In Column 3, I explore whether the observed increase in earnings occurs by allocating family members across higher-paid agencies or by increasing wages within institutions. I compare the results in Column 2 with a more demanding specification that includes a comprehensive set of agency fixed effects. Since the coefficients of interest do not vary significantly across these columns and wages are deterministically settled via pay grades within each institution, the mechanism that seems to support this salary premium is likely the allocation of temporary leadership positions and provisional appointments to family members *within* rather than *across* public sector agencies.

Panel A of Figure 3 presents the corresponding event-study to these comparisons according to Equation 6. It is based on 34,887 first-time connections to top bureaucrats. The figure establishes that there is no evidence of pre-trends before the connection event, which validates the primary identification assumption since, on average, total earnings exhibited parallel trends before the top bureaucrat connection. It also indicates that the treatment effects are heterogeneous over time and, if anything, somewhat larger than the average treatment effect as time goes by. Top connected bureaucrats do not start experiencing a positive wage premium until 6 months after their relative becomes a top bureaucrat; 1.5 years after they experienced this connection, they earn a salary premium that steadily increases to 5.5%. These results hold and are qualitatively similar to those using the alternative Sun & Abraham (2021) estimator that I report in Appendix Figure A-9.

Figure 3: Effects of having a family tie to a top unelected bureaucrat

Panel A - Effects on Total Earnings (in logs) *Panel B - Effects on Hierarchical Promotions*



Notes: Figure displays the coefficients and 99% and 95% confidence intervals from the event-study of ever getting a family connection to a top bureaucrat (i.e., to a public sector manager or advisor) when looking at total earnings and hierarchical promotions as outcomes. These coefficients correspond to η parameters in the econometric specification in Equqtion 6. Standard errors are clustered at the dyadic family-agency level. The reference period is the year before the first family connection to a top bureaucrat (-2 half-years in the graph). Each figure is based on 6,390,117 panel observations coming from 722,366 bureaucrats and 34,887 connection events to top bureaucrats.

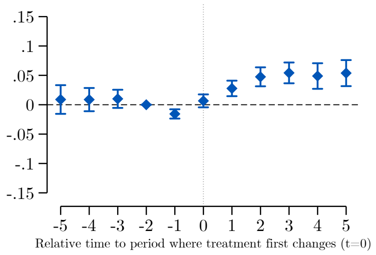
The interpretation of these magnitudes is subject to objections since they focus on a specific form of staggered treatment – the event of ever being connected to a top bureaucrat. While these results imply that initial connections have a persistent effect on the career trajectories of the affected bureaucrats, they disregard the possibility that such impacts can be heterogeneous across individuals and agencies. Nor do they account for the fact that some workers have more than one connection during their careers or lose connections that are not necessarily symmetrical in their impacts. More importantly, they also reject the possibility that never-connected individuals are inadequate controls since they may differ in many other dimensions from those who have at least one top bureaucrat connection during their careers.

To account for all of these additional identification issues, Panel A of Figure 4 presents the corrected DID_M estimator proposed by de Chaisemartin & D'Haultfœuille (2020), which uses a properly computed average treatment effect generated by all pairs of 'clean DIDs' in the sample.

Figure 4: Effects of having, winning, or losing a family connection to a top unelected bureaucrat, corrected by treatment heterongeneity

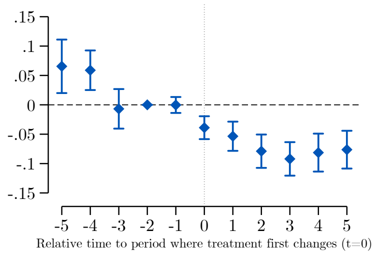
Panel A – Diff-in-Diff estimates of having a family connection on total earnings
$\text{DID}_M = 0.0233\ (0.0074)$ based on *95,758 switcher events*

Panel B — Winning a connection
DID_ℓ estimates based on *67,222 switcher events.*



Average dynamic effect $= 0.0597(0.0095)$

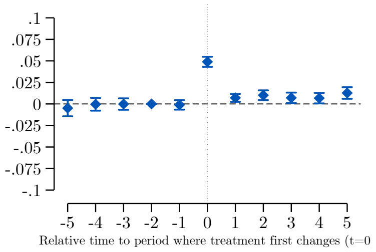
Panel C — Losing a connection
DID_ℓ estimates based on *28,536 switcher events.*



Average dynamic effect $= -0.0858\ (0.0144)$

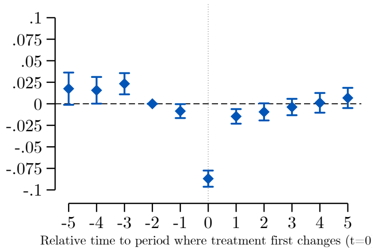
Panel D – Diff-in-Diff estimates of having a family connection on hierarchical promotions
$\text{DID}_M = 0.0121\ (0.0023)$ based on *95,758 switcher events*

Panel E — Winning a connection
DID_ℓ estimates based on *67,222 switcher events.*



Average dynamic effect $= 0.0258(0.0028)$

Panel F — Losing a connection
DID_ℓ estimates based on *28,536 switcher events.*



Average dynamic effect $= -0.0274(0.0047)$

Notes: Figure displays the coefficients from the event study of getting a family connection to a top bureaucrat (i.e., to a top manager or advisor) when looking at the log of total earnings as the outcome. These coefficients correspond to the ones proposed by de Chaisemartin & D'Haultfœuille (2020, 2022)

The DID_M estimator is based on 95,758 switcher events (i.e, when bureaucrats switch from unconnected to connected to a top bureaucrat or vice versa). I find that even after controlling for potential heterogeneity across individuals and allowing for treatment reversals, family-connected bureaucrats still receive a positive wage premium of 2.33% in total earnings. Despite its consistent sign and significance, this estimate could still be biased if the ultimate effects across individuals are also heterogeneous over time. Since the results using only the first connections in Figure 3 point in that direction, in Panels B and C of Table 4 I separately present the dynamic DID_ℓ effects of winning and losing a connection based on the de Chaisemartin & D'Haultfœuille (2022) dynamic estimator.

The results of this exercise indicate that after dividing the 95,758 switcher events into winning and losing connection events, the ultimate impact of obtaining a family connection to a public sector manager or advisor is a salary premium of 5.9%. Crucially, Panel B of Figure 4 also shows that the prospects of these connected bureaucrats are closely linked to the fate of their top bureaucrat relatives: if managers and advisors leave, previously connected bureaucrats experience a significant reduction in total earnings that exceeds the benefits of obtaining the connection. Given that these estimators are conditional on remaining in the public sector, these effects are simply lower bounds of the actual impact of losing a connection since I do not account for any potential exits generated by losing a connection to a top bureaucrat. I interpret these last results with caution since there is some evidence of negative pre-trends in earnings that could suggest the top bureaucrats' exit was anticipated or that their influence declined just before they left.

5.2.2 Hierarchical promotions

Columns 4–6 in Table 3 establish how family connections to top bureaucrats shape an individual's likelihood of being promoted. The promotion indicator used as an outcome includes all transitions moving up the ladder of the public administration and a shift from being a contractor to a permanent position within the official hierarchy. Not all of these transitions represent the extensive margin of the total earnings increases explained above. Salaries can increase without changes in the hierarchical levels driven by leadership premia, bonuses, or extra hours, just as rank promotions do not always directly increase earnings. For example, moving from a contractor to a staff position as a provisional worker is considered a rank promotion. Even though interim workers enjoy most of the non-pecuniary benefits of an official career position, they do not necessarily receive a higher wage than they earned as contractors.

Column 4 indicates that individuals who have ever had a family connection to a top bureaucrat are 1.4% more likely to be promoted. This effect is sizable since hierarchical promotions are rare. Compared to an overall 3.3% mean in the occurrence of rank promotions, having a family connection to a manager or advisor implies a nearly 40% increase in the likelihood of being promoted. This result is not explained by common shocks that affect all individuals, individual-specific characteristics, or by differential public or private experience profiles (Column 5). After controlling for all agency-specific characteristics (Column 6), it is clear that most of the returns on this margin come from promotions within the institution where the top bureaucrats work. Yet since the coefficients vary slightly from those in Column 5, I cannot reject the hypothesis that some of those hierarchical promotions also occurred across agencies.

Panel B of Figure 3 presents the analogous event study according to the econometric specification in Equation 6. It contains no evidence of problematic pre-trends before the connection event, thus validating the primary identification assumption for this outcome variable. Second, the effect on the probability of becoming connected kicks in immediately when the family connection occurs, increasing up to 1.75%; however, in contrast to the impact on earnings, this effect decreases 1.5 years after the event takes place. These results hold and are qualitatively similar to those using the alternative Sun & Abraham (2021) estimator that I report in Appendix Figure A-9.

Following the same arguments detailed above for total earnings, in Panel A of Figure 4, I present the alternative DID_M estimate. The average treatment effect is slightly smaller – 1.21% compared to 1.34% in Column 6 of Table 3 (i.e., about 35% with respect to the mean of promotions). These results imply that the simple specification in Column 6 of Table 3 is not affected as much by subsequent treatment reversals or individual treatment heterogeneity. Nevertheless, to account for potential heterogeneity in the effects of connectedness over time, Panels B and C of Figure 4 display the dynamic DID_ℓ estimates separately for winning and losing a top bureaucrat connection. These estimates indicate an overall symmetrical effect between winning and losing a family connection on the probability of being promoted. Yet, since immediate effects are asymmetrical at $t = 0$ and there are more winning than losing events, the ultimate net result is consistent with the DID_M estimate of 1.21% found above.

5.3 Key robustness tests

In Appendix B, I present a set of robustness tests addressing the potential confounding factors of common shocks at the family-time and agency-time level, including the role of political patronage. I address also the potential influence of middle managers, and the influence of the likely endogenous connections through spouses.

5.4 Fundamental sources of heterogeneity

The importance of the degree of consanguinity Which family members benefit the most from their connections to top bureaucrats? Appendix Figures A-11 and A-11 present the baseline results by the degree of consanguinity between bureaucrats and top managers and advisors. Each sub-figure comes from an independent regression model following the econometric specification in Equation 6. However, I redefine $B^{top}_{f(i),k,\ell} \equiv B^{top,s}_{f(i),k,\ell}$ as a dummy equal to 1 if worker i has had a family tie to a top bureaucrat at the degree of consanguinity s at institution k in relative period ℓ. I document the results for all degrees of separation from 1 to 6 and report the fully dynamic event-study set of coefficients.

There are two main takeaways from these figures. First, the effects on hierarchical promotions and earnings do not operate through close family connections such as parents, children, or spouses (which HR audits), or through distant family connections of more than 6 degrees of separation. Second, the effects on earnings are concentrated in connections of 2 to 4 degrees of consanguinity, while the returns on hierarchical promotions are concentrated in connections of 3 to 5 degrees (see Appendix Figure A-2). These results indicate that most of the estimated returns to family connections come from clear violations of the anti-nepotism law. More importantly, these private returns operate through relationships that institutions' HR departments do not typically audit.

5.5 Better screening or pure favoritism?

Although most of the returns estimated in Section 5.2 are already illegal under Colombia's anti-nepotism legislation, it is unclear whether those returns are still consistent with better screening of workers. It could be the case, for example, that those higher earnings and probability of being promoted simply reflect compensation differentials in terms of the bureaucrats' relative (prior or expected) performance, which top bureaucrats might better identify if promotees are family members.

However, estimating whether managers and advisors screen and select better workers using family connections is empirically challenging. For example, studying the *pre-promotion* characteristics that managers assess to make their promotion decisions requires (1) observing the criteria used to evaluate all workers eligible for promotion and (2) determining the pool of candidates. Similarly, to examine the selection in *post-promotion* performance, it would be necessary to observe the counterfactual accomplishments of those who were not promoted but were part of the pool of potential candidates.

To overcome the first challenge, I build on recent works by Benson et al.

Table 4: **Differences in pre-promotion characteristics**

	Pre-promotion characteristic			
	Misperformance		Qualifications	
Dependent Variable:	Had a Disciplinary Record	Had an Active Inability Record	Exceeded Education Requirement	Public Experience Ratio
	(1)	(2)	(3)	(4)
Mean dependent variable	0.297	0.112	0.337	0.721
Panel A: Hierarchical Promotions				
All columns include fixed effects at the Seniority×Position×Agency×Period level				
H. Promoted	−0.06364	−0.05112	0.00458	0.04869
	(0.00890)	(0.00474)	(0.00068)	(0.00098)
TopConnected	−0.08890	−0.03226	0.01662	0.02392
	(0.03146)	(0.01810)	(0.00207)	(0.00184)
H. Promoted × TopConnected	0.08873	0.05180	−0.01073	−0.00587
	(0.04120)	(0.02474)	(0.00302)	(0.00356)
Pools of candidates	194,426	194,426	194,426	188,620
Observations	4,906,044	4,906,044	4,906,044	4,818,860
R-squared	0.04264	0.02617	0.68061	0.29577
Panel B: Promotions in Total Earnings				
All columns include fixed effects at the Seniority×Position×Agency×Period×WageBin level				
E. Promoted	−0.10577	−0.05551	0.00626	0.05650
	(0.01289)	(0.00811)	(0.00055)	(0.00060)
TopConnected	−0.08570	−0.02534	0.01470	0.02414
	(0.03596)	(0.01990)	(0.00205)	(0.00187)
E. Promoted × TopConnected	0.06343	0.00671	−0.00273	−0.01230
	(0.03640)	(0.02219)	(0.00175)	(0.00191)
Pools of candidates	345,402	345,402	345,402	339,596
Observations	4,668,473	4,668,473	4,668,473	4,581,289
R-squared	0.09548	0.07221	0.70733	0.35931

Notes: The unit of observation is bureaucrat-choice period. See, details in Section 5.5.1.

(2019) and Voth & Xu (2021) to estimate the difference in pre-promotion characteristics and quantify the selection effects in promotions. Since post-promotion misconduct (i.e., the likelihood of observing disciplinary records after promotions occur) could be endogenous to the existence of promotions and connections to managers, I focus only on pre-promotion differences. I describe this approach below.

5.5.1 Differences in pre-promotion characteristics

I first evaluate the decision process that top bureaucrats use to evaluate pre-promotion qualifications. I start by approximating the candidate pool of workers that public sector managers observe every period:

i) For each time and agency, I restrict the panel of workers to all unpromoted bureaucrats, some of whom are about to be promoted.

ii) I further restrict the panel of workers to agencies and choice periods in which at least one promotion was made from 2011 to 2017.

iii) After a promotion occurs, I exclude promoted workers from the candidate pool for subsequent periods to ensure they are only used to compute differences within the choice period in which they were promoted.

Using this new dataset, I evaluate the decisions made by managers and advisors by calculating the differences in bureaucrats' pre-promotion characteristics ($Q_{i,t}^{pre}$) between promoted and passed-over bureaucrats and their relationship to family connectedness. For bureaucrat i and choice-period t, I estimate:

$$Q_{i,t}^{pre} = \lambda_{l \times h \times k \times t} + \mu_1 \cdot P_{i,t} + \mu_2 \cdot B_{f(i),k,t}^{top} + \mu_3 \cdot \left[P_{i,t} \cdot B_{f(i),k,t}^{top} \right] + \epsilon_{i,t}, \tag{7}$$

where $P_{i,t}$ is an indicator of a promotion (hierarchical or via earnings) and $B_{f(i),k,t}^{top}$ is an indicator of a family connection between bureaucrat i and a top bureaucrat at agency k in choice period t. The characteristic $Q_{i,t}^{pre}$ is predetermined and measured in pre-choice period $t-1$. Crucially, the full set of fixed effects $\lambda_{l \times h \times k \times t}$ restricts comparisons to groups of workers within the same choice period t, agency k, hierarchical position h, and seniority level l. To account for serial correlation in outcomes and for the fact that not-yet promoted bureaucrats are observed in multiple choice periods, standard errors $\epsilon_{i,t}$ are clustered at the bureaucrat level.

The parameters of interest in Equation 7 are μ_1 and μ_3. The first estimates how fair or meritocratic the promotion of bureaucrats is relative to the characteristics of passed-over workers at the time they were promoted. When $Q_{i,t}^{pre}$ represents a desirable qualification, a positive and significant μ_1 would capture how promotees positively differ from other candidates in the choice set – and therefore, the extent to which their promotion was based on merit. μ_3 estimates whether such effect diverges or is amplified when workers are family connected to a top bureaucrat when they were promoted; it therefore captures the selection effect of connected promotions in pre-promotion characteristics.

Table 4 presents the results for both types of promotions – hierarchical promotions in Panel A and pay raises in Panel B. I focus on four pre-promotion characteristics. Column 1 reports the estimated coefficients from Equation 7 based on whether bureaucrats had any record of misconduct as an outcome. This indicator variable equals 1 if the bureaucrat had been dismissed, suspended, or had been reprimanded as a consequence of a disciplinary process in the public sector.

Similarly, Column 2 includes an indicator for being unable to work in the current position due to a past criminal, disciplinary, or fiscal investigation. Column 3 uses an indicator variable that equals 1 if the bureaucrat has a higher educational attainment than required for his current position. Finally, Column 4 uses the ratio between public sector experience (in semesters) and total work experience as the outcome.

The estimated coefficients convey two key messages. First, movements up the ladder are generally merit-based. Promoted bureaucrats are, on average, less likely to have a previous record of misconduct, or active inability causes, and tend to have more relevant experience and education than passed-over bureaucrats. However, in most cases, these effects are reversed or heavily attenuated when promotees were family connected to a manager or advisor when they were promoted. In other words, even though managers and advisors help promote better-suited individuals relative to other available and similar candidates, they are also more likely to overlook these qualifications when promoting family members.

6 Evaluating the impacts of anti-nepotism legislation

The previous sections demonstrate that family connections to public sector managers and advisors significantly distort key public employment outcomes. Top bureaucrats extract private rents in the form of earnings and promotions for their family members and hinder the selection of more qualified public sector employees.

How can regulatory agencies tackle this issue? Is anti-nepotism legislation effective at preventing this behavior? This section assesses whether introducing a more comprehensive anti-nepotism law mitigates some of these distortions. I evaluate the impacts of Colombia's 2015 anti-nepotism law that prohibited top bureaucrats from appointing, designating, nominating, or contracting (directly or indirectly) any family member up to the fourth degree of consanguinity.

6.1 Empirical strategy

I start by evaluating how family connections immediately responded to the policy change. I construct a biannual panel of public sector institutions from 2011 to 2017 in which each agency is represented by 16 observations (or bins) per period. These bins correspond to all degrees of separation (1–16), based on which I define and calculate $N_{k,s,t}$ as the total number of family connections per 10,000 employees in institution k, at degree of separation s, and time t. When constructing $N_{k,s,t}$, I only count the total number of family ties among bureaucrats at different hierarchical levels rather than all family connections to ensure I capture the asymmetries of power that could lead to favoritism. Using this new database and dependent variable, I estimate the following empirical specification:

(8)

$$N_{k,s,t} = \beta \cdot \left[\underbrace{\mathbb{1}(t \geq \text{2015-II})}_{\text{Post Reform}} \times \underbrace{\mathbb{1}(s \leq 4)}_{\text{Illegal}} \right] + \delta \cdot \underbrace{\mathbb{1}(t \geq \text{2015-II})}_{\text{Post Reform}} + \lambda \cdot \underbrace{\mathbb{1}(s \leq 4)}_{\text{Illegal}} + \alpha_k + \xi_{k,s,t},$$

where α_k represents a full set of agency fixed effects and $\mathbb{1}(\cdot)$ are indicator variables. Here, β captures the impact of the reform on family ties restricted by the law, i.e., those below 4 degrees of consanguinity. My preferred specification further accounts for institution-time fixed effects and degree of consanguinity fixed effects ($\gamma_{k,t}$ and λ_s, respectively) instead of the aggregate indicator variables of post-reform and illegal connections. These fixed effects fully control for agency-specific shocks over time and the overall distribution of connections at different degrees of separation. I cluster standard errors $\xi_{k,s,t}$ at the institution-separation level in all specifications, which corresponds to the level of identifying variation in this case. To account for potential differences in panel composition, I restrict the estimation sample in two ways. First, I focus on agencies with at least one family connection at any degree of separation over the whole period. Second, I analyze the subsample of institutions that started reporting information in the system before the policy change to address concerns about the merging of institutions after the reform and the differential timing in the adoption of the SIGEP. These modifications are without loss of identifying variation since the discarded observations are uninformative conditional on the fixed effects included in the model.

The identification assumption in this context is that, in the absence of the anti-nepotism legislation, bins above and below the threshold would have exhibited parallel trends in the number of family connections within institutions. I check the plausibility of this assumption by running the following event-study counterpart:

$$N_{k,s,t} = \sum_{\tau=\text{2011-I}, \tau \neq \text{2014-I}}^{\text{2017-II}} \beta_\tau \cdot [\mathbb{1}(t = \tau) \times \mathbb{1}(s \leq 4)] + \lambda_s + \gamma_{k,t} + \xi_{k,s,t} \tag{9}$$

where I expect β_τs in the pre-period to be statistically indistinguishable from 0.

6.2 Empirical results

6.2.1 Number of illegal connections

Table 5 presents the main results of the policy evaluation for different combinations of the fixed effects. The main coefficient of interest is stable across all columns. My preferred specification in Column 4 shows that following the reform, the number of illegal connections per 10,000 employees decreased, on average, by 9.1. Compared to the sample mean of 58, this implies a 15.6% reduction in the total number of family ties below 4 degrees of consanguinity. Crucially, these results are not explained by any common agency-specific shock or time-unvarying characteristics at the degree of separation or agency level.

This result is conditional on the parallel-trends assumption. Appendix Figure A-10 presents the corresponding event-study specification that checks the plausibility of this assumption. The estimated coefficients illustrate that there are no significant pre-trends and, more importantly, reveal that the dynamic effects are stable over time; if anything, they are slightly larger than the average effect reported in Table 5.

Table 5: Evaluating the anti-nepotism legislation of 2015

	(1)	(2)	(3)	(4)
Dependent Variable:	**Family connections per ten-thousand employees**			
Mean dependent variable:	58.01	58.01	58.01	58.01
Illegal degrees of separation × Post Reform	−9.0522	−9.0522	−9.0522	−9.0522
	(1.8582)	(1.8115)	(1.7989)	(1.8200)
Illegal degrees of separation	53.6483	53.6483		
	(1.9926)	(1.9088)		
Post Reform	0.0308	2.1223		
	(0.2029)	(0.3956)		
Fixed effects				
- Agency		Yes	Yes	
- Time			Yes	
- Degree of consanguinity			Yes	Yes
- Agency × Time				Yes
Observations	180,976	180,976	180,976	180,976
R-squared	0.0742	0.1232	0.1443	0.1540

Notes: Unit of observation is degree of separation-institution-time. The number of family connections excludes family ties at the same hierarchical level. The sample includes all institutions with at least one family connection at any degree of separation between 2011 and 2017. Standard errors clustered at the institution × degree of separation level in parentheses.

6.2.2 Differential impact across agencies

Appendix Table B-4 reports the results grouping the agencies by the branch of the government they belong to. These results yield two main conclusions. First, illegal connections are more widespread in the executive and judiciary branches (where the majority of institutions are and where the delivery of public goods occurs) than in the legislative branch or among autonomous and independent agencies such as the Central Bank, Office of the Attorney General, the *Superintendencias*, and public universities.

Second, the reform's impact is consistent with an overall reduction in family connections below 4 degrees of consanguinity. Appendix Figure A-12 presents the associated event studies for the three main branches of government, validating the identification assumption. These figures also confirm that most of the effects are coming from the reduction in family connections in the executive and judiciary branches. Although the effects for the legislative branch lack significance, the event-study specification demonstrates a rapid reduction in the number of illegal connections immediately after the law's introduction.

6.2.3 Differential impact across degrees of relatedness

According to Section 5.4, nepotistic returns are concentrated among family connections of 2–5 degrees of consanguinity. To determine whether the reform reduced the presence of these most problematic connections, following the same notation as in Equation 8, I estimate:

$$N_{k,s,t} = \sum_{\phi=1}^{15} \beta_\phi \cdot [\mathbb{1}(t \geq 2015\text{-II}) \times \mathbb{1}(s = \phi)] + \sum_{\phi=1}^{15} \lambda_\phi \cdot \mathbb{1}(s = \phi) + \gamma_{k,t} + \xi_{k,s,t}. \tag{10}$$

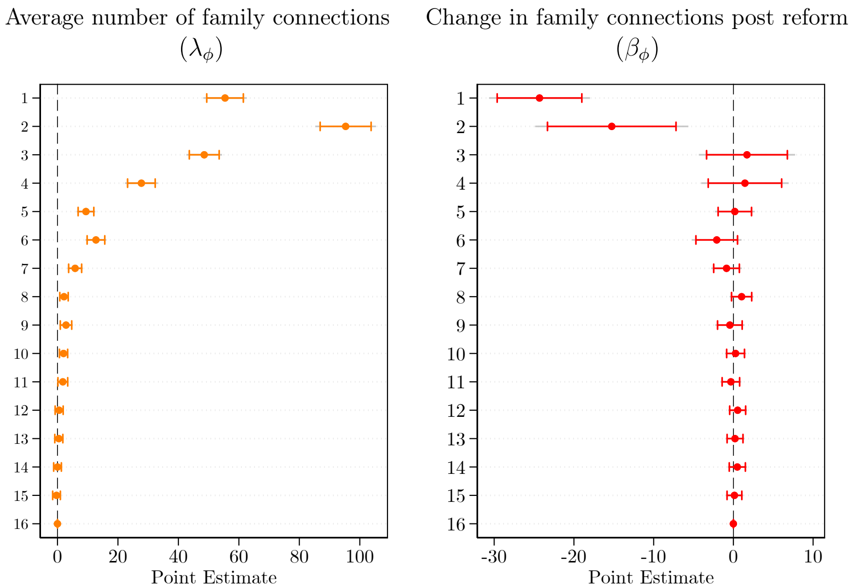
where λ_ϕ estimates the average number of family connections at degree of consanguinity ϕ before the law and β_ϕ captures the average change in family connections at the corresponding degree of separation after the reform was introduced. The excluded category in this specification, and therefore the reference point for all of these coefficients, is the bin of 16 degrees of consanguinity.

Figure 5 presents the estimated coefficients. There are two main takeaways from this figure. First, there was a significant decrease in the most common connections at degrees 1 and 2, corresponding to a reduction of 44% and 16%, respectively, from their sample means (Effects at 1 degree of consanguinity $= -24.31/55.41 \approx 43.8\%$; effects at 2 degrees of consanguinity $= -15.25/95.31 \approx 16\%$.). Second, the reform was completely ineffective at reducing connections at degrees 3 and 4. These results and those in Section 5.4 suggest that even though the policy had a significant impact on close family connections, it did not affect the most profitable (and hardest to identify) links.

6.3 Studying the impacts on performance

This subsection explores whether the 15% reduction in illegal connections impacted agencies' overall performance. According to the preliminary results of Table 2, the decrease in the total number of illicit connections could be expected to be

Figure 5: Effects of the 2015 anti-nepotism reform by relatedness



Notes: Point estimates and 95% and 90% confidence intervals corresponding to the coefficients λ_ϕ and β_ϕ in Equation 10. The reference category is 16 or more.

associated with an improvement in agency performance. I, therefore, run analogous regressions to those reported in Panel B of Table 2 to explore the relationship between agencies' overall performance and the existence of family connections below 4 degrees of consanguinity before and after the law was passed.

Table 6 reports the results. I extend Table 2 by adding an interaction term between the share of family connections below 4 degrees of consanguinity and an indicator variable of performance outcomes after 2015. The logic here is that after the quasi-experimental reduction of family connections that applied to all agencies, we could determine whether reducing the number of illegal family ties improved public sector performance.

I find that the negative relationship documented in Table 2 does not change significantly after the anti-nepotism legislation came into effect, even after controlling for a different set of institution-type fixed effects. Therefore, I conclude that the law did not reduce the total number of illegal connections *or* improve public sector performance.

A number of factors could explain this ineffectiveness, such as the law's differential enforcement over time or the limited time window of 1 year after the reform I analyze. However, in the next two subsections, I argue that bureaucrats' strategic response to the reform could partially explain why it was so ineffective and why bureaucratic nepotism has been so persistent in Colombia.

Table 6: Agency performance and close family connections after the reform

Dependent variable:	Agency performance index based on TI data					
	(1)	(2)	(3)	(4)	(5)	(6)
Share of connections below	−0.2757	−0.2840	−0.2196	−0.2975	−0.2326	−0.2332
4 degrees of consanguinity	(0.0602)	(0.0613)	(0.0637)	(0.0601)	(0.0795)	(0.0833)
Share of connections below	−0.0408	−0.0499	−0.0184	−0.0586	−0.0202	−0.0153
4 degrees of consanguinity × Post Reform	(0.0576)	(0.0584)	(0.0580)	(0.0591)	(0.0609)	(0.0620)
Fixed effects						
- Degree of centralization		Yes				Yes
- Administrative level			Yes			Yes
- Branch of the government				Yes		Yes
- Type of agency (legal nature)					Yes	Yes
Observations	419	419	419	419	419	419
Agencies	251	251	251	251	251	251
R-squared	0.1737	0.1808	0.1880	0.2161	0.2756	0.2994

Notes: Observations are at the agency-year level, using all public sector agencies in the 2014 and 2016 on ITEP. 'Share of connections below 4' denotes family connections within four degrees of consanguinity per 1,000 employees. Columns control for employee count and total family connections. The table displays standardized coefficients with robust standard errors in parenthesis.

6.4 Assessing bureaucrats' strategic response to the reform

6.4.1 Top bureaucrats' response

To determine how public sector managers and advisors reacted to the policy change, I estimate the differential impact of family connections to top bureaucrats after the introduction of the law using the following econometric specification:

$$(11)\ \ E_{i,t} = \theta_i + \delta_t + \gamma_k + \eta_1 \cdot B^{top}_{f,k,t} + \eta_2 \cdot \left[\underbrace{\mathbb{1}(t \geq \text{2015-II})}_{\text{Post Reform}} \times B^{top}_{f,k,t} \right] + \boldsymbol{X'_{it}\Phi} + \xi_{i,t},$$

where the notation is the same as in Equation 4. The outcome variables are again the log of total earnings and the indicator of hierarchical promotion. The coefficient of interest is η_2, which captures the differential return of family connections to top bureaucrats following the reform. Since this policy directly affected the appointment and promotion of family members, one would expect individuals with a family connection to a top bureaucrat to become less likely to be hierarchically promoted, and for their total earnings to remain unchanged given that they were not contemplated or covered by the law.

Table 7 presents the main results. The most demanding specifications in

Columns 3 and 6 indicate that the law reduced the likelihood of being hierarchically promoted by almost 50% with respect to the sample mean, a sizable decrease. However, those who became family-connected after the law was passed experienced a 2% increase in total earnings. These results are consistent with top bureaucrats substituting between the two available margins of favoritism.

These results are substantially different from those reported in the closest empirical setting to this paper. Xu (2018) finds that after the Warren Fisher reform, which ended patronage in the British empire, the salary gap between socially connected and unconnected governors disappeared entirely. By contrast, I find that top bureaucrats strategically responded to the new anti-nepotism legislation by reacting only to the restricted type of appointments.

Table 7: Nepotistic returns and the anti-nepotism law of 2015

Dependent variable:	Total Earnings			Hierarchical Promotion		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean dependent variable	9.22	9.22	9.22	0.033	0.033	0.033
Top Connected	0.02168	0.01585	0.01532	0.02779	0.02742	0.02556
	(0.00686)	(0.00682)	(0.00675)	(0.00142)	(0.00142)	(0.00142)
Top Connected × Post Reform	0.01982	0.01824	0.01905	−0.01652	−0.01646	−0.01523
	(0.00518)	(0.00511)	(0.00509)	(0.00114)	(0.00114)	(0.00114)
Time-varying controls						
- Private Experience		Yes	Yes		Yes	Yes
- Public Experience		Yes	Yes		Yes	Yes
Fixed effects						
- Bureaucrat fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
- Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
- Agency fixed effects			Yes		-	Yes
Observations	6,390,201	6,390,201	6,390,117	6,390,201	6,390,201	6,390,117
Bureaucrats	722,375	722,375	722,366	722,375	722,375	722,366
R-squared	0.73122	0.73208	0.74050	0.10883	0.10893	0.11363

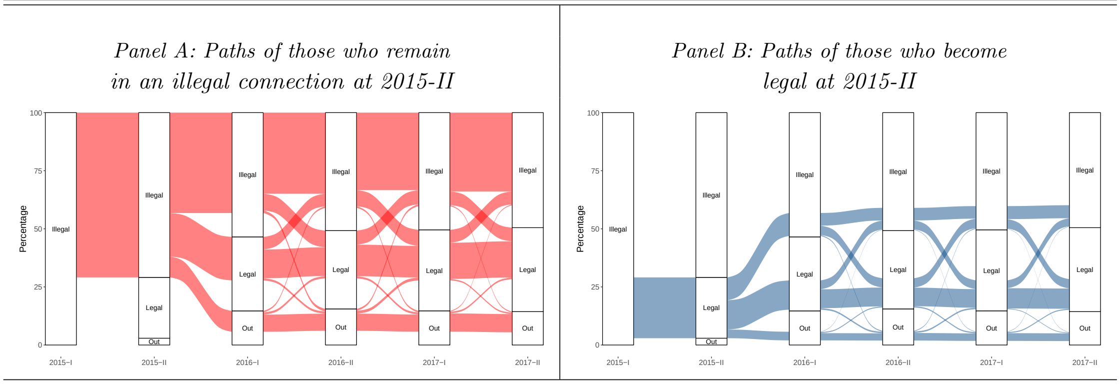
Notes: See notes in Table 3 since all those apply to this Table as well. Private and public experience varying by level of education l are included as follows $\sum_{l \in E}$ experience $\times\ \mathbb{1}(\text{education}= l)$. Post Reform equals 1 if the year is post 2015, the year the anti-nepotism law was enacted.

6.4.2 Middle- and lower-tier bureaucrats' response

Here, I restrict the analysis to non-top bureaucrats who may have been involved in an illegal connection one period before the law was enacted. I consider middle- and lower-tier bureaucrats connected to a top bureaucrat at 4 degrees of consanguinity or less in the same institution they were working in by the first half of 2015.

I follow these individuals over time through three mutually exclusive states: (1) "illegal" (bureaucrats stay put or become connected to another top bureaucrat at 4 degrees of consanguinity or less), (2) "legal" (bureaucrats move to another public sector agency with no family connection to a top bureaucrat at 4 degrees of consanguinity or less), and (3) "out" (bureaucrats leave public administration by moving to the private sector or becoming unemployed).

Figure 6: Recidivism and reshuffling within the public administration



Notes: This figure tracks middle and lower-tier bureaucrats involved in illicit connections during the first half of 2015 over time. For detailed data, refer to Appendix Table B-5. Panel A focuses on those who stayed "illegal" in the second half of 2015, while Panel B highlights those transitioning to "legal" in the same period.

Figure 6 displays the flow of bureaucrats from one state to another over time. The hollow bars denote the fraction (i.e., stock) of bureaucrats in each state from the first half of 2015 to the second half of 2017. Panel A presents the paths of bureaucrats who potentially had an illegal connection before the law was enacted and remained in the same state in the semester the law was passed. Similarly, Panel B traces the paths of those who shifted to the "legal" state in the semester the law was enacted.

The results presented in Figure 6 imply that the law was ineffective at purging the administration of these connections and is consistent with anecdotal evidence of the difficulty of eradicating this behavior within public administrations. Nearly half (40%) of the bureaucrats were entirely unresponsive to the reform, and only 13% left the public administration after 2 years as the law stipulated. More than 30% of these potentially illegal bureaucrats reshuffled within the public administration, while the recidivism rate was about 10% every period.

7 Discussion

Bureaucratic nepotism vis-à-vis other forms of favoritism How is bureaucratic nepotism different from other forms of favoritism in the public sector? why does this matter? In distinguishing bureaucratic nepotism from other forms of favoritism in the public sector, four distinct factors emerge. Firstly, bureaucratic nepotism can be more impactful because managers and supervisors significantly outnumber politicians in the public sector (by roughly 16:1 in Colombia, for example), and they are primarily responsible for personnel outcomes while operating under different civil service rules (Alesina & Tabellini, 2007, 2008; Spenkuch et al., 2023). Secondly, bureaucratic nepotism is economically consequential since family ties are predetermined and irrevocable compared to reversible social connections such as friendships or political affiliations. This distinction redefines accountability mechanisms in workplaces, particularly between quid-pro-quo based on political nepotism, cronyism, and political patronage. Thirdly, as bureaucrats generally hold their positions longer than elected officials, nepotistic practices could persistently influence the public sector workforce, potentially reducing diversity and impacting long-term service delivery (Rasul & Rogger, 2015). Lastly, the inherent differences in transparency between bureaucrats and politicians make nepotistic tendencies within bureaucratic systems harder to detect. While politicians are routinely subjected to public scrutiny, especially during elections, bureaucrats often function away from public view, further complicating the identification and accountability of nepotistic practices.

8 Conclusion

Bureaucratic nepotism is one of the most chronic and hard-to-identify pathologies within public administrations around the world. Yet, a lack of comprehensive data and suitable empirical settings have limited its measurement and understanding in modern bureaucracies.

By collecting and combining confidential information on bureaucrats' family ties and employer-employee records on the universe of civil servants in Colombia (2011–2017), this paper provides the first systematic empirical examination of bureaucratic nepotism and anti-nepotism legislation in an entire modern bureaucracy.

My results suggest that family networks, in general, and family connections to public sector managers and advisors, in particular, can severely distort the promotion, compensation, and performance of public sector workers. I show not only that close family ties are negatively related to the performance of government agencies and individual bureaucrats but also that workers who become family-connected to top bureaucrats receive significantly higher salaries and are more likely to be promoted. However, since promotions and compensation in the public sector are usually determined by rigid pay grades, I argue that these effects are mainly driven by allocating family members to higher-paid contracts, temporarily promoting workers to leadership positions, and temporarily filling vacancies that are in the process of being assigned via meritocratic examinations.

I demonstrate that these estimated private benefits occur at the cost of promoting workers with less public sector experience and education, and a history of misconduct, which directly affects the state's administrative capacity. When analyzing the introduction of the anti-nepotism legislation of 2015, I show that these distortions are difficult to overcome since (1) it is challenging to identify distant family connections and (2) workers can strategically respond to these reforms, since this type of legislation cannot cover all potential margins of favoritism available to managers and supervisors.

These findings have three important implications and inform the debate over public sector reforms designed to prevent nepotism and other forms of corruption within public administrations.

First, while anti-nepotism legislation has been extensively implemented in most countries, efforts to improve the monitoring and enforcement of these laws are usually inadequate. This makes it difficult to identify the problem over time and extremely challenging to overcome, especially in developing countries where state capacity is already low. My results point to the need for more systematic ways of identifying conflicts of interest based on administrative data and automated systems of transparency and enforcement. My empirical methodology provides a starting point for this improved way of detection using data that most governments in Latin America and other LMICs already collect.

Second, my results speak to the documented problem of temporary contracts and positions in the public sector. Prior work has established that politicians use such positions to reward political supporters (Colonnelli et al., 2020); I show that top *unelected* bureaucrats use them to extract rents for their family members, which supports recent findings by Brassiolo et al. (2021). Redirecting attention to limiting direct and temporary contracts thus constitutes an essential step in the fight against corruption in developing countries.

Finally, the overall emphasis on political rather than bureaucratic nepotism has jeopardized efforts to prevent the practice in the public sector. In this regard, my results also complement recent work that highlights the importance and influence of public sector managers and other senior bureaucrats in influencing public employment outcomes and public sector performance (Rasul & Rogger, 2018; Fenizia, 2022). However, my results indicate that context and opportunity determine the ultimate effects of discretionary appointments involving family members. Where state capacity is already low, allowing public sector managers to make discretionary decisions is detrimental to state performance and administrative capabilities, which contrasts with what others have found and proposed in more capable states (cf. Fenizia (2022)). While the design of optimal forms of monitoring and enforcement of anti-nepotism legislation is outside the scope of this paper, it is a fruitful avenue for future research.

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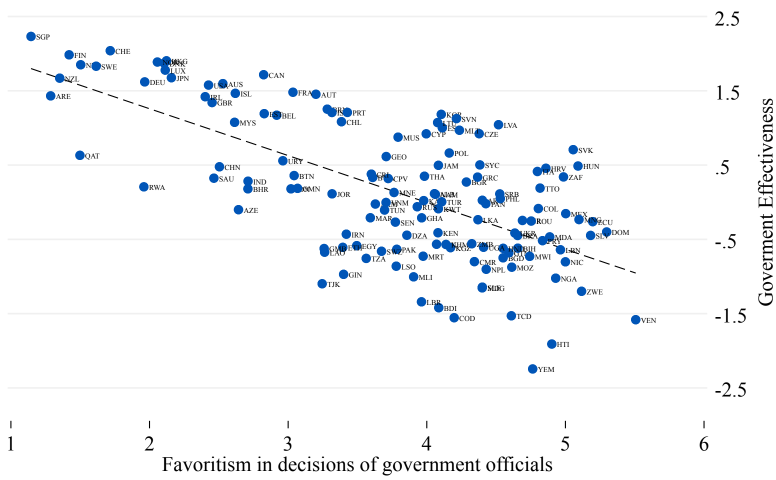
Online Appendix

Figures in the Appendix

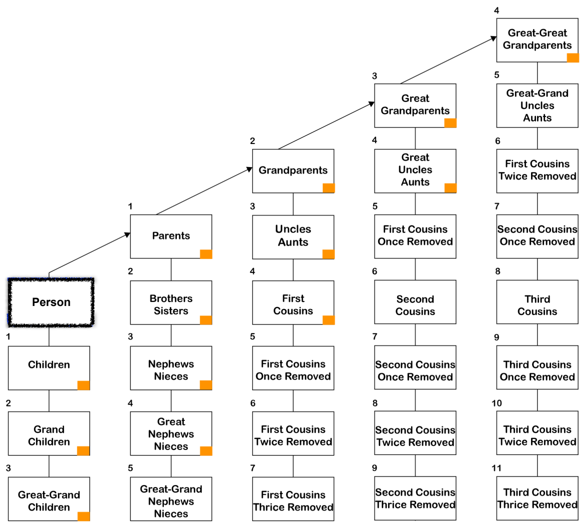
Tables in the Appendix

Figure A-1: Perception of favoritism and government effectiveness



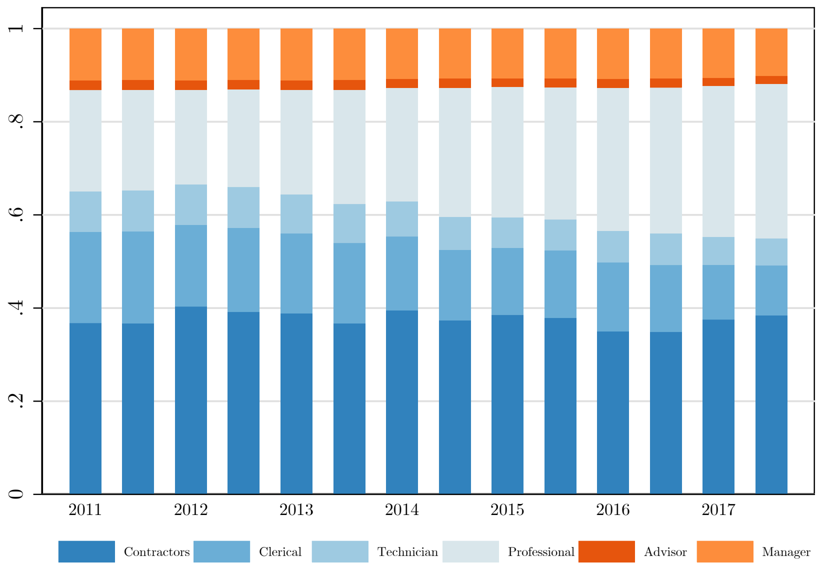
Notes: Data from the World Bank governance indicators and GovData360 (2018). Favoritism by government officials comes from The Global Competitiveness Report 2017-2018; the index goes from 1 = Never show favoritism to 7=Always show favoritism. The government effectiveness index measures the quality of public services, civil service, policy formulation, policy implementation and credibility of the government's commitment to raise these qualities or keeping them high. This index includes 193 countries ranked from -2.5 (least effective) to 2.5 (most effective).

Figure A-2: Consanguinity degrees and family relationships



Notes: This figure presents a table of consanguinity between different family relationships. The number next to each box indicates the degree of relationship relative to a given person highlighted in the bold box. For example, parents and children of this generic person are at one degree of consanguinity while first cousins and great uncles and aunts are at four.The relationships considered illegal according to the anti-nepotism legislation in Colombia are highlighted in orange. The degree of affinity through spouses is considered the same as the consanguineous level a couple was joined, so that, for example, the degree of affinity of a husband to his sister-in-law is two.

Figure A-3: Hierarchical composition of the public sector over time

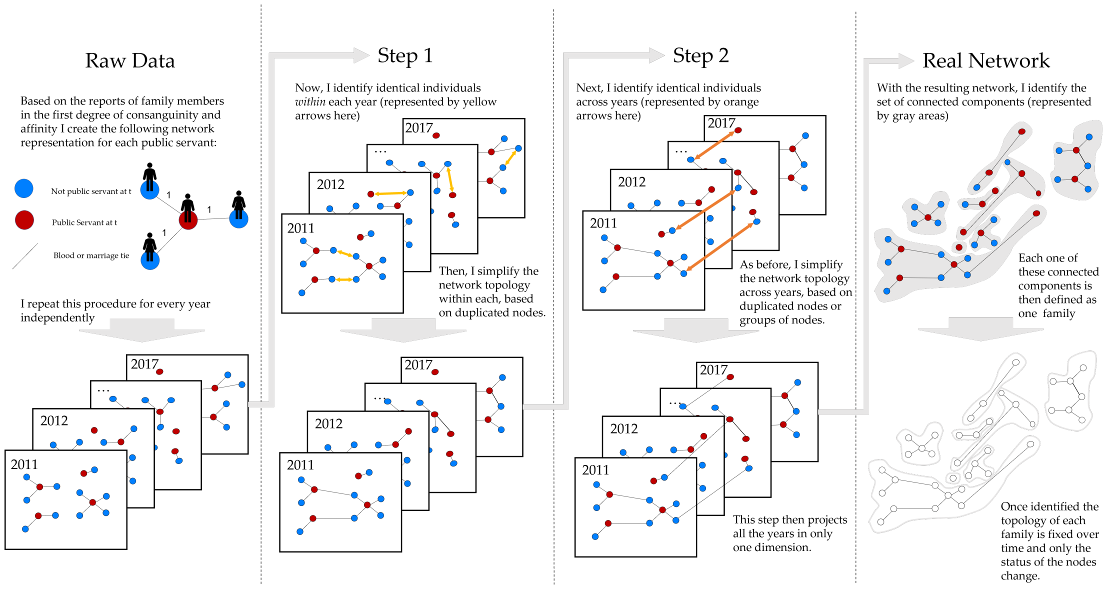


Notes: Hierarchical composition of the jobs within the Colombian public sector. It excludes elected officials, military, and police forces.

Figure A-4: Public Employment Information System, SIGEP

Notes: Figure displays an annotated example of the common CV format in the employer-employee database of Colombian public employment.

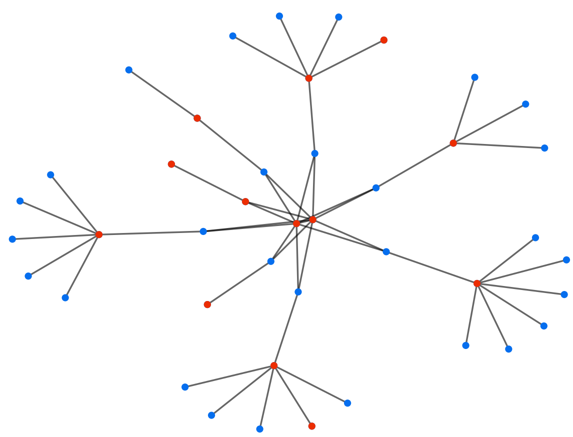
Figure A-5: Family network reconstruction



Notes: This figure represents a schematic diagram with the steps followed to convert the reports of family ties to the ultimate network topologies of the families used in the empirical strategy. The number of connections recovered in each step are presented in Table 1.

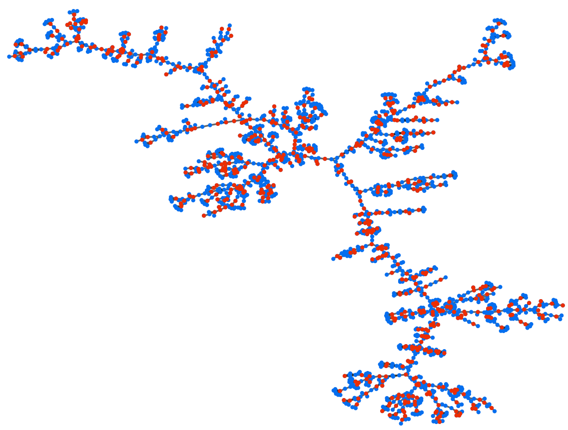
Figure A-6: Stepwise reconstruction of largest family network

Official Largest Family Network



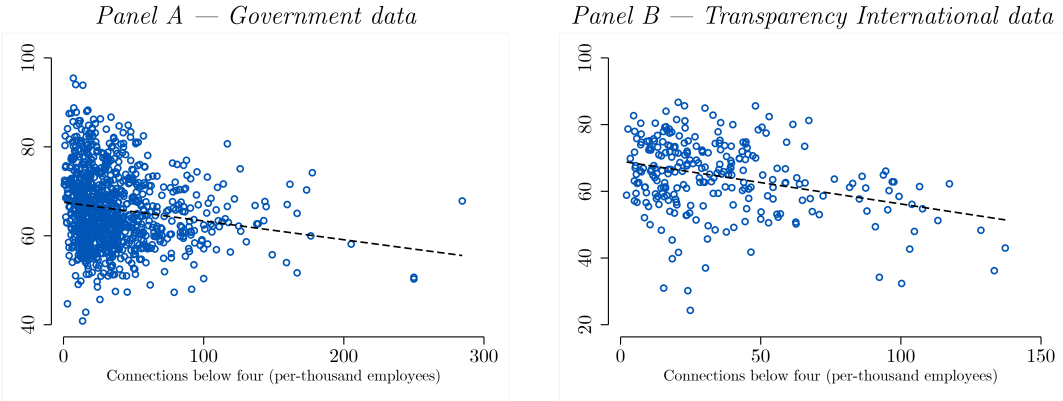
Panel A - Largest family network topology after Step 1

Real Largest Family Network



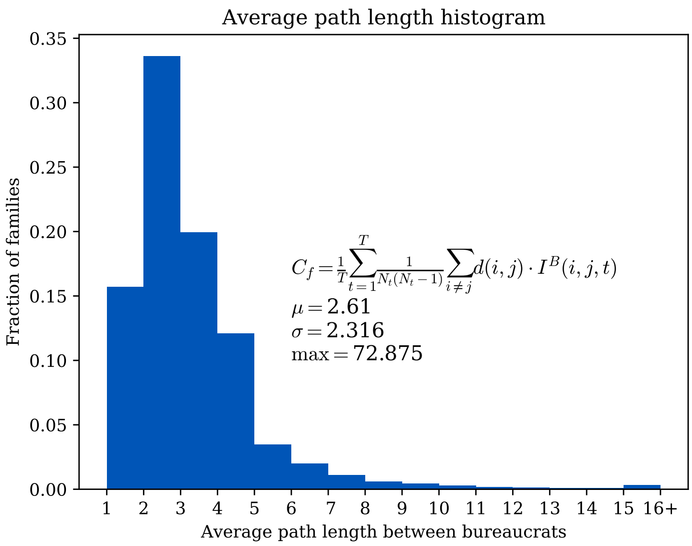
Panel B - Largest family network topology after Step 2

Figure A-7: Close family ties and agency performance



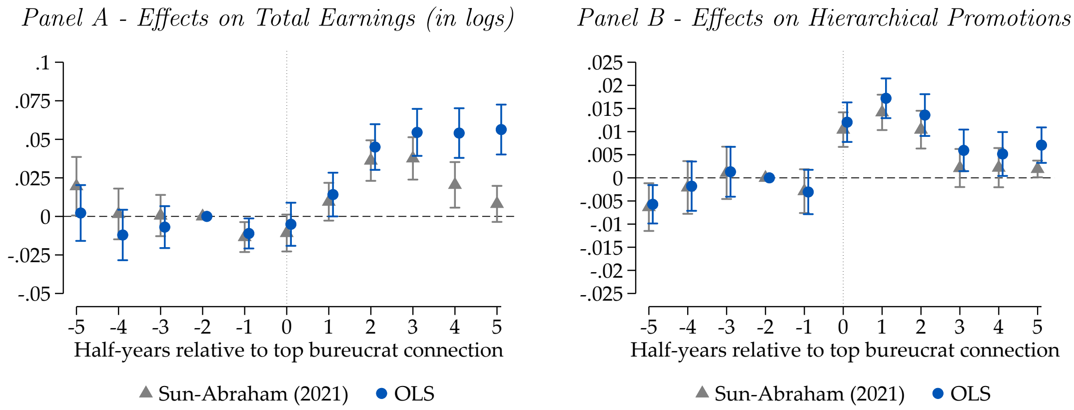
Notes: This figure presents the scatter plot and linear fit between the number of family connections below four degrees of consanguinity and the overall performance index of public sector agencies in 2016 according to government data (Panel A) and the independent assessment from Transparency International (Panel B). The corresponding regressions with further controls are reported in Table 2.

Figure A-8: Average degree of consanguinity between bureaucrats



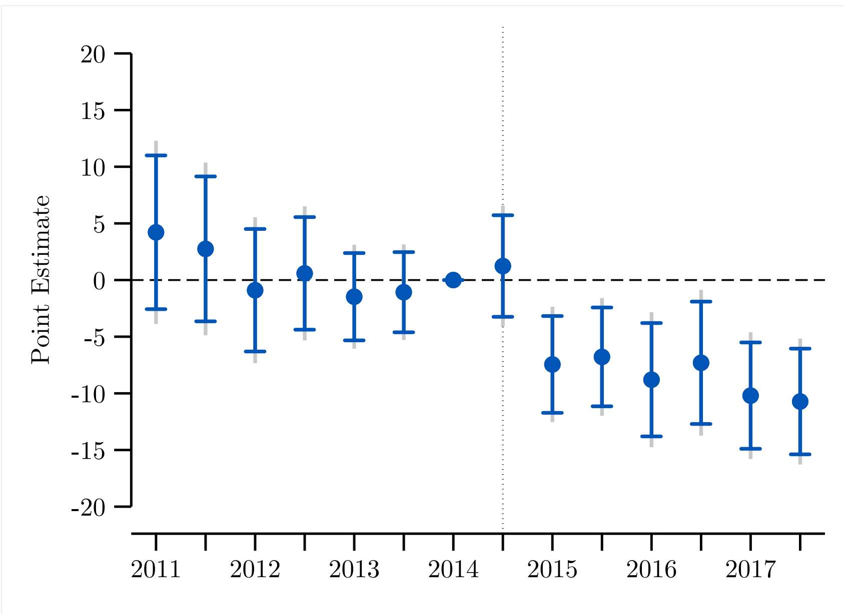
Notes: This figure displays the distribution and summary statistics across families of the average path length (in terms of degrees of consanguinity) between family members working in the public administration at the same time. For each family C_f is computed using Equation 1.

Figure A-9: Robustness using Sun and Abraham (2020) estimator



Notes: Figure displays the coefficients and 95% confidence intervals from the event-study of ever getting a family connection to a top bureaucrat (i.e., to a public sector manager or advisor) when looking at total earnings and hierarchical promotions as outcomes. It compares the coefficients estimated via the (Sun & Abraham, 2021) estimator and the OLS estimates. The OLS coefficients correspond to η parameters in equation 6. Standard errors are clustered at the dyadic family-agency level. The reference period is the year before the first family connection to a top bureaucrat (-2 half-years in the graph). Each set of coefficients in the figure is based on 6,390,117 panel observations coming from 722,366 bureaucrats and 34,887 connection events to top bureaucrats.

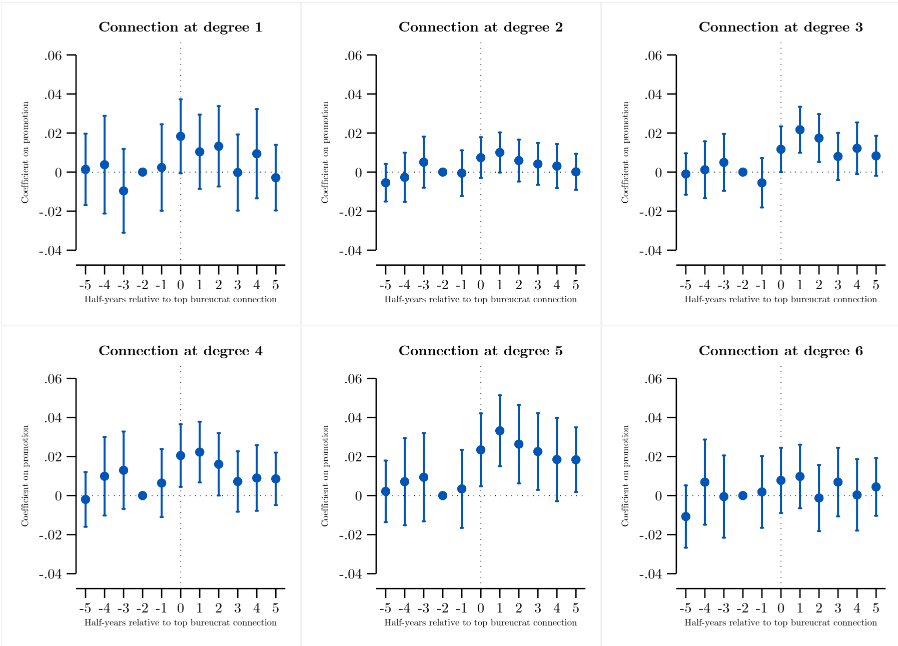
Figure A-10: 2015 Anti-Nepotism Reform: Effects on Illegal Connections



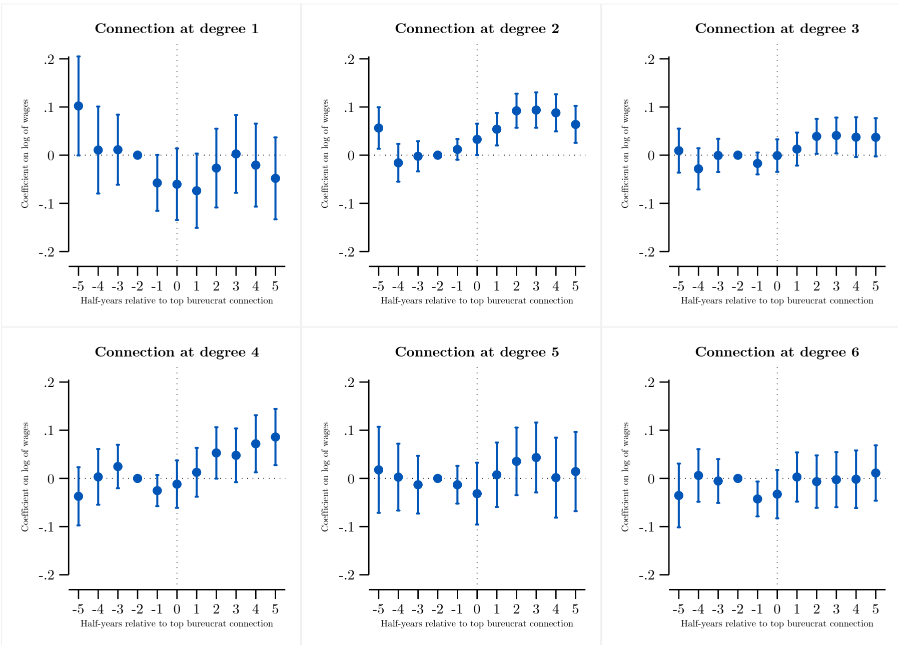
Notes: Point estimates and 95% and 90% confidence intervals corresponding to the coefficients β_τ in equation $N_{skt} = \sum_{\tau=2011\text{-I}}^{2017\text{-II}} \boldsymbol{\beta_\tau} \cdot [\mathbb{1}(t=\tau) \times \mathbb{1}(s \leq 4)] + \lambda_s + \gamma_{kt} + \xi_{skt}$. The reference period is the first semester of 2014.

Figure A-11: Returns to family ties: Impact on promotions by degree of separation

Panel A: Hierarchical Promotions

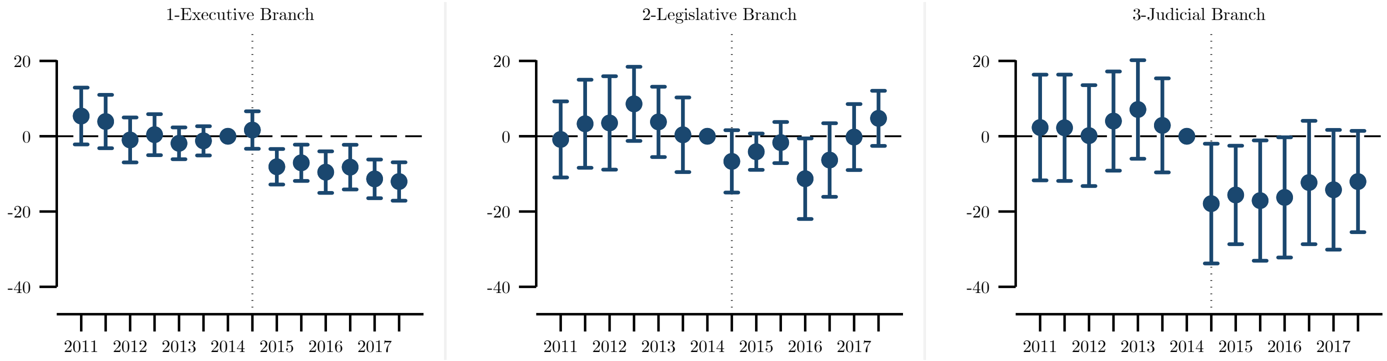


Panel B: Earning Increases



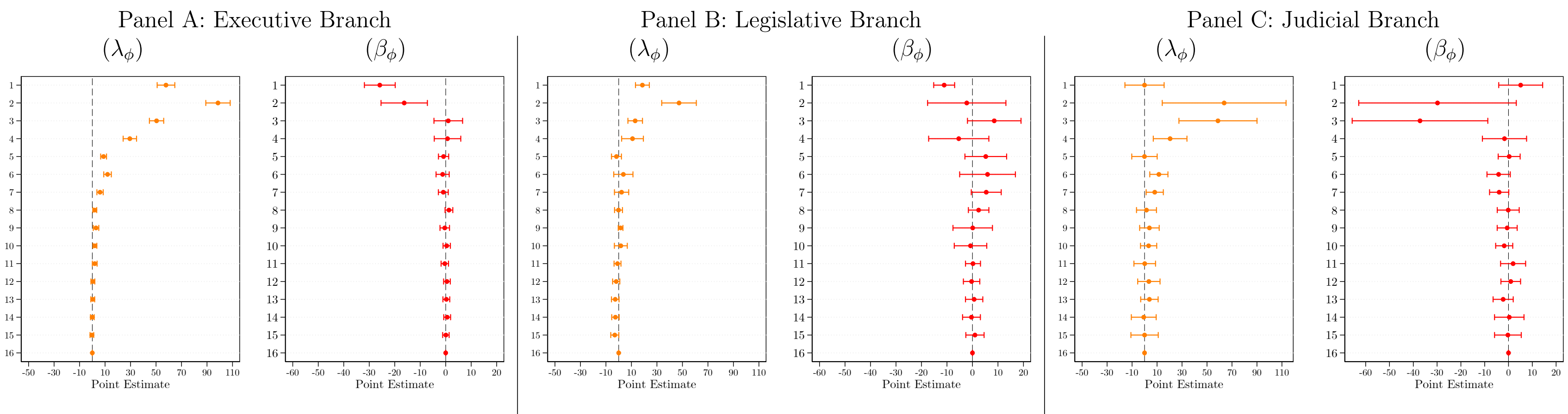
Notes: Figure shows coefficients from the event study on gaining a connection to a top bureaucrat across degrees of separation. Outcomes include hierarchical promotions (Panel A) and earnings increases (Panel B). Coefficients represent η parameters from Equation 6, with 99% confidence intervals and standard errors clustered at the family-agency dyadic level. The baseline is the year prior to the first top bureaucratic connection (-2 half-years in the graph).

Figure A-12: Event study plot by branch: Anti-nepotism legislation reform of 2015



Notes: Figure presents the point estimates and 90% confidence intervals corresponding to the coefficients β_τ in Equation 9. The reference period is the first semester of 2014.

Figure A-13: Anti-nepotism legislation effects by degrees of separation and branch



Note: Figure presents the point estimates and 90% confidence intervals corresponding to the coefficients λ_ϕ and β_ϕ in Equation 10. The reference category is family connections at 16 or more degrees of separation.

Table A-1: Descriptive statistics at the individual-time level

Variables	Mean	SD	Min	Max	Observations
Panel A - Full panel					
Wage (inverse hyperbolic sine of the wage)	7.165	3.806	0	13.641	11,524,713
Public sector experience (half-years)	10.613	15.823	0	116	11,524,713
Private sector experience (half-years)	5.264	8.335	0	104	11,524,713
Public sector employment	0.559	0.497	0	1	11,524,713
Enters into the public sector	0.057	0.232	0	1	11,524,713
Exits from the public sector	0.030	0.170	0	1	11,524,713
Has a family connection to...					
- any bureaucrat	0.353	0.478	0	1	11,524,713
- a top bureaucrat	0.166	0.372	0	1	11,524,713
Panel B - Private sector observations					
Total earnings (inverse hyperbolic sine)	4.557	4.412	0	13.278	5,082,626
Public sector experience (half-years)	1.94	5.287	0	91	5,082,626
Private sector experience (half-years)	6.295	8.789	0	104	5,082,626
Has a family connection to...					
- any bureaucrat	0.314	0.464	0	1	5,082,626
- a top bureaucrat	0.143	0.35	0	1	5,082,626
Panel C - Public Sector observations					
Total earnings (inverse hyperbolic sine)	9.224	0.975	0.002	13.641	6,442,086
Promoted	0.033	0.179	0	1	6,442,086
Public sector experience (half-years)	17.455	17.879	1	116	6,442,086
Private sector experience (half-years)	4.45	7.863	0	103	6,442,086
Hierarchical position is...					
- professional	0.293	0.455	0	1	6,442,086
- technician	0.092	0.289	0	1	6,442,086
- clerical	0.188	0.391	0	1	6,442,086
- contractor	0.427	0.495	0	1	6,442,086
Has a family connection to...					
- any bureaucrat	0.384	0.486	0	1	6,442,086
- a top bureaucrat	0.184	0.387	0	1	6,442,086
- any bureaucrat in the same agency	0.111	0.314	0	1	6,442,086
- a top bureaucrat in the same agency $\equiv$ Top Connected	0.027	0.162	0	1	6,442,086

Notes: Observations at the bureaucrat×half-year level. The panel includes all bureaucrats that never become top managers or advisors, n_{ntop} = 824,320.

Table A-2: Descriptive statistics at the individual level

Sample of individuals:	**Non Top Bureaucrats**		**Top Bureaucrats**		**All Bureaucrats**	
Observations:	(n_{ntop} = 824,320)		(n_{top} = 175,792)		(n_b = 1,000,112)	
Statistic:	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
Woman	0.515	0.500	0.479	0.500	0.508	0.500
Age at...						
- date of entry into the labor force	29.375	9.224	29.340	8.594	29.369	9.117
- date of entry into the public sector	32.192	9.241	31.603	8.989	32.088	9.200
- the beginning of 2011	34.389	10.716	38.686	10.180	35.145	10.749
Highest level of education is...						
- Ph.D. degree	0.003	0.055	0.014	0.118	0.005	0.071
- masters degree	0.047	0.211	0.119	0.323	0.059	0.236
- specialization degree	0.130	0.336	0.352	0.477	0.169	0.375
- college degree	0.256	0.437	0.244	0.430	0.254	0.435
- less than college degree	0.564	0.496	0.272	0.445	0.513	0.500
Has ever had a family connection to...						
- any bureaucrat	0.407	0.491	0.481	0.500	0.420	0.494
- a top bureaucrat	0.232	0.422	0.298	0.458	0.244	0.429
- any bureaucrat in the same agency	0.143	0.350	0.180	0.384	0.149	0.356
- a top bureaucrat in the same agency ≡ Top Connected	0.044	0.205	0.069	0.254	0.048	0.215

Notes: Observations at the bureaucrat level. Top bureaucrat refers to a bureaucrat in a hierarchical level of manager or advisor. Columns 1 and 2 present summary statistics for those individuals who never become top bureaucrats, while Columns 3 and 4 correspond to the same statistics for those who become managers or advisors in the public sector at some point in their careers.

Table A-3: Descriptive statistics at the individual-time level by connectedness

Sample of individuals:	Non Top Connected		Top Connected		All	
Observations:	6,267,732		174,354		6,442,086	
Statistic:	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
Wage (inverse hyperbolic sine of the wage)	9.223	0.975	9.234	0.958	9.224	0.975
Promoted	0.033	0.178	0.039	0.193	0.033	0.179
Public sector experience (half-years)	17.454	17.897	17.482	17.229	17.455	17.879
Private sector experience (half-years)	4.455	7.864	4.272	7.790	4.450	7.863
Hierarchical position						
- Professional	0.292	0.455	0.316	0.465	0.293	0.455
- Technician	0.092	0.289	0.107	0.309	0.092	0.289
- Clerical	0.189	0.391	0.155	0.362	0.188	0.391
- Contractor	0.427	0.495	0.422	0.494	0.427	0.495

Notes: Observations at the bureaucrat×half-year level. Top Connected refers to having a family connection to a top bureaucrat, i.e., a connection to a bureaucrat in a hierarchical level of manager or advisor.

A Effectiveness of the family network reconstruction

This appendix describes the simulation process I use to estimate the percentage of family linkages between ever-bureaucrats that I could reconstruct using the method proposed in Section 3.2.

The simulation procedure starts by randomly generating family network topologies (or family trees) of a given bureaucrat based on three parameters:

1. Number of generations modeled (generations living at the same time): $g \sim U\{2, 4\}$
2. The probability that individuals find a couple: $p \sim U[0, 1]$
3. The probability that once a couple is formed, it has k number of descendants: $q(k) \sim U[0, 5]$.

To simulate most of the family relationships displayed in Figure A-2 but to keep the problem bounded, I limit the generation of couples and descendants to one generation beyond the original family tree of g generations.

Once the base family network is created, I consider two additional dimensions that influence the simulation process and the ultimate performance of my algorithm:

1. The bureaucratic density of the network: Fraction of family members that are ever public servants
2. Truthfulness: The probability that a bureaucrat discloses each one of his/her family connections in the first degree of consanguinity or affinity.

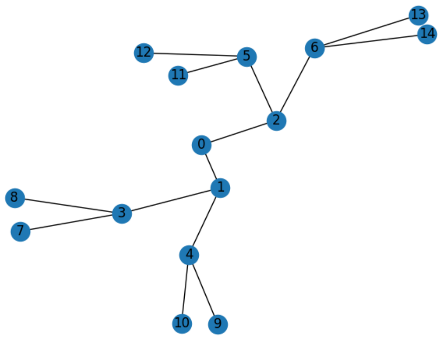
Next, I generate N number of family networks for a given level of truthfulness and bureaucratic density. Then, after applying the algorithm of family network reconstruction, I compute the fraction of bureaucrat-to-bureaucrat connections that I can recover for this combination of truthfulness and bureaucratic density. Figure A-14 presents, for reference, the four sub-stages followed in a representative instance of the simulation when $g = 4$, $p = 0.5$, and $\forall i \in \{0, 1, 2, 3, 4, 5\}, q(k = i) = 1/6$. The fourth stage shows the reconstructed topology after applying the method of family reconstruction and the percentage of family connections between red nodes (ever bureaucrats) that can be reconstructed.

Table A-4 presents the average percentage of bureaucrat-to-bureaucrat connections that I recover after simulating $N = 10,000$ families for each combination of Density and Truthfulness $= \{0.16, 0.33, 0.5, 0.66, 0.83, 1\} \times \{0.16, 0.33, 0.5, 0.66, 0.83, 1\}$, while Table A-5 shows the average number of connections per node of the reconstructed network for the same combination of parameters.

Now, I use the number of connections per node that I can observe in my simulations (Table A-5) and in the recovered part of the family network reconstructed in this paper (1.79 according to Figure 1) to approximate how much of the real network I might be recovering with my algorithm. To do so, I look at all the pairs of truthfulness and bureaucratic density such that 1.79 is included in the confidence interval of the simulations, Then, I look for those pairs in Table A-4 and argue that I am recovering about 14.65% to 27.22% of all bureaucrat-to-bureaucrat ties.

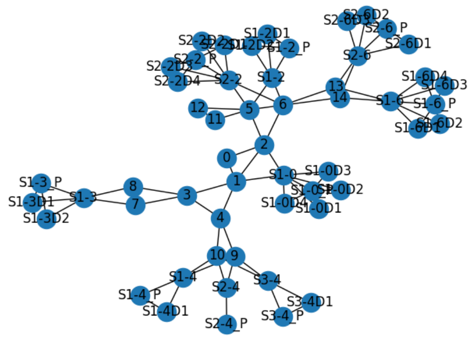
Figure A-14: Effectiveness of the family network reconstruction

Stage 1: Creates initial tree



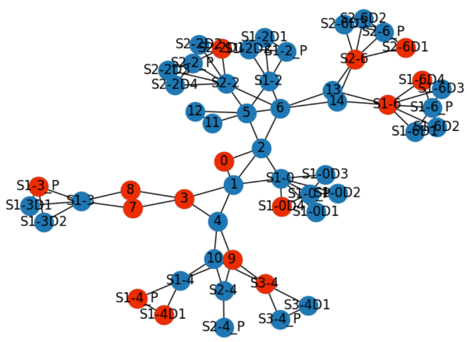
Basic family tree of individual "0" for $g = 4$ generations. In this example, "1" and "2" are "0"'s parents, while "5", "6", "3" and "4" are "0"'s grand-parents and so on.

Stage 2: Adds descendants and couples



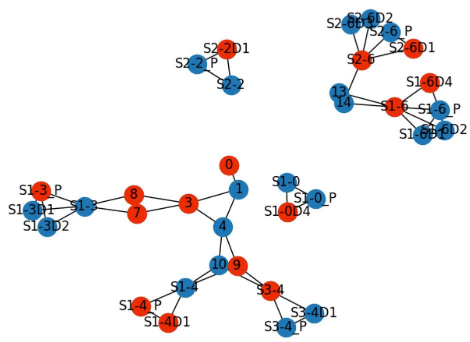
Adding additional descendants in each generation, their couples (if any) and their offspring (if any) with $p = 0.5$, and $q(k = i) = 1/6$. Also, add the implicit affinity linkages between couples.

Stage 3: Simulates density of bureaucrats



Red nodes represent individuals who are bureaucrats at some point in their lives. In this case, each node has a probability of 0.3 of being a public servant.

Stage 4: Applies the reconstruction algorithm



Use a level of truthfulness, in this case, 0.8, to recreate the network using the proposed algorithm. For this instance, the algorithm reconstructs 15 out of 105 possible bureaucrat-bureaucrat ties, about 14.28%.

Table A-4: Percentage of bureaucrat-bureaucrat connections recovered

Bureaucratic Density	Truthfulness 0.16	0.33	0.50	0.66	0.83	1.00
0.16	3.65	4.60	5.49	6.42	7.43	8.84
	[3.46 ; 3.85]	[4.44 ; 4.76]	[5.34 ; 5.65]	[6.26 ; 6.57]	[7.27 ; 7.59]	[8.66 ; 9.01]
0.33	8.84	12.07	16.05	20.51	27.22	35.19
	[8.52 ; 9.16]	[11.76 ; 12.37]	[15.73 ; 16.37]	[20.18 ; 20.84]	[26.84 ; 27.59]	[34.8 ; 35.59]
0.50	14.65	22.58	31.18	43.25	56.97	72.56
	[14.23 ; 15.07]	[22.14 ; 23.02]	[30.72 ; 31.64]	[42.77 ; 43.74]	[56.49 ; 57.45]	[72.14 ; 72.98]
0.66	21.19	33.80	48.18	65.11	81.61	93.83
	[20.68 ; 21.69]	[33.27 ; 34.32]	[47.63 ; 48.72]	[64.6 ; 65.62]	[81.21 ; 82.02]	[93.6 ; 94.06]
0.83	28.57	45.53	64.44	81.95	94.25	99.55
	[27.98 ; 29.15]	[44.95 ; 46.12]	[63.9 ; 64.99]	[81.52 ; 82.38]	[94 ; 94.5]	[99.49 ; 99.61]
1.00	35.96	56.50	77.47	92.36	99.02	100.00
	[35.31 ; 36.61]	[55.89 ; 57.12]	[76.97 ; 77.98]	[92.05 ; 92.67]	[98.9 ; 99.13]	[100 ; 100]

Notes: This table shows the percentage of recovered bureaucrat connections with 95% confidence intervals for combinations of bureaucratic density and truthfulness levels. Values are averages from 10,000 family tree simulations per cell, totaling 360,000 simulations.

Table A-5: Average number of connections per node

Bureaucratic Density	Truthfulness 0.16	0.33	0.50	0.66	0.83	1.00
0.16	0.80	0.84	0.86	0.97	1.01	1.09
	[0.58 ; 1.03]	[0.63 ; 1.05]	[0.69 ; 1.03]	[0.76 ; 1.18]	[0.86 ; 1.16]	[0.93 ; 1.24]
0.33	1.31	1.47	1.63	1.71	1.88	2.05
	[1.01 ; 1.6]	[1.26 ; 1.69]	[1.42 ; 1.84]	[1.49 ; 1.93]	[1.66 ; 2.1]	[1.89 ; 2.21]
0.50	1.61	2.00	2.15	2.35	2.52	2.66
	[1.29 ; 1.93]	[1.77 ; 2.24]	[1.94 ; 2.35]	[2.15 ; 2.55]	[2.33 ; 2.7]	[2.49 ; 2.83]
0.66	2.06	2.32	2.65	2.77	2.91	3.05
	[1.79 ; 2.32]	[2.09 ; 2.54]	[2.47 ; 2.83]	[2.59 ; 2.95]	[2.73 ; 3.1]	[2.9 ; 3.21]
0.83	2.41	2.75	2.93	3.05	3.18	3.22
	[2.16 ; 2.66]	[2.53 ; 2.98]	[2.75 ; 3.1]	[2.86 ; 3.24]	[3.03 ; 3.32]	[3.09 ; 3.34]
1.00	2.70	3.03	3.14	3.24	3.29	3.24
	[2.49 ; 2.9]	[2.87 ; 3.19]	[3 ; 3.28]	[3.1 ; 3.38]	[3.16 ; 3.42]	[3.09 ; 3.39]

Notes: This table presents the average number of connections per node and 95% confidence intervals associated with each combination of bureaucratic density and level of truthfulness specified in rows and columns. Each cell is calculated as the average across 10,000 family tree simulations (i.e., the table is based on 360,000 family tree simulations).

Table A-6: Agency performance & close family ties (government data)

Dependent Variable :	Agency Performance Index	Dimensions included in the performance index: Management of the Human Resources	Strategic Direction and Planning	Management by values towards Results	Evaluation of agency goals	Information and Communications with Citizens	Management Knowledge and Innovation	Disciplinary Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Close family connections	−0.0728	−0.0442	−0.0736	−0.0613	−0.0712	−0.0715	−0.0730	−0.0786
	(0.0120)	(0.0140)	(0.0127)	(0.0142)	(0.0137)	(0.0136)	(0.0152)	(0.0121)
Observations	3,853	3,853	3,853	3,853	3,853	3,853	3,853	3,853
R-squared	0.2183	0.1464	0.1881	0.2428	0.1755	0.2176	0.1523	0.2205

Notes: Observations are at the agency level. The sample includes all Public Sector agencies included in the *Medicion del desempeno Institucional* (MDI) database in 2016. Close family connections refer to the number of family connections below four degrees of consanguinity per one thousand employees within the agency. All columns control for the total number of employees. The table reports the standardized (beta) coefficients, i.e., dependent and independent variables were standardized before estimating the regressions. Robust standard errors in parenthesis.

Table A-7: Agency performance & close family ties (government data with fixed effects)

Dependent Variable :	Agency Performance Index	Aggregated dimensions included in the performance index: Management of the Human Resources	Strategic Direction and Planning	Management by values towards Results	Evaluation of agency goals	Information and Communications with Citizens	Management Knowledge and Innovation	Disciplinary Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Close Family Connections	−0.0356	−0.0076	−0.0247	−0.0379	−0.0302	−0.0352	−0.0252	−0.0395
	(0.0124)	(0.0150)	(0.0128)	(0.0145)	(0.0141)	(0.0136)	(0.0155)	(0.0123)
Fixed effects								
- Degree of centralization	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
- Administrative level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
- Branch of the government	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
- Legal nature	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,853	3,853	3,853	3,853	3,853	3,853	3,853	3,853
R-squared	0.3747	0.2983	0.3484	0.3743	0.3313	0.3360	0.2730	0.3710

Notes: Observations are at the agency level. Same notes of Table A-6 apply.

B Key robustness tests

Ruling out common shocks at the family and agency level While the absence of pre-trends and consistent signs across specifications alleviate concerns about time-varying individual-specific confounders, one alternative interpretation of my results is that the turnover and subsequent change in connectedness is masking other common shocks that affect additional sources of social connectedness or bureaucrats' coordinated behavior. One might worry that the results are not only capturing the role of family connections to top unelected bureaucrats but also reflect, for example, the ultimate influence of politicians targeting entire clusters of families (i.e., that they are simply reflecting patronage practices). Likewise, the results could be consistent with unconnected bureaucrats voluntarily withdrawing from the pool of potential promotees after a managerial turnover. Finally, jointly determined responses of family members to other reforms at the agency level could confound my results.

To address these valid additional concerns, in Appendix Table B-1 I extend the results of Table 3 by including a complete set of family-time and agency-time fixed effects that account for any potential agency- or family-specific shocks. The sign and significance of the coefficients of interest remain unchanged. In fact, once I include family-specific shocks, the role of being family-connected is significantly larger for both hierarchical promotions and total earnings.

Ruling out the influence of middle managers Members of large organizations spend considerable time and effort attempting to influence decision-makers (Milgrom, 1988). A potential concern with interpreting my results as nepotism is that they might be confounding the possible influence of non-family members in intermediate chains of command. For example, middle managers in big organizations could favor relatives of top bureaucrats to be on better terms with them after a managerial turnover.

While this influence could be considered a common shock to the family network (and thus covered by the previous robustness test), I address this concern by focusing on the direct chains of command that exclude middlemen's potential influence and by testing my baseline results depending on organizational size. Appendix Table B-2 presents the results on earnings by restricting the sample of bureaucrats in the hierarchical level of *professionals* – the level just below managers and supervisors – and by showing the heterogeneous effects of connections by agency size in terms of the total number of employees at each point in time. I cannot run the same exercise for hierarchical promotions, since those who reached the level of "professional" at t must have been below that hierarchical level in $t-1$. Thus the middle manager could be a "professional" themselves, which makes it difficult to rule out the intermediary influence. Comparing the estimated coefficients, I find that the magnitude of the returns in this group is only slightly smaller and if anything, decreases with agency size. Based on this evidence, these potential influence activities do not necessarily explain my overall results.

Excluding the impact of potentially endogenous connections As mentioned in Section 3, family ties are generally predetermined with respect to public employment outcomes. Blood ties between parents and children are unlikely to be explained by promotions or pay raises many years after blood connections originated. However, relationships involving spouses could be endogenous to public sector outcomes if, for instance, bureaucrats find their romantic partner in the workplace or divorce when both are employed by the public sector.

Since the lack of reporting of a spouse in the raw data could be driven by both separations in the workplace or strategic misreporting, I cannot distinguish the effects of those separate events in my data. However, to estimate the robustness of my results to the potential endogeneity in the formation of these connections, I report the baseline estimations of Table 3 by excluding family ties to top unelected bureaucrats when these involve relationships through spouses (I exclude all connections to spouses before running the two-step algorithm again). These results in Columns 3 and 6 of Appendix Table B-3 confirm that links through potentially endogenous ties lead to only a slight underestimation of the returns in terms of earnings (0.13 pp) and a slight overestimation of the nepotistic returns for hierarchical promotions (0.36 pp).

Table B-1: Ruling out family- and agency-specific common shocks

Dependent variable:	Total Earnings			Hierarchical Promotion		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean dependent variable	9.22	9.22	9.22	0.033	0.033	0.033
Top Connected	0.03047	0.02593	0.03297	0.01345	0.00957	0.02075
	(0.00565)	(0.00533)	(0.01270)	(0.00105)	(0.00103)	(0.00237)
Time varying controls by levels of education						
- Private experience	Yes	Yes	Yes	Yes	Yes	Yes
- Public experience	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects						
- Bureaucrat fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
- Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
- Agency fixed effects	Yes			Yes		
- Agency×Time FE		**Yes**			**Yes**	
- Family×Time FE			**Yes**			**Yes**
Observations	6,390,117	6,390,117	6,390,117	6,390,117	6,390,117	6,390,117
Bureaucrats	722,375	722,375	722,366	722,375	722,375	722,366
R-squared	0.74049	0.76522	0.93759	0.11358	0.19867	0.76775

Notes: The unit of observation is bureaucrat-time. The sample includes just bureaucrats within the public sector. Top connected is a dummy variable equal to one if the bureaucrat has had a family connection to a manager or advisor within the governmental agency he/she is working in. The sample includes all serving bureaucrats from 2011 to 2017. Promotion dummy refers to an upward change within the hierarchy of the institution. Log of earnings in thousand Colombian pesos. 51,969 singleton observations dropped. Standard errors clustered at the dyadic family-agency level in parentheses.

Table B-2: Addressing influence activities of in large organizations

	Total Earnings (log)			
	(1)	(2)	(3)	(4)
Top Connected	0.02670	0.04735	0.04170	0.02270
	(0.01027)	(0.01111)	(0.01099)	(0.01071)
Agency Size		-0.01650	-0.01834	-0.01062
		(0.00301)	(0.00300)	(0.00361)
Top Connected × Agency Size		-0.02340	-0.02127	-0.01054
		(0.00588)	(0.00583)	(0.00576)
Fixed effects				
- Bureaucrat fixed effects	Yes	Yes	Yes	Yes
- Time fixed effects	Yes	Yes	Yes	Yes
- Agency fixed effects				Yes
Time-varying controls				
- Private experience			Yes	Yes
- Public experience			Yes	Yes
Observations	1,876,040	1,876,040	1,876,040	1,876,040
R-squared	0.82924	0.82928	0.83008	0.83638

Notes: The unit of observation is bureaucrat-time. The sample includes just bureaucrats within the public sector in the hierarchical level of *Profesionales*. Top connected is a dummy variable equal to one if the bureaucrat has had a family connection to a manager or advisor within the governmental agency he/she is working in. The sample includes all *Profesionales* from 2011 to 2017. Log of earnings in thousand Colombian pesos. Agency size is the total number of employees at time t. Standard errors clustered at the dyadic family-agency level in parentheses.

Table B-3: Addressing the potential bias from endogenous marriage ties

Dependent variable:	Total Earnings			Hierarchical Promotion		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean dependent variable	9.22	9.22	9.22	0.033	0.033	0.033
Top Connected	0.03464	0.02902	0.03182	0.00987	0.00966	0.00981
	(0.00749)	(0.00745)	(0.00730)	(0.00135)	(0.00135)	(0.00135)
Time-varying controls by levels of education						
- Private Experience	-	Yes	Yes	-	Yes	Yes
- Public Experience	-	Yes	Yes	-	Yes	Yes
Fixed effects						
- Bureaucrat fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
- Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
- Agency fixed effects	-	-	Yes	-	-	Yes
Observations	6,390,201	6,390,201	6,390,117	6,390,201	6,390,201	6,390,117
Bureaucrats	722,375	722,375	722,366	722,375	722,375	722,366
R-squared	0.73119	0.73217	0.74057	0.10877	0.10886	0.11355

Notes: The unit of observation is bureaucrat-time. The sample includes just bureaucrats within the public sector. Top connected is a dummy variable equal to one if the bureaucrat has had a family connection to a manager or advisor within the governmental agency he/she is working in. The reconstruction of the family network in this table excludes connections through spouses. The sample includes all serving bureaucrats from 2011 to 2017. A promotion dummy refers to an upward change within the hierarchy of the institution. Total earnings refers to the inverse hyperbolic sine of the wage in thousand Colombian pesos. Private and Public Experience varying by level of education l are included as follows $\sum_{l \in E}$ experience $\times$ $\mathbb{1}$(education$= l$). Standard errors clustered at the dyadic family-agency level are in parentheses.

Table B-4: Anti-nepotism law of 2015: Effects by branch

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	**Total family connections per ten-thousand employees**				
	Branches of government			**Autonomous & Independent**	
Institution belongs to:	Executive	Legislative	Judicial	Others	Control & Regulation
Mean dep var pre reform	60.20	25.57	45.65	41.18	40.78
Illegal	56.0534	22.5980	32.8131	35.7671	33.8360
	(2.1762)	(5.5106)	(10.9587)	(3.5661)	(8.0265)
Illegal × Post Reform	-10.0303	-4.0021	-15.0945	1.6905	-14.8698
	(2.0412)	(4.0090)	(7.3605)	(2.7664)	(7.8932)
Fixed Effects					
- Institution × Time	Yes	Yes	Yes	Yes	Yes
Institutions	1,219	3	7	84	38
Observations	160,976	512	960	13,936	4,224
R-squared	0.1361	0.4514	0.5429	0.1749	0.0624

Notes: Unit of observation is degree of consanguinity-institution-time. The number of family connections includes all family ties between bureaucrats within the same institution at time t, i.e., excludes family ties at the same hierarchical level. The sample includes all institution-time observations with at least one family connection at any degree of separation between 2011 and 2017. Robust standard errors clustered at the institution × degree of consanguinity level in parentheses.

Table B-5: Transition matrices

Starting period		Next period 2015-II Illegal	Legal	Out
2015-I	Illegal	0.712739	0.258071	0.02919
Starting period		**Next period** 2016-I Illegal	Legal	Out
2015-II	Illegal	0.433404	0.193795	0.08554
	Legal	0.096269	0.123816	0.037986
	Out	0.00464	0.004736	0.019814
Starting period		**Next period** 2016-II Illegal	Legal	Out
2016-I	Illegal	0.410497	0.105741	0.018075
	Legal	0.095786	0.21042	0.016142
	Out	0.009472	0.016335	0.117533
Starting period		**Next period** 2017-I Illegal	Legal	Out
2016-II	Illegal	0.391456	0.113377	0.010922
	Legal	0.098492	0.219505	0.014498
	Out	0.013339	0.020298	0.118113
Starting period		**Next period** 2017-II Illegal	Legal	Out
2017-I	Illegal	0.398512	0.098299	0.006476
	Legal	0.09221	0.251885	0.009086
	Out	0.005993	0.013532	0.124009

Notes: This table shows the transition matrices of middle- and lower-tier bureaucrats initially involved in illegal connections in the first semester of 2015. They progress through three states: "illegal," "legal," or "out". The "illegal" state is for those staying or reconnecting within four degrees of consanguinity to top bureaucrats. "Legal" is when they move to agencies with no such family connections. "Out" is when they exit public service for the private sector or unemployment.

C Measurement error and potential OLS bias

This appendix explores the potential measurement error and bias in the baseline specification from Section 5. Chandrasekhar & Lewis (2016) emphasize that "incomplete survey coverage of network nodes may lead to biased aggregate network statistics even if nodes are randomly sampled"(de Paula et al., 2023).

Following Chandrasekhar & Lewis (2016), let G represent the global and historical family tree (network) consisting of all relatives of bureaucrats in the Colombian public administration. We are interested in estimating the magnitude and sign of the potential bias generated by the sampling method and family network reconstruction described in Section 3.2. Assume, for simplicity, that we are interested in estimating the simplified version of my main estimated equation in Equation 4 given by $E_{i,t} = \eta \cdot B^{top}_{i,t}(G) + \epsilon_{i,t}$, where we explicitly state that our definition of family connectedness to a top bureaucrat, $B^{top}_{i,t}$, depends on the true and underlying family tree network G. Now consider that the econometrician only observes $\widetilde{G}$, the induced sub-graph of family networks reconstructed by the two-step algorithm in Section 3.2, and therefore observe only $\widetilde{B^{top}_{i,t}}(\widetilde{G})$. Under the assumption that $\mathbb{E}[\widetilde{B^{top}_{i,t}}|B^{top}_{i,t}] = \pi \cdot B^{top}_{i,t} + o(1)$, the resulting estimator of η satisfies,

$$\text{plim}\,\hat{\eta} = \eta \cdot \frac{\text{Cov}(\widetilde{B^{top}_{i,t}}, B^{top}_{i,t})}{\text{Var}(\widetilde{B^{top}_{i,t}})} = \eta \cdot \overbrace{\underbrace{\frac{1}{\pi}}_{\text{Scale}} \cdot \underbrace{\frac{\text{Var}(B^{top}_{i,t})}{\text{Var}(B^{top}_{i,t}) + \frac{1}{\pi^2} \cdot \text{Var}(\widetilde{B^{top}_{i,t}} - \mathbb{E}[\widetilde{B^{top}_{i,t}}|B^{top}_{i,t}])}}_{\text{Classical attenuation bias}}}^{\text{Overall bias}}, \tag{12}$$

where it is clear that, depending on the sign and magnitude of the scale effect (i.e., π), we can overestimate or underestimate the true value of η.

To make progress on the task at hand, I extend the simulation process described in Appendix A to estimate π by running bi-variate regressions between $B^{top}_{i,t}$ and $\widetilde{B^{top}_{i,t}}$ without intercept across simulations. To do so, I assume further that, according to the hierarchical structure observed in my data (see., Figure A-3), the fraction of top bureaucrats in the public sector is 0.13. Based on this, for each simulation, I obtain an estimate of π and the overall bias for different combinations of bureaucrat density and truthfulness in reporting. Appendix Tables C-1 and C-2 present the results. Since the overall bias is always less than one and $\in [0.017, 0.098]$ for the applicable combinations of truthfulness and bureaucratic density used in simulations in Appendix A, I argue that, if anything, the sampling method proposed in this paper lead us to underestimate the actual return of family connections to top bureaucrats.

Table C-1: Estimating scale effect to evaluate measurement error bias

Bureaucratic Density	Truthfulness 0.16	0.33	0.50	0.66	0.83	1.00
0.16	0.134	0.127	0.164	0.192	0.241	0.290
	[0.13 ; 0.14]	[0.12 ; 0.13]	[0.16 ; 0.17]	[0.18 ; 0.2]	[0.23 ; 0.25]	[0.28 ; 0.3]
0.33	0.215	0.244	0.356	0.458	0.556	0.675
	[0.2 ; 0.23]	[0.23 ; 0.25]	[0.34 ; 0.37]	[0.45 ; 0.47]	[0.54 ; 0.57]	[0.66 ; 0.69]
0.50	0.264	0.377	0.533	0.679	0.798	0.893
	[0.25 ; 0.28]	[0.36 ; 0.39]	[0.52 ; 0.55]	[0.67 ; 0.69]	[0.79 ; 0.81]	[0.89 ; 0.9]
0.66	0.336	0.517	0.691	0.826	0.903	0.963
	[0.32 ; 0.35]	[0.5 ; 0.53]	[0.68 ; 0.7]	[0.82 ; 0.84]	[0.9 ; 0.91]	[0.96 ; 0.97]
0.83	0.425	0.627	0.789	0.887	0.951	0.991
	[0.41 ; 0.44]	[0.61 ; 0.64]	[0.78 ; 0.8]	[0.88 ; 0.9]	[0.94 ; 0.96]	[0.99 ; 0.99]
1.00	0.478	0.687	0.853	0.926	0.979	1.000
	[0.46 ; 0.5]	[0.67 ; 0.7]	[0.84 ; 0.86]	[0.92 ; 0.93]	[0.97 ; 0.98]	[1 ; 1]

Notes: This table presents the estimated scale parameter π, from Equation 12, and the 95% confidence intervals associated with each combination of bureaucratic density and level of truthfulness specified in rows and columns. Each point estimate is the average calculated across 10,000 family tree simulations (i.e., the table is based on 360,000 family tree simulations).

Table C-2: Estimation of the overall bias induced by measurement error

Bureaucratic Density	Truthfulness 0.16	0.33	0.50	0.66	0.83	1.00
0.16	0.041	0.019	0.015	0.008	0.008	0.006
	[0.03 ; 0.05]	[0.01 ; 0.02]	[0.01 ; 0.02]	[0.01 ; 0.01]	[0.01 ; 0.01]	[0 ; 0.01]
0.33	0.072	0.033	0.027	0.019	0.017	0.016
	[0.06 ; 0.08]	[0.03 ; 0.04]	[0.02 ; 0.03]	[0.01 ; 0.02]	[0.01 ; 0.02]	[0.01 ; 0.02]
0.50	0.075	0.051	0.052	0.047	0.043	0.053
	[0.06 ; 0.09]	[0.04 ; 0.06]	[0.04 ; 0.06]	[0.04 ; 0.06]	[0.03 ; 0.05]	[0.04 ; 0.07]
0.66	0.098	0.081	0.076	0.084	0.102	0.162
	[0.08 ; 0.11]	[0.07 ; 0.09]	[0.06 ; 0.09]	[0.07 ; 0.1]	[0.08 ; 0.12]	[0.14 ; 0.18]
0.83	0.138	0.108	0.135	0.162	0.257	0.618
	[0.12 ; 0.15]	[0.09 ; 0.12]	[0.12 ; 0.15]	[0.14 ; 0.18]	[0.23 ; 0.28]	[0.59 ; 0.65]
1.00	0.159	0.151	0.207	0.331	0.697	1.000
	[0.14 ; 0.18]	[0.13 ; 0.17]	[0.18 ; 0.23]	[0.3 ; 0.36]	[0.67 ; 0.72]	[1 ; 1]

Notes: This table presents the estimated overall bias, from Equation 12, and the 95% confidence intervals associated with each combination of bureaucratic density and level of truthfulness specified in rows and columns. Each estimate is the average calculated across 10,000 family tree simulations (i.e., the table is based on 360,000 family tree simulations).