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# Corruption and Talent Allocation*

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**Abstract**

Talent is a key input in the delivery of public services, yet less is known about what affects the supply of talent for the public sector. This paper studies the role of political corruption in shifting talent allocation across public and private sector careers. I exploit a randomized anti-corruption audit program in Brazil together with comprehensive micro-data on educational and labor market outcomes of college students. Using a generalized difference-in-difference research design, I find that high-ability students in audited municipalities are less likely to choose majors tailored toward public sector careers, such as business administration and law. Moreover, tracking students to the labor market demonstrates that audits also lead to a lower share of high-ability students working as civil servants. Finally, I provide suggestive evidence that the effects of audits on talent allocation can be driven by the perception of lower rent-seeking returns and higher reputation costs. Taken together, these findings highlight an understudied negative consequence of corruption on the economy: the distortion of talent allocation toward rent-seeking in the public sector.

**JEL classification:** D73, H83, I25, J24
**Keywords:** Corruption, Audits, Talent Allocation, Major Choice, Public Sector

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# 1 Introduction

Talent can be a key determinant of productivity in the public sector (Fenizia, 2022; Best et al., 2023). Across different countries in the world, talented individuals opt for the public sector seeking prestige, job stability, or a wage premium over the private sector. Corruption could be another relevant factor that affects the attractiveness of public sector careers.[1] In theory, corruption could increase the relative rewards of rent-seeking activities, thus luring talent away from potentially more productive activities such as firm creation (Baumol, 1990; Acemoglu, 1995). Conversely, corruption might crowd out individuals who are equally talented but have a higher intrinsic motivation to work in the public sector.[2] The ambiguity in theoretical predictions calls for an empirical investigation of how corruption shifts a society's talent allocation, yet establishing causality proves challenging due to the endogenous nature of corruption.

This paper studies the impacts of combating corruption on the allocation of talent across the public and private sectors.[3] I address the identification challenge by leveraging a plausibly exogenous shock to rent-seeking opportunities in local governments: randomized anti-corruption audits. The context of Brazil provides a unique policy experiment to address this empirical question: a large-scale randomized audit program implemented among municipal governments from 2003 to 2015. As a top-down effort to fight corruption, the audit program has been demonstrated to diminish corruption in local governments effectively (Avis et al., 2018).[4] Linking the occurrence of audits to detailed administrative records on higher education and the labor market, I investigate how audits trigger the reallocation of talent across public and private sectors. My findings reveal that high academic achieving students in Brazil shy away from public-sector career paths after government anti-corruption efforts, both in terms of college major choice and realized careers

[1] Hanna and Wang (2017), Barfort et al. (2019) and Gans-Morse (2022) provide experimental evidence on distinctive patterns of self-selection of (dis)honest individuals into the public sector in institutional settings with different levels of corruption. Exploiting a natural experiment in Argentina, Cruces et al. (2023) also demonstrates that dishonest behavior in youth predicts a higher propensity to occupy non-meritocratic public sector jobs later in life.

[2] An implicit assumption for this argument to be relevant is that intrinsic motivation is independent of or positively correlated with ability among the applicant pool, as suggested by the literature on motivation crowding out by extrinsic rewards in different contexts of public sector hiring (Dal Bó et al., 2013; Deserranno, 2019; Ashraf et al., 2020). For other relevant literature on intrinsic motivation, see Frey and Jegen (2001), Bénabou and Tirole (2003, 2006), Besley and Ghatak (2005, 2018), Prendergast (2007, 2008).

[3] It is worth noting that this paper focuses on corruption in the public sector, broadly defined as activities that involve exploitation of public office for private gain (Fisman and Golden, 2017).

[4] Specifically, they find that being audited in the past reduces future corruption acts by 8%, where the increased perception of nonelectoral costs of engaging in corruption (such as legal punishment or reputation costs) plays a major role.

in the labor market.

To construct the dataset, I utilize various sources of country-wide administrative data for Brazil at the individual level on both higher education and the labor market. With the universe of college students recorded in the higher education census, I focus on those enrolled in universities during 2010-2018 as the pool of talent. I further classify students as high- or low-ability based on their performance in a standardized exam taken prior to college application. The *allocation of talent* is characterized along two margins: pre-labor market sorting of college majors and early-career labor market sorting. First, I define individual students' exposure to audits based on their municipality of residence right before college enrollment. The higher education census then allows me to observe the specific degree program students enrolled in tertiary education.[5] Finally, using individual identifiers linking higher education census to the Brazilian employer-employee data, I track students to the labor market and observe whether they obtain their first jobs in the public or private sector. The final dataset constructed, to the best of my knowledge, is the most comprehensive data ever used to study how nationwide anti-corruption efforts affect the allocation of talent within a society.

The randomized and staggered nature of the anti-corruption audits across time and locality leads naturally to a municipal-level event-study estimation method. My preferred specification follows a stacked-by-event event-study design, which estimates the treatment effects based on the comparison of units switching into treatment to not-yet-treated units in the time window of interest.[6] As the outcomes I observe for students are available from 2010 to 2018, I restrict my analysis to audits conducted during 2011-2014, the later stage of the randomized phase of the entire anti-corruption program.[7] Students from municipalities that received an audit for the first time during 2011-2014 are thus taken as the treated group, while those from never-audited municipalities throughout the program (till 2018) are included as "clean" controls.

I begin my main analysis by documenting two empirical facts on baseline patterns of

[5]Prospective students in Brazil apply for specific degree-institution programs (Law degree at the University of Brasilia, for instance). This system is similar to that of China and continental Europe but different than the United States, where students decide on fields of study in the first years of university studies. Switching majors during college in Brazil is often not allowed or comes with a large cost (Oliveira et al., 2022).

[6]By explicitly eliminating "forbidden" comparisons between units treated earlier versus later, this method deals with potential biases of the standard two-way fixed-effect (TWFE) estimator in the presence of treatment effect heterogeneity, as highlighted in De Chaisemartin and d'Haultfoeuille (2020) and Goodman-Bacon (2021), among others.

[7]The randomized phase of the program lasted till 2015. Municipalities audited in 2015 are not included in the treated sample due to an arbitrary change in eligibility criteria in terms of municipality population. In addition, the program was upgraded in 2015 and entered the non-randomized phase. Municipalities that were audited in the non-randomized phase during 2015-2018 are excluded from the control group.

major enrollment and subsequent careers. First, among all degree fields, business administration and law[8] is the most popular choice among high-ability students (defined as students with top 25% exam performance), followed by engineering. Second, students in business/law exhibit a high propensity to become civil servants, especially compared to engineering students, who are the least likely to join civil service across all majors. These two pieces of evidence motivate the main focus of my first set of empirical analyses on the comparison between enrollment in business/law versus engineering.

I then examine how anti-corruption audits affect college major enrollment, where major is taken as a proxy for intended careers. I find that students from audited municipalities are 5.3% less likely to major in business/law and 9.4% more likely to choose engineering relative to their counterparts from municipalities that never receive an audit. I show that the effects are driven by major-switching behavior, rather than the entry of new students. A simple back-of-the-envelope calculation suggests that on average, 1 in 70 students switches major after anti-corruption audits. Moreover, the effects on major enrollment persist in the longer run (up to seven years) for younger enrollment cohorts. Notably, separately examining public and private universities reveals that the effects on major shares are almost fully concentrated in private universities. The results are consistent with the interpretation that public institutions in Brazil are more competitive and over-subscribed, while private institutions can flexibly cater to the market demand. However, the lack of reactions in aggregate enrollment masks underlying changes in student composition. Decomposition by student ability reveals a relative decline of 13.4% in the share of high-ability students studying business/law in public institutions. To the extent that major choice reflects career preferences, the results thus far suggest that audits lead to an inferior candidate pool aspiring for public sector careers.

Next, I track the students to the labor market using employer-employee data and demonstrate that the negative sorting by ability observed in major enrollment further translates to final hires in civil service. Overall, audits are associated with more students landing first jobs in the private sector, yet do not significantly impact the aggregate number of students working in the public sector. A closer examination of workforce composition, however, sheds light on heterogeneous responses to audits by student ability. Students from audited municipalities who take up first jobs in the civil service are of lower ability as measured by the high school exam performance, while the opposite happens to their private sector counterparts. In particular, audits lead to more than 50% relative decline in

[8]Referred to as "business/law" for simplicity in the rest of the paper. Specifically, it includes subfields such as accounting and taxation, management and administration, finance, banking and insurance, secretary and clerical work, law etc.

the share of high-ability students among all civil servants. This result falls in line with the recoil of high-ability students from public-sector-oriented majors in higher education. Together, these findings illustrate a *brain drain* out of the public sector following government anti-corruption audits.

Why would anti-corruption audits divert high-ability students away from the public sector trajectories? I investigate three main hypotheses of how top-down anti-corruption efforts might trigger the behavioral responses of students. The first explanation is that audits may lead to a perception of reduced corruption opportunities and/or increased corruption monitoring in the public sector. I refer to this first channel as *diminished rent-seeking*, following the long-standing literature on rent-seeking, talent allocation and productivity growth (Baumol, 1990; Murphy et al., 1991, 1993; Acemoglu, 1995). Second, by revealing local corruption to the public, audits could drive away pro-social individuals who are intrinsically motivated to work in the public sector.[9] I refer to this second channel as *motivation crowding-out*.[10] Third, corruption scandals and subsequent legal charges following the audits can damage the reputation of a public sector career and lead to what I call a *reputation deterrence effect*.[11]

In the last part of the paper, I provide some suggestive evidence that the perception of diminished rent-seeking opportunities and reputational concerns are likely behind the changes in talent distribution. Specifically, by leveraging finer event timing at the semester level, I find an immediate and salient effect of audits on college major enrollment following the audit announcement, even before the revelation of corruption in the audit reports. Moreover, these immediate effects are concentrated in municipalities where the audits end up detecting a high level of corruption, and in municipalities with better internet access. The evidence is consistent with the interpretation that students hold largely accurate priors regarding the level of local corruption, and the connotations of an audit are conditioned on the municipality being highly corrupt. The occurrence of an audit can alter the perceived rent-seeking opportunities in public sector careers via both channels of reduced

[9] Pro-sociality can be equated with a certain kind of intrinsic motivation where agents undertake pro-social actions for their own sake or out of a sense of moral duty (Besley and Ghatak, 2018), which is also closely tied to the idea of warm glow in the literature on charitable donations (Andreoni, 2006).

[10] Originally, motivation crowding-out refers to the phenomenon that the promise of monetary reward for completing some task can undermine intrinsic motivation for performing the task (Frey and Oberholzer-Gee, 1997; Frey and Jegen, 2001; Bénabou and Tirole, 2003, 2006). In this paper, I adopt the extensive margin equivalence of this concept (Ashraf et al., 2020) and adapt it to focus on monetary rewards in the public sector associated with corruption rents.

[11] The argument on reputation or prestige can be generalized to other self-interested career motives related to political corruption, such as re-election or promotion incentives. Existing literature has focused on politicians or bureaucrats post-selection (Iyer and Mani, 2012; Jia, 2017; Bertrand et al., 2020; Mattsson, 2022), leaving the extensive margin under-explored.

corruption and increased monitoring.[12] The implications of an audit and expected legal consequences faced by corrupt officials can also increase the perceived reputational costs of public-sector careers. Both channels decrease the attractiveness of working in the public sector for corrupt-minded students. Alternatively, the motivation crowding-out hypothesis is unlikely to account for the immediate heterogeneous effects. Assuming pro-social students aspire to join the public sector in high-corruption municipalities likely because they underestimate local corruption, they receive no negative surprise shock at the time of the audit announcement before corruption revelations.[13] Lastly, I discuss alternative explanations regarding potential changes in labor demand, either in private or public sectors, as well as in education supply. I find these alternative mechanisms are inconsistent with patterns I observe in the data.

Taken together, my findings shed light on an overlooked negative consequence of corruption on the economy: the distortion of talent allocation across public and private sector careers. When corruption is rampant, high-ability individuals can be attracted to the public sector out of rent-seeking rather than pro-social motives. The resulting misallocation of talent can have dire consequences on government performance (Finan et al., 2017; Besley et al., 2022; Fenizia, 2022; Best et al., 2023). Stamping out political corruption in turn helps improve this allocative inefficiency by re-diverting talented individuals into potentially more productive activities. All in all, anti-corruption policies have the potential to bring a halt to the "corruption-attracts-the-corrupt" vicious circle (Fisman and Golden, 2017) and enhance public performance via improved bureaucratic selection.

### 1.1 Related Literature

This paper contributes to several strands of literature. First, it adds empirical evidence to the long-standing theoretical literature on rent-seeking and talent allocation (Baumol, 1990; Murphy et al., 1991, 1993; Acemoglu, 1995).[14] Shaped by a society's reward struc-

[12] Disentangling the role of increased monitoring from that of reduced corruption is not the focus of this paper. Audits could decrease the opportunities of being corrupt and in the meantime increase the possibility of being caught (Becker and Stigler, 1974), both explanations can drive talent away from the public sector.

[13] It is important to note, however, that there could be simultaneous crowding-in of pro-social students expecting a cleaner public sector post the audits. Disentangling this channel requires information on student pro-sociality, which is rarely available in adminstrative data. Nevertheless, this implies that the net effect I observe on talent sorting is a lower bound of rent-seeking/reputation-driven students being crowded out.

[14] A closely related literature has discussed corruption and the selection of elected politicians (Caselli and Morelli, 2004; Brollo et al., 2013; Bernheim and Kartik, 2014; Martinelli, 2022). Another strand of empirical literature studies talent (mis)allocation in a variety of settings, such as labor market frictions in talent discovery (Terviö, 2009, Abebe et al., 2021), occupational choice under different income tax regimes (Lockwood et al., 2017), discrimination and minority talent in the United States (Hsieh et al., 2019), entrepreneurial talent

ture, talent allocation into rent-seeking activities (such as corruption) versus productive activities could have long-run implications on economic growth and public goods provision. However, empirical evidence on how rent-seeking opportunities causally impact the allocation of talent is scarce due to issues such as reverse causality. Notable exceptions are Brassiolo et al. (2021), in which the authors experimentally vary "corruption" opportunities in the lab among college students in Colombia,[15] as well as a concurrent work by Hong (2023), which addresses a closely related question in the context of staggered anti-corruption inspections in China during Xi's anti-corruption campaign.[16] In this paper, I overcome the identification challenge by leveraging randomized audits as a source of exogenous policy shock to (perceived) rent-seeking opportunities in the public sector, providing one of the first causal evidence of reward structure affecting the allocation of talent in a natural experiment setting.

By underscoring the role of self-selection driving talent allocation toward the state sector, this paper connects to the literature on the personnel economics of the state (Finan et al., 2017; Besley et al., 2022). An important strand of this literature studies how different selection practices of bureaucrats and frontline providers impact hiring outcomes and public performance (Dal Bó et al., 2013; Deserranno, 2019; Ashraf et al., 2020; Dahis et al., 2020; Weaver, 2021; Mocanu, 2022). A related set of papers utilizes experimental approaches to underpin patterns of the selection of honest individuals into the public sector, illustrating distinct findings in different institutional contexts (Hanna and Wang, 2017; Barfort et al., 2019; Gans-Morse, 2022). I contribute to this literature by linking *within-country* variation of reduced corruption resulting from a policy intervention to comprehensive administrative data at scale. To the best of my knowledge, this is the first paper to examine how anti-corruption affects talent allocation along both the margins of college majors and re-

in China (Bai et al., 2021; Fang et al., 2023), the allocation of immigrant (Birinci et al., 2021) and female talent (Ashraf et al., 2022; Lee, 2022) and the macroeconomy, as well as talent allocation within organizations (Haegele, 2022).

[15] In Brassiolo et al. (2021), they document negative selection by honesty into "corruptible" contracts, which persists when controlling for student GPA. However, it is not clear how well the lab-designed public versus private contracts mimic the real scenario of occupational choice. In particular, the share of students in their control group who end up choosing the "public" contract is about 32%, much lower than the baseline share of students reporting they prefer a public sector job (56%).

[16] Using representative applicant data for state organizations in China, Hong (2023) finds evidence of positive selection by integrity yet no differential selection by ability into the state sector after corruption inspections. One advantage of my setting is that the anti-corruption audits in Brazil are implemented by random lottery draws. Moreover, I utilize economy-wide data linking the labor market records in tandem with the higher education census and investigate sorting that potentially happens at the stage of choosing fields of study. Nevertheless, our distinct findings in terms of ability selection following anti-corruption efforts suggest whether corruption disproportionately attracts a society's talented individuals into the public sector can be context-specific and hinges on specific institutional features.

alized careers.

This paper also speaks to the vast literature on political corruption and the effects of anti-corruption policies.[17] Several recent papers emphasize the detrimental consequences of corruption on human capital triggered by behavioral responses to local corruption scandals (Ajzenman, 2021; Gulino and Masera, 2023). With respect to the same anti-corruption audit program in Brazil, previous literature has established that information on local corruption disclosed in the audits helps improve the selection of elected politicians (Ferraz and Finan, 2008; Cavalcanti et al., 2018), reduces subsequent corruption (Avis et al., 2018) and clientelism (Bobonis et al., 2023), boosts public hiring via patronage ties (Gonzales, 2021) and fosters local firm entry and growth (Colonnelli and Prem, 2022). While Colonnelli and Prem (2022) focuses on resource misallocation within private sector firms, this paper sheds light on an overlooked margin of allocative inefficiency: talent misallocation across the public and private sectors. In particular, I document the behavioral responses of students to top-down anti-corruption efforts, highlighting the role of self-selection in shaping bureaucratic supply and talent distribution.

Lastly, this paper relates to the literature on college major choice and subsequent career outcomes. Existing studies have documented factors such as expected labor market returns, marriage market prospects, as well as other degree-specific features or stereotypes that could alter student major choice (Wiswall and Zafar, 2015, 2021; Shu, 2016; Conlon and Patel, 2022; Ersoy and Speer, 2022), in addition to enrollment policies targeting supply-side constraints (Estevan et al., 2019). Moreover, pre-market sorting in terms of major choice could result in divergent outcomes later on in the labor market (Kirkeboen et al., 2016; Sloane et al., 2021). I contribute to this literature by zooming in on the career prospects in the public sector and providing evidence that political corruption could be another factor affecting major choices. The findings of this paper suggest that anti-corruption policies could have unintended consequences on the allocation of a society's human capital into different fields of specialization, with trickling-down effects on realized labor market outcomes.

The rest of the paper is organized as follows. Section 2 elaborates on the institutional context. Section 3 lists the data sources and provides some descriptive statistics. Section 4 presents the main results of the paper. In Sector 5, I discuss possible mechanisms at play. Finally, Section 6 concludes.

[17] Some examples are Mauro (1998, 2004), Ehrlich and Lui (1999), Olken (2007), Fisman and Miguel (2007), Treisman (2007), Barr and Serra (2010), Olken and Pande (2012), Niehaus and Sukhtankar (2013), Bobonis et al. (2016), Decarolis et al. (2020), and Detkova et al. (2021).

## 2 Institutional Background

In this section I elaborate on the institutional background of the context of focus: Brazil. I start by discussing the anti-corruption program implemented during 2003-2015 in detail. I then provide an overview of the higher education system as well as public sector career outlooks in Brazil.

### 2.1 Anti-Corruption Audits in Brazil

Brazil is a country where corruption is pervasive across different levels of government. Perception of corruption among experts and the public is also notable. As of 2021, the Corruption Perceptions Index (CPI) produced by Transparency International ranked Brazil 96 out of 180 countries regarding perceptions of an honest public sector. According to the 2018 *Latinobarometro*, more than 80% of the survey respondents believes that at least some of the civil servants are corrupt, while 30% believes almost all civil servants are involved in some acts of corruption.

On the other hand, Brazil is a large developing country with ample state capacity to enable top-down anti-corruption initiatives (Cuneo et al., 2023). In May 2003, the Lula government announced an anti-corruption audit program to be implemented by CGU (*Controladoria-Geral da União*), the main anti-corruption body in Brazil founded by the central government earlier that year to combat nationwide corruption. The program, named *Programa de Fiscalização por Sorteios Públicos*, aimed to audit municipal governments for their use of federal funds. On average, approximately 60 municipalities are selected each audit round, with replacement.[18] A unique feature of the audit program is that municipalities audited in each round are randomly selected by publicly aired lotteries. Representatives of the written press, television and radio, political parties and civil societies are invited to witness the lotteries to ensure fairness and transparency. Specifically, all non-capital municipalities with a population below 500,000 are eligible for the lottery draws.[19]

From the official website of CGU, I obtain lists of municipalities drawn for each of the 40 lotteries spanning the period 2003-2015. It is worth noting that the program was upgraded post-2015, where the selection process incorporated other forms such as targetted auditing based on municipal characteristics rather than pure randomization.[20] Figure 1 illustrates

[18] Once audited, a municipality can be audited again after some draws have elapsed, where the number of waiting draws has slightly changed over time.

[19] It is worth noting that the population threshold changed over the years, starting from 100,000 at the launch in 2003 and immediately rose to 300,000 in the lotteries drawn later that year, finally to 500,000 starting the 9th draw in April 2004 and stayed unchanged till 2014.

[20] As of 2015, the program was renamed the Inspection Program in Federative Entities

the yearly variation of the number of municipalities audited during the randomized phase only. One can see that the program was more intense during the first half of the campaign, with more lottery draws implemented and more municipalities audited during 2003-2010. As the data on higher education and the labor market are available from 2010 onwards, I limit my analysis to the second half of the program between 2011 and 2014.[21] In total, my sample consists of 6 lottery draws and 323 municipalities, out of 1,949 municipalities that are audited at least once throughout the program (in the randomized phase). Among the 323 audited municipalities, around 70% (221 municipalities) are audited for the first time and the rest have already been audited at least once prior to 2011.

Once a municipality is announced to be audited, the CGU gathers information on all federal funds transferred to the municipal government mostly in the past 3 years and issues a selection of inspection orders, each associated with a specific government project. Once these inspection orders are decided, a team of centrally-appointed auditors is sent to the municipality within days of the announcement to conduct fieldwork.[22] Importantly, auditors also meet with members of the local community in order to get direct complaints about any malfeasance. Within weeks of the inspection, a detailed report containing all irregularities found is submitted to the central CGU office in Brasilia and further distributed to other federal agencies responsible for further investigating and punishing illicit acts in the political and public spheres. Finally, for each municipality audited, a detailed written report will also be made public on the Internet and disclosed to other media sources approximately six to eight months after the audit announcement.

The CGU audit program has been studied extensively, both in terms of how the information obtained in the audits has been utilized in political campaigns and voters' selection and sanctioning of municipal politicians (Ferraz and Finan, 2008) and in terms of its effectiveness in combating subsequent corruption (Avis et al., 2018). In addition, both studies have documented the role of local media as a crucial venue for citizens to learn about audit results as well as subsequent legal action against corrupt politicians and of-

(https://www.gov.br/cgu/pt-br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos). Since then the selection has become hybrid, incorporating forms called "Census" (universal inspection) and "Vulnerability Matrix" (targeted inspection) in addition to lottery draws.

[21] I also do not consider the 2015 lottery draw as the threshold for eligible municipalities suddenly dropped from 500,000 to 100,000 in the last year. Consequently, the municipalities audited in 2015 are much smaller in terms of population size and have a higher share of the workforce in the public sector compared to the other audited cohorts during 2011-2014.

[22] At the beginning of the program all sectors are investigated for all municipalities. Beginning in August 2005 the CGU decided to target a limited number of selected sectors in larger municipalities as they receive substantially more transfers (Avis et al., 2018). For example, in the 36th lottery drawn in July 2012, only the education and social assistance sectors were audited in municipalities with more than 50,000 inhabitants, while in smaller municipalities the health sector was also audited in addition to the previous two.

ficials in their home and surrounding municipalities.[23] Although there lacks direct evidence showing that citizens learned about the audits or the audit reports, Ferraz and Finan (2008) provides both anecdotal and empirical evidence that information from the audits reached voters and was widely used during municipal elections. Moreover, Bobonis et al. (2023) shows that by reducing citizens' interaction with politicians and their knowledge of incumbents, audits also undermine clientelist relationships and perceptions of politician reciprocity. In support of findings from previous literature, in Appendix B I provide some additional analysis using survey data from *Latinobarómetro* and show suggestive evidence that audits lead to a (locally) improved perception in progress made combatting corruption in state institutions, both immediately following the audits and in the longer run. However, there's little evidence that audits altered corruption perception at the national level, which remains stably high over the years of the program (Appendix Figure B1).

### 2.2 Higher Education in Brazil

The Brazilian Higher Education System consists of both private and public universities. The public universities can be further divided into federal, state and municipal universities, which account for approximately 35.6%, 38.8% and 25.6% of 278 public institutions in the 2010 Census of Higher Education respectively. Private universities account for a much larger share of the higher education market, with a total number of 2,100 universities and about 1.72 million freshmen enrolled in the year 2010, accounting for almost 88.3% of all institutions and 78% of total new enrollments. However, public universities are tuition-free and widely perceived to be of higher quality and more prestigious. They are typically over-subscribed and more selective compared to their private counterparts.

Similar to many other countries, prospective college students in Brazil enroll in specific university-degree programs (Law degree at the University of Brasilia, for instance). In other words, students potentially take into account career paths to pursue (a lawyer, economist, engineer, teacher etc.) at the college application stage. The bachelor's degree takes about 4 to 6 years to finish, the exact time of which varies across fields of study.[24]

Before 2010 college admissions in Brazil were fully decentralized, in which students applied for degree programs months before institution-specific exams called *Vestibular*.[25]

[23] See example: http://tresfronteirasam.com.br/radio/noticias.php?noticia=1003.

[24] For example, degrees in Business Administration on average take 4 years to complete, degrees in Law or Engineering normally take 5 years, and degrees in Medicine take 6 years.

[25] Candidate students must choose their majors by the time they sign in for the Vestibular, which often only include a single stage exam where subject-specific scores are adjusted by weights depending on the

In the year 2010, the Ministry of Education of Brazil carried out centralization reforms, introducing the centralized digital platform called SISU. Federal and state universities have gradually adopted SISU, which matches students to degree programs using their uniform exam scores from the National High School Exam (ENEM). Private universities, on the other hand, can also take students' performance in ENEM into account for admissions, although the exact selection criteria may vary across institutions (it can be based on institution-specific *Vestibular* exam grades only, ENEM grades only, or a mixture of both).

The academic year in Brazil normally runs from March to December. In general, students take the ENEM test in November or December of the year when they are about to graduate high school. They can also opt to take institution-specific *Vestibular* exams, which could take place from November to January. They then have the option to use these test scores to enroll in universities in the following academic year, which normally begins in February or March. Some universities or degree programs also open up second rounds of admissions in July and August.[26]

### 2.3 Public Sector Careers in Brazil

As of the year 2018, public employees (federal, state and municipal) make up about 19% of the entire Brazilian workforce. The majority of the public sector posts are allocated via a highly competitive public contest called "*Concurso Público*", which generally consists of a screening stage of basic academic credentials as well as both written and oral exams. The *concursos* in Brazil are considered highly meritocratic and legally professional (Grindle, 2012), while previous literature has also shown that grades in civil service examinations reliably predict performance post-selection (Dahis et al., 2020). Compared to the private sector, careers in the public sector typically come together with more job security and a significant wage premium (Cavalcanti and Santos, 2021). As a consequence, public sector jobs are highly competitive with an average probability of being hired around 4% (Mocanu, 2022).

There are distinct types of contracts for public employment. Public sector workers recruited through the meritocracy-based *concursos* are called tenure-track civil servants. They acquire tenure after three years of full service, after which dismissals can only occur after a judicial ruling for misconduct such as corruption or job abandonment.[27] A

student's major choice.

[26]The exact admission dates could vary by institution, particularly for private institutions.

[27]See an example of dismissals of civil servants on the ground of corruption charges: https://agenciabrasil.ebc.com.br/en/geral/noticia/2016-07/brazil-government-dismissed-251-civil-servants-corruption.

different group of public sector workers can also be directly appointed under temporary contracts. This type of temporary contract allows more flexibility in public hiring yet in the meantime grants politicians more discretion in the bureaucratic selection process, as studies have shown that municipal bureaucrats in Brazil are closely tagged to local politicians and political turnovers (Colonnelli et al., 2020b; Akhtari et al., 2022). At the end of 2018, temporary workers accounted for only about 13.6% of the total public workforce and yet 51% of all new public contracts generated.

## 3 Data & Descriptive Statistics

To construct my main dataset I combine individual-level information for both the higher education market and the labor market in Brazil and aggregate them at the municipal level. In this section I describe the data sources, how I merge the datasets and some descriptive statistics of my final sample of focus.

### 3.1 Data Sources

I first combine several sources of individual-level data listed as follows.

**ENEM:** This is a dataset that includes the universe of students who participate in the national high school exit exam called ENEM (*Exame Nacional do Ensino Médio*), with records on subject-specific test scores along with a socioeconomic survey asking about student family background. Participation in ENEM is not mandatory yet has become increasingly prevalent post the 2009 reform, after which ENEM is required for applying to public universities as well as soliciting loans and scholarships to attend private universities. In principle, students can use the ENEM test scores from the previous academic year to apply for university only in the following year. I observe the universe of students taking the exam for the period 2009-2017, which corresponds to university enrollment seasons in 2010-2018. I also observe students' geo-location (municipality of residence) at the time of participating in the exam. Lastly, given its standardized format, I utilize the ENEM test score as a proxy for student cognitive ability in my empirical analysis.

**Census of Higher Education:** The second dataset I use is the Brazilian Census of Higher Education (*Censo da Educação Superior*). The student module contains the universe of students enrolled in higher education in Brazil, with information on specific institutions and degree programs enrolled, as well as their enrollment status (actively enrolled, dropped out or graduated). In line with ENEM, I observe the census data for the period 2010-2018. The data is considered of very high quality, as most institutions have their sys-

tems integrated with the census in real-time (Dobbin et al., 2021; Otero et al., 2021). In addition to student-degree level data, the dataset also incorporates separate modules for degrees and institutions.

**Matched Employer-Employee Data:** The third dataset I use is the Brazilian matched employer-employee dataset, known as RAIS (*Relação Anual de Informações Sociais*) and available from 2010-2018. RAIS is considered a high-quality census of the formal labor market in Brazil (Dix-Carneiro, 2014). It contains the universe of formal labor market employees, both for the private and public sector, with information on contract details, hiring and firing dates, detailed occupations and wages. Linking students from higher education to RAIS, I create a mapping of degree enrollment and student demographics to jobs in their early careers.

All the individual-level datasets listed above are available at the National Institute of Educational Studies and Research (INEP), of the Brazilian Ministry of Education.[28] Individual identifiers (pseudo social security number) are provided to merge across datasets, allowing me to trace students from high school to college and eventually to the labor market. I then aggregate the individual-level data into a municipal-level panel.

**CGU Audits:** From the official website of CGU, I collect the full list of lottery draws during the randomized phase of the program (2003-2015) as well as the list of municipalities audited in the hybrid phase (2015-2018). I focus on municipalities audited during 2011-2014 (corresponding to lotteries numbered 34-39), together with detailed audit reports associated with each audited municipality. The reports contain the total amount of federal transfers audited, the sectors audited as well as an itemized list describing each irregularity uncovered in detail, where I follow Avis et al. (2018) and further classify each irregularity listed as an act of mismanagement or corruption. I then merge the municipal-level panel with the occurrence of CGU audits and construct my main dataset.

**Other Data:** I complement the main dataset with municipal-level characteristics from two additional sources: the 2010 Population Census and a 2009 municipal survey called *Perfil dos Municípios Brasileiros*. Both of them are made publicly available by IBGE, the Brazilian Institute of Geography and Statistics. Finally, I utilize the *Latinobarómetro* survey (2001-2020) where a range of public opinions on corruption and trust in institutions are recorded for a representative sample of Brazilian municipalities every survey year.

[28]The data is accessed through authorized entry into the Secure Room of the Protected Data Access Service (Sedap) of the National Institute of Educational Studies and Research Anísio Teixeira (INEP). Data access is available upon approval of research projects. See details: https://www.gov.br/inep/pt-br/areas-de-atuacao/gestao-do-conhecimento-e-estudos-educacionais/cibec/servico-de-acesso-a-dados-protegidos-sedap/solicitacao-de-acesso.

### 3.2 Sample and Descriptive Statistics

To start with, I focus my sample on freshmen (first-year) students who enrolled in any Brazilian university during 2010-2018. I restrict the sample to students who took the ENEM exam, as I can observe the key information on geolocation (municipality of residence). For this subset of students, I observe the major(s)[29] they are enrolled in higher education and calculate the shares of each major enrollment among all students from the same municipality. The classification of majors is based on the 2018 edition of the International Standard Classification of Education Adapted for Undergraduate and Sequential Courses.[30] Appendix Figure A1 plots the yearly trend of major enrollment in higher education in Brazil. Overall, the period of 2010-2014 witnessed a slight rise in enrollment in STEM degrees, mainly driven by engineering, in tandem with a decline of interest in education degrees. Other popular degree choices such as business/law and medicine remain relatively stable over the period of study.

My final sample for the first part of the analysis consists of 6.17 million observations at the student-major level that I observe beginning the high school exit exam. For the full sample of freshmen students, I then follow a classification of *high-ability* versus *low-ability* students, based on whether their performance in the ENEM test is on the top 25% or the bottom 50% among all students from the same exam year. Lastly, all individual-level datasets are aggregated to a panel of 3,630 municipalities observed over the period 2010-2018.

For the second part of the analysis, I further trace students to RAIS and observe their career realization in the formal labor market. Given the timespan of the available data, I can observe a subsample of students enrolled in universities during 2010-2018 who appeared in the RAIS dataset in the same period. Appendix Figure A2 illustrates the share of students that are successfully traced to RAIS both by year of enrollment (Panel A) and degree enrolled (Panel B). One can see that around 8% of students enrolled in universities in 2010 entered the formal labor market within 8 years, while as a contrast less than 1% of students enrolled in 2018 are found in RAIS by the end of 2018.[31] In total, I managed to

[29] Students who are enrolled in more than one major are counted multiple times when calculating the share of majors.

[30] Commonly referred to as *Cine Brasil*. The ten broad categories (abbreviations in parentheses) are education (edu), arts and humanities (hum), social sciences (sol), business administration and law (adm), engineering (eng), natural sciences and mathematics (nat), computer science and IT (csi), medicine (hea), agriculture (agr) and services (ser).

[31] To capture a student's intention to study certain major in the first part of the analysis, I include all freshmen students in my sample regardless of whether they eventually complete the enrolled program. This also implies that students who are traced in RAIS can include college dropouts as well as students who

trace about 3.4% of the sample from the previous part of the analysis.[32] I then categorize public versus private sector workers by the contract types recorded in RAIS and calculate the share of students who end up getting their first job in the public versus the private sector among those from the same municipality.

Table 1 presents the summary statistics comparing treatment and control municipalities during the period 2011-2014. Panel A reports characteristics from the 2010 population census as well as a 2009 municipal survey, Panel B reports characteristics of the higher education market and Panel C reports characteristics of the labor market. In the main analysis, I focus on *first-audited* municipalities (defined as those audited for the first time during 2011-2014 and not audited during 2003-2010) for reasons related to the empirical strategy that I'll elaborate on in section 4.1. In comparison, the control group includes the *never-audited* municipalities, meaning that they were eligible yet never received an audit throughout 2003-2018. Importantly, I also exclude municipalities that were audited later in the hybrid phase during 2015-2018 to avoid the confounding role they play as the "later-treated".[33] The differences in the group means as well as the standard errors of these differences are reported in column 5. Out of the 16 characteristics, only one (share of urban population) is statistically significant at the 10% level. Audited municipalities also appear to have a larger public workforce compared to control municipalities, yet the differences are not statistically significant. Overall, the first-audited municipalities look very similar to never-audited municipalities across different dimensions at baseline.

## 4 Anti-Corruption Audits and Talent Allocation

The central part of my analysis investigates the impact of anti-corruption audits on student major enrollment and career allocation. By exploiting a stacked-by-event difference-in-difference research design, I show that following anti-corruption audits, high-ability

work part-time. I address this issue partly by restricting the time window of the "post" period to be at least 4 years since college enrollment, as discussed in more detail in section 4.3.

[32] Several reasons could account for the seemingly low fraction of students that are traced to RAIS. First, a large fraction of students could be yet studying their undergraduate degrees, pursuing post-graduate education, preparing for public sector examinations, or simply under unemployment spells after graduation. Second, students who end up working in the informal sector or go abroad will not appear in RAIS, which documents the universe of domestic employees in the formal sector only. Third, students who are working part-time or have past work experience are excluded from my sample as their career choice is made prior to their major choice.

[33] A caveat here is that municipalities audited in the hybrid phase are in principle selected on their propensity to be corrupt. The sample balance check in Table 1 alleviates this concern in part. In addition, I provide robustness checks when such municipalities are included in the controls in Appendix Table E3. The baseline results are similar in either case.

students are less likely to major in business/law, which is more tailored toward public sector careers. Tracking students to the labor market further demonstrates that audits are associated with fewer of these high-ability students landing their first jobs as civil servants. Together, the results illustrate a *brain drain* out of the public sector following anti-corruption audits.

### 4.1 Empirical Strategy

The objective of this paper is to examine how anti-corruption audits affect the allocation of talent within a society. I address this empirical question under a generalized difference-in-difference framework, utilizing the staggered nature of the randomized audit program across municipalities and years. Different than previous studies on CGU audits using the standard two-way fixed-effect (TWFE) regression setup (Colonnelli and Prem, 2022; Gonzales, 2021), I implement a "stacked" difference-in-difference design which estimates the treatment effects based on the comparison of units switching into treatment to not-yet-treated units in the time window of interest (Cengiz et al., 2019; Deshpande and Li, 2019; Vannutelli, 2022).[34] In Appendix Figure E1 I discuss and compare alternative estimators proposed in the literature and show that the main results are robust to alternative estimation methods. In contrast, the traditional TWFE estimator is downward biased compared to other estimators.

Specifically, I consider each "treatment cohort" as a separate sub-experiment. "Treatment cohort" $c$ includes all *first-audited* municipalities at time $c$, together with *never-audited* yet eligible municipalities (throughout 2003-2018) as "clean" controls. In the baseline specification, I consider a year as the timespan and focus on four treatment cohorts audited in 2011, 2012, 2013, and 2014.[35] I then "stack" all cohort-specific difference-in-differences and estimate the following:

$$Y_{mct} = \beta Audit_{mc} \times Post_{ct} + \delta_{mc} + \lambda_{cst} + \epsilon_{mct}, \tag{1}$$

where $Y_{mct}$ is the outcome aggregated at the municipality $m$ for treatment cohort $c$ measured at time $t$ (for instance, the share of college freshmen enrolled in engineering or the share of freshmen who end up in the public sector). $Audit_{mc}$ is the cohort-specific treat-

[34]The stacked design explicitly deals with the potential bias of the traditional TWFE estimator in the presence of treatment effect heterogeneity (Goodman-Bacon, 2021). One can refer to De Chaisemartin and d'Haultfoeuille (2020), Borusyak et al. (2021), Callaway and Sant'Anna (2021), Sun and Abraham (2021), Roth and Sant'Anna (2021) for more recent discussions in the applied econometrics literature.

[35]The same is true if semester (half-year) is the timespan, in which one can further divide the "2011" treatment cohort into "2011 winter" and "2011 summer" cohorts.

ment indicator that equals 1 for municipalities that received an audit in year $c$, while $Post_{ct}$ is the cohort-specific event time dummy that equals 1 for all periods $t$ after audit announcement in year $c$.[36] Importantly, the geolocator I use to define "exposure" to audits is the students' municipality of residence when they take the high school exit exam (before college enrollment).[37] In other words, outcomes of interest for municipality $m$ at year $t$ (such as the share of freshmen enrolled in engineering) are calculated using all students reportedly residing in that municipality at $t-1$ (the year before college enrollment).

Therefore the key parameter $\beta$ captures the average treatment effect of the local government audits.[38] $\delta_{mc}$ is the cohort-specific municipality fixed effect that absorbs any time-invariant differences in municipal characteristics. I also include cohort-specific state-by-year fixed effects $\lambda_{cst}$ to control for changes over time that affect all municipalities in the same state similarly and restrict the comparisons to municipalities within the same state. Unless otherwise specified, all regressions are weighted by the number of students reportedly residing in the corresponding municipality in the baseline year of 2010.[39] Finally, standard errors are clustered at the municipality level.

To investigate the dynamic evolution of treatment effects and test for pre-trends, I also estimate the following "stacked" event-study design:

$$Y_{mct} = \Sigma_{\tau=-k}^{k} \beta_\tau D_{ct}^{\tau} \times Audit_{mc} + \delta_{mc} + \lambda_{cst} + \epsilon_{mct}, \tag{2}$$

where same as before $Y_{mct}$ is the outcome at the municipality $m$ for treatment cohort $c$ measured at time $t$. The simple pre-post indicator is now replaced by period-specific dummies $D_{ct}^{\tau}$, spanning from $k$ periods before the audit to $k$ periods after. The pre-audit period ($k=-1$) is omitted as the reference period. In my main specification, I focus on the time window $k = \{-4, ..., 7\}$, where the unit of time is the year.[40]

[36] This corresponds to the date of the lottery (public announcement of the group of municipalities to be audited), as well as the actual audit activities since a team of auditors is sent to the municipalities within weeks of the lottery draw. The audit reports, however, are generally made public around six to eight months after the announcement of audits (see section 5.1 for a more detailed discussion). In my sample of focus (lotteries draws in 2011-2014), the median time lapse between audit announcement and audit report is 8 months and the minimum is 5 months (for the wave in 2014 only).

[37] This corresponds to the key decision-making period regarding college major choice, and I assume that students are more likely to be exposed to information regarding audits happening in the municipality where they are physically located. An alternative geo-locator would be the students' birthplace municipality. However, according to ENEM 2010, about 50% of students do not reside in their birthplace municipality at the time when they take the high school exit exam.

[38] Note that $\beta$ is a (convex) weighted average of cohort-specific average treatment effects, where the weights are determined by the number of treated units in each cohort (Gardner, 2022).

[39] I stick with the baseline year 2010 to allay the concern of endogenous weights.

[40] This is the largest time window I can observe given the outcome data and it is not a balanced panel. Note

The underlying identifying assumption is that the timing of the audit is uncorrelated with municipal outcomes (the shares of major enrollment among freshmen, for instance), conditional on the set of municipality and time fixed effects. Potential threats to identification include violation of the parallel trend assumption or anticipatory effects. Previous studies have documented the validity of the randomization assumption (Ferraz and Finan, 2008, 2011; Colonnelli and Prem, 2022), which mitigates concerns that audits were expected by institutions or prospective college students. Lotteries are drawn based on the pool of all eligible municipalities, including those that have been audited before. In the meantime, the nature of the "stacked" design requires me to focus on a slightly different sample of municipalities compared to previous studies, namely the municipalities that receive an audit for the first time as treated and those that have never received an audit (throughout 2003-2018) as control. Note that, however, within each lottery wave whether a municipality drawn for an audit has been audited previously is still random. Nevertheless, I examine the randomization pattern in the data. In column (5) of Table 1 I compare the characteristics of *first-audited* versus *never-audited* municipalities and find few differences between the two at the baseline. Overall the patterns in the data suggest that the randomization assumption is still valid for this "selected" group of treated and control municipalities. I also directly verify the parallel trend assumption by analyzing the dynamics in the $\beta_\tau$ coefficients of equation 2, as I will illustrate in the remaining part of this section.[41]

### 4.2 Audits and College Majors

Among incoming college students in Brazil, the most popular fields of study are business/law, education, medicine and engineering, accounting for 30%, 22%, 15% and 14% of total freshman enrolment in 2010 respectively.[42] The higher education census allows me to document detailed major enrollment in each category. In my main analysis, however, I highlight the comparison between changes in enrollment in business/law versus engineering following anti-corruption audits. This is largely motivated by two pieces of

that there exists a trade-off between the length of the time window and the number of treatment cohorts in the "stacked" event study. Alternatively, I could also restrict the sample to a balanced panel and focus on a shorter time window $k = \{-1, ..., 4\}$. The results on major enrollment using the balanced panel are quantitatively and qualitatively similar and are reported in Appendix Figure E3 and columns 1 and 2 of Appendix Table E3.

[41] To account for potential statistical issues related to pre-trend testing, I conduct sensitivity analysis using methods proposed in Rambachan and Roth (2023). The results are reported in Appendix Figure E2.

[42] Appendix Figure A3 provides more details regarding the shares of major enrollment at the baseline year 2010, separately by institution type and by student performance in the ENEM exam.

empirical patterns observed in major enrollment and subsequent career realization among Brazilian college freshmen in the baseline year of 2010.

First, high-ability students (defined as students with the highest 25% ENEM grades) face a clear choice between studying business/law or engineering. As illustrated in Panel A of Figure 2, business/law and engineering are the two most popular major choices among high-ability students, together occupying more than 40% of total high-ability enrollment in 2010. Low-ability students (defined as students with the lowest 50% ENEM grades), on the other hand, are more likely to study business/law rather than engineering (Panel B). Second, high-ability students majoring in business/law are more likely to become civil servants compared to their counterparts who study engineering. Panel C of Figure 2 plots the demeaned shares of high-ability students becoming civil servants for each major. On average, around 16% of high-ability students who enrolled in business/law in 2010 later appeared in the labor market as civil servants, compared to 14% from engineering, the lowest among all major fields. These numbers indicate that high-ability students in business/law are 14.3% more likely to join civil service than those studying engineering, while the same statistic for low-ability students is less drastic (around 8.9%). Notably, degrees such as education and medicine stand out as they both constitute large shares of major enrollment and exhibit a high overall propensity for careers in civil service. In Appendix C, I elaborate on why degrees such as education, which are more tailored for students aspiring to be frontline providers such as public school teachers, should be considered a special case in the discussion of bureaucratic corruption in Brazil.[43]

**Aggregate major enrollment:** Table 2 presents the main results of the effects of anti-corruption audits on freshmen major enrollment. The results are estimated from equation 1 relying on a simple set of cohort-specific municipality and state-by-year fixed effects. Pooling first-year students from all universities, results in Panel A suggest that audits significantly reduced the share of enrollment in business/law (column 1) and increased the share in engineering (column 4). In terms of magnitude, audited municipalities experience a decline of enrollment in business/law by about 1.6 p.p and an increase in engineering enrollment by 1.5 p.p. Compared to the mean shares of enrollment, these estimates correspond to a 5.3% relative decline in enrollment in business/law and a 9.4% relative increase in enrollment in engineering. A simple back-of-the-envelope calculation suggests that about 1 in 70 incoming college students switch majors, where 70 is the average num-

[43]In Appendix C, I also implement an auxiliary analysis by creating a detailed mapping from majors to careers in the public sector and offer some additional justification for the focus of the main analysis on enrollment in business/law versus engineering.

ber of freshmen across all municipalities in 2010.[44] These results suggest that audits divert freshmen students toward relatively less public-sector-oriented majors (engineering compared to business/law). As a comparison, I repeat the analysis for enrollment in other fields of study in Appendix Table A2. One can see a small positive effect of audits on enrollments in natural sciences, and no effect overall for other fields of study. In particular, the coefficients for enrollment in education and medicine are positive and negative, respectively, but neither are statistically significant.

Splitting the sample into students in private and public universities (Panels B and C of Table 2, respectively) demonstrates that the effects are mainly driven by enrollment in private universities. The results are consistent with the interpretation that public universities in Brazil tend to be highly competitive and over-subscribed. Provided that degree vacancies are fixed, total enrollment should not be affected by audits unless students are systematically driven away from public universities. Meanwhile, private universities are under-subscribed and can flexibly cater to the students' demand for degrees. As shown in Panel B, I find effects on major enrollment in amplified magnitudes for private institutions. In particular, audits significantly decreased enrollment in business/law by about 1.9 p.p (column 1) and increased enrollment in engineering by 1.9 p.p (column 4) in private universities. These estimates further translate to a 5.3% relative decline in enrollment in business/law and a 12% relative increase in enrollment in engineering.

I provide additional evidence that the effects on the shares of major enrollment reflect major-switching behavior among the same group of students, rather than the entry or exit of new students. On the extensive margin, column 1 of Panel A in Appendix Table A3 shows audits have no significant impact on the total number of freshmen enrolling in universities. In addition, columns 2-3 show that there are slightly more students entering public universities after the audits, yet the coefficients are imprecisely estimated. This suggests that changes one observes in major enrollment mostly result from the reallocation of students across different fields of study within the same type of institution (public or private) and the same municipality.[45] Nevertheless, I complement the results on enrollment shares with those on the actual number of students enrolled in Table 2. The dependent variables in columns 2 and 5 are reported in inverse hyperbolic sine transformation to take

[44] The magnitude is moderate compared to other papers that study college major choice in Brazil. For instance, by exploiting an affirmative action policy in a large public Brazilian university, Estevan et al. (2019) finds that students affected by the policy are about 10% more likely to choose competitive majors (STEM and medicine), with downstream effects on actual enrollment.

[45] An additional concern remains regarding student composition across and within types of institutions. Panel B and Panel C of Appendix Table A3 further report the effects of audits on aggregate enrollment by ability group and find no differential effects for either subgroup.

into account the extensive margin, while those in columns 3 and 6 are log-transformed.[46] Even though the point estimates are not always statistically significant, reassuringly the signs of the estimates are in line with those on enrollment shares.

Finally, Figure 3 provides visual evidence for the effect of audits on major enrollment. I now identify the dynamic effects by plotting the point estimates obtained from the estimation of equation 2. Panel A corresponds to the universe of freshmen enrollment in the pooled sample, for both the shares of enrollment in business/law and engineering. I observe little difference between the trends of audited versus never-audited municipalities prior to the audits, supporting the parallel-trend assumption. After an audit is announced to occur, there is an immediate decline in the share of enrollment in business/law for students coming from audited municipalities compared to their counterparts in never-audited municipalities while the positive effect on engineering enrollment shows up more gradually. Importantly, both gaps in enrollment shares persist over time (at least 7 years since the audit announcement), suggesting long-lasting consequences of anti-corruption efforts on shifting the distribution of local human capital among different fields of specialization.[47] Splitting the sample into private and public universities (Panels B and C) further strengthens the argument that the dynamic effects one observes in the pooled sample can be mostly attributed to enrollment in private universities.

**Student ability composition:** A major limitation with the analysis on aggregate major enrollment is that one might neglect underlying changes in within-major student composition. The concern applies to public universities in particular, as their overall higher quality attracts more high-ability students, yet their over-subscription feature could masks potential changes in student composition. To the extent that students studying different majors can be used as a proxy for the candidate pools targeted toward different career trajectories, my empirical setting offers an opportunity to examine patterns of selection among candidate pools following anti-corruption audits. In the following step, I investigate how audits affect the ability composition of students by major. I focus on cognitive ability proxied by standardized test scores taken prior to college applications.

[46]The results are similar partly because not many municipality-year bins have zero students enrolling in business/law (less than 4%) or engineering (less than 15%). Meanwhile, I recognize issues with the interpretation of average treatment effects estimated with outcomes in inverse hyperbolic sine (IHS) transformation as approximating a percentage effect, as pointed out recently by Chen and Roth (2022). I provide some robustness checks using alternative methods in Table E1 in Appendix E, and the results are similar.

[47]One possible driver behind the long-run effects (effects of audits on major enrollment of younger birth cohorts) is the altered perception of corruption opportunities for parents and other family members (Hauk and Saez-Marti, 2002; Hong, 2023) who often play a role in student major choice. A closely related explanation is that audits shift social norms and stigma associated with corruption (Corbacho et al., 2016; Stephenson, 2020) where individual preferences are further shaped by peer exposure and social interactions.

Table 3 summarizes the main results for public universities only, to highlight compositional changes even when little changes are observed in aggregate enrollment.[48] As illustrated in column 1, total enrollment in business/law saw a negligible decrease following audits at the intensive margin. However, columns 3-4 reveal non-negligible changes in terms of student composition by ability. Specifically, fewer high-ability students opt for a degree in business/law (a relative decline of 13.4% in shares), who are in turn mainly replaced by students from the second highest grade quartile (a relative increase of 12.9%). A similar pattern is observed in Panel B for engineering enrollment, where column 4 shows a 5.7% relative decrease in the share of high-ability students studying engineering. It is worth noting, however, that the decrease in high-ability shares in engineering is driven by the flooding-in of low-ability students, where the number of high-ability students actually increased. Appendix Table A5 provides a more comprehensive illustration of changes in enrollment patterns by ability group following the audits. In particular, high-ability students are leaving business/law and by and large entering into STEM fields (mathematics and natural sciences, engineering, and computer science & IT). To sum up, the results above illustrate the phenomenon of high-ability students shying away from public-sector-oriented majors, manifested by enrollment patterns in business/law. Given that major choice is nonetheless a noisy proxy for career preferences, I continue to investigate the downstream effects of audits on the labor market in the next step.

### 4.3 Audits and Realized Careers

As discussed in section 3.2, I observe labor market outcomes for the subgroup of students enrolled in higher education during 2010-2018 and later appeared RAIS before the end of 2018. In particular, I focus on the first full-time jobs they acquire in the formal labor market and define whether they work in the public or private sector by the type of labor contract.[49] I then aggregate the individual outcomes at the municipality-year level and examine the effects of audits on both aggregate career allocation and workforce composition by student ability.

[48] The results for private universities are also reported in the Appendix Table A4, where similar patterns follow but are less salient for shares of enrollment in business/law. In addition, I report the event-study plots in Appendix Figure A5.

[49] Given RAIS only records formal labor market employees, I am not able to track students who find jobs in the informal sector, which is one reason behind the sample attrition. The informal sector accounts for a substantial fraction of the Brazilian economy (Ulyssea, 2018), yet existing research on anti-corruption activities and the informal sector is limited due to data availability. Colonnelli and Prem (2022) provides suggestive evidence that the same CGU audits have limited overall impacts on switching between informal and formal activities, both in terms of employment and firm registrations.

It is important to note that whether students are exposed to audits is defined by the timing of whether they enroll in higher education after an audit occurs in their municipality of residence, which is irrespective of the locations where students go to universities or work, and is consistent throughout the entire empirical analysis. For this section, I restrict *Post* from equation 1 to be $[t+4, t+7]$ only. The reason is to focus on the sample of students that appear in RAIS at least 4 years apart from university enrollment, a hypothetical minimum timing of degree completion. By doing so, I remove, at least partially, students who dropped out in the very first years of the degree program as well as those who were already preparing to enter public sector careers prior to enrollment in higher education.[50] For this subsample of students, I first confirm that audits do have an impact on their major enrollment similar to the effects uncovered in the full sample.[51] I also recognize the caveat that the differential attrition rates by field of study can be a source of potential bias in the estimates.[52]

**Aggregate career allocation:** Table 4 summarizes the effects of anti-corruption audits on the allocation of first jobs in the labor market. Overall audits lead to more students obtaining first jobs in the private sector, while the effect on public sector career realizations is ambiguous. As one can see in column 1, a lower share of students from audited municipalities end up in the public sector compared to control municipalities, although the coefficient is not precisely estimated. Columns 2 and 3 report the estimates for the number of students working in the public sector, in IHS and log transformations separately. The former takes into account the extensive margin when in some small municipalities no students show up in the public sector in the RAIS sample, while the latter focuses on the intensive margin only. Nevertheless, the distinct estimates and large standard errors do not allow me to pin down whether audits have a positive or negative impact on the aggregate

[50] This is somewhat prevalent in Brazil as many lower-level public sector positions only require high school education. Having this group of students in the sample could be problematic, as their career path is chosen preceding the major choice. The second row in Appendix Table A1 summarizes the average time students in the 2010 enrollment cohort take from university enrollment to first appearance in RAIS, which is 3.7 years for private sector workers and 4.7 years for civil servants. The differences narrow down when I impose the hypothetical minimum degree completion time of 4 years. Nevertheless, I also report the results when no restrictions are put on the time horizon in Appendix Table E4 as well as robustness checks using alternative timespans in Appendix Table E5.

[51] Results are reporeted in Appendix Table E2.

[52] In particular, given there is higher attrition in business/law compared to engineering (Panel B of Appendix Figure A2), the control municipalities naturally witness higher attrition compared to their audited counterparts where students move from business/law to engineering. Assuming that business/law students stay longer in universities or endure longer unemployment spell as they prepare for public sector examinations, this implies that attrition leads to fewer (more) realized public (private) sector jobs being observed in the control group and can bias the estimates on aggregate career realizations towards 0.

number of students choosing public sector careers.[53] The result is plausible under the postulation that public sector positions in Brazil are highly competitive and over-subscribed. On the other hand, the opposite pattern is observed for students who choose private sector careers. Columns 5 and 6 illustrate an increase in the number of students working in the private sector, regardless of whether the extensive margin is taken into account. The coefficients suggest that, in audited municipalities approximately 23% more students end up undertaking their first job in the private sector.

Complementing the estimates in Table 4, Figure 4 further explores the dynamic effects of audits on realized careers. I now highlight both the short-run effects within 3 years since university enrollment ($[t, t+3]$) and the longer-run effects beginning $t+4$ onwards. The effects of audits on the number (IHS-transformed) of students employed in the public and private sectors are reported separately. One can see the increase in the share of students in the private sector is mainly driven by more students heading to the private sector. Specifically, a positive effect on private employment sets in almost immediately following college enrollment, coming from students who most probably haven't finished their degrees. These positive effects also persist in the longer run, at least until 7 years apart from college enrollment. For public sector employment, however, the pattern is less clear-cut. Figure 4 shows that audits do not affect public employment until around 3 years after college enrollment when a negative effect kicks in. The negative effect then begins to level out in the longer timespan, a pattern that is more salient when I focus on civil servants only.[54] All in all, the evidence presented in this section implies that anti-corruption audits lead to a rise in private-sector employment yet have an ambiguous effect on public-sector employment. However, the null effect on aggregate employment in the public sector (*quantity*) could mask underlying changes in terms of who selects into the public sector (*quality*). In the next step, I closely examine whether audits altered the composition of both private and public sector workforce, highlighting potential differential selection by student ability.

**Workforce ability composition:** Given that public sector jobs (tenure-track positions in particular) in Brazil are highly competitive and over-subscribed, they eventually become fulfilled anyway providing that there are no dramatic changes in government hiring

[53] I further divide public sector workers into tenure-track workers (or civil servants) and temporary workers and also find no differential effects (results reported in Appendix Table A6).

[54] Results for civil servants and temporary workers are reported in Appendix Figure A6. One possible explanation for the initial dip and subsequent reversal of the effect is that in audited municipalities students are taking longer time to finish their degrees and prepare to enter public sector careers. This is because audited municipalities 'produce' more engineers, as demonstrated in section 4.1, the degree of which takes longer to finish on average than business/law. In the longer run the locally produced engineers eventually enter the labor market and fulfill the positions in the public sector.

practices.[55] Echoing the analysis in the previous section on the ability composition of students in different majors, I explore whether the audits also affect the ability composition of students entering the labor market.

Table 5 summarizes the effects of audits on private and civil service workforce composition in terms of student ability.[56] Column 1 first recapitulates the effect of audits on the intensive margin of aggregate career allocation (the denominator). Columns 2-4 then report separately for the shares of students in each career category by their relative position in the ability (ENEM grade) distribution. As suggested by results in column 4 in Panel A, audits lead to a lower share of students among civil servants from the top quartile of the ability distribution (a relative decline of 52%). Columns 2-3 reveal that this relative decline is accompanied by the replacement of students from the second-highest ability group. In contrast, students who end up in the private sector are more likely to come from this high-ability sub-group (a relative increase of 20%). Alternative measure using the standardized average ENEM test scores shows similar results (column 4 of Appendix Table A7), although the coefficients are less precisely estimated.[57]

The results above on compositional changes in workforce ability highlight a selection of high-ability students out of the civil service into the private sector in audited municipalities, illustrating a public-sector brain drain. To the extent that major backgrounds can be used as a proxy for intended careers, the similar pattern of high-ability students selecting out of business/law alleviates the concern that the compositional changes among final hires are driven by the screening process. Taken together, the findings highlight that following anti-corruption efforts in their local governments, high academic achieving students in Brazil shy away from public sector careers.

[55] I discuss audits and hiring practices separately in section 5.2.

[56] Appendix Figure A7 provides the dynamic effects using the event-study specification. Panels A and C report for the entire sample, while Panels B and D restrict the sample to the less noisily observed when more than one student shows up in the corresponding group of workers. It is worth emphasizing that workforce composition is observed conditioning on having a positive number of workers. In my sample, about 54% municipality-year units have only one student appearing in RAIS as a civil servant (the sample mean [std. dev.] is 2.6 [3.8]). To alleviate the concern that the effects on ability composition are driven by the subsample where outcomes (in shares) are noisily measured, I provide robustness checks in Appendix Table E6 where alternative sample restrictions are applied.

[57] In addition, I provide results on composition in terms of other characteristics such as degree background as well as demographic and socioeconomic background (gender, parental education, and family income). The results are reported in Appendix Tables A7 and A8.

# 5 Drivers of Talent Allocation - Mechanisms

Talent allocation toward the public sector is pinned down by both the supply of and demand for talented individuals in public sector career trajectories. The presented evidence thus far suggests that anti-corruption audits shift talent allocation away from public-sector-oriented college majors as well as realized careers in the public sector. This section discusses plausible mechanisms at play. I start by laying out the main hypotheses that could be driving the behavioral responses of students to audits and provide some suggestive evidence for (against) these hypotheses. I then discuss and dismiss the alternative explanations regarding changes in education supply or labor demand.

## 5.1 Talent Supply: Perceived Career Returns

Arguably, both ability and pro-sociality (or honesty) are key dimensions that characterize the overall quality of public personnel.[58] According to the classical theory on motivation crowding-out (Bénabou and Tirole, 2003, 2006), extrinsic awards such as financial incentives could attract talented agents, whose effort is more productive, at the expense of pro-social agents who, other things equal, exert more effort (Ashraf et al., 2020). The same argument, however, may not apply to corruption rents. Conceptually, the prevalence of corruption in the public sector would attract *rent-seekers* at the expense of *pro-social* talent, assuming that corruption is perceived as entailing a negative externality to the public community. To what extent anti-corruption efforts could crowd out (in) agents in terms of ability, however, may depend on institution-specific factors such as the correlation between ability and pro-sociality in the candidate pool.

The context I study provides an opportunity to shed light on this empirical question. By utilizing standardized test scores as a proxy for student cognitive ability, my main results illustrate a brain drain from public sector careers. However, the lack of measures on pro-sociality or honesty from administrative data makes it challenging to pin down exactly why anti-corruption audits might crowd out high-ability students. On one hand, audits could lead to a perception of reduced corruption, deterring high-ability students

[58] An extensive literature has elaborated on the important role of these two traits, together and respectively. The literature stems from discussions on what makes a good elected politician (Caselli and Morelli, 2004), to more recent papers on the selection of frontline providers and the delivery of public services spanning various contexts (Gregg et al., 2011; Dal Bó et al., 2013; Deserranno, 2019; Ashraf et al., 2020; Khan, 2020). Furthermore, Dahis et al. (2020), Fenizia (2022), and Best et al. (2023) show that bureaucratic capability is a reliable predictor for the performance of bureaucrats in office (the intensive margin). In particular, Dahis et al. (2020) uses scores from the public sector entrance exams as a proxy for cognitive ability focusing on state judges in the context of Brazil.

who join the public sector for rent extraction. I refer to this channel as *diminshed rent-seeking*. On the other hand, through the revelation of local corruption and subsequent legal charges against corrupt officials, audits may alter non-pecuniary incentives to join the public sector. These incentives can be further classified as pro-social motivation or reputation concerns (Besley et al., 2022), based on which I separately label the other two channels as *motivation crowding-out* and *reputation deterrence effect*.

Previous studies have shown that the CGU audits are effective at curbing local corruption (Avis et al., 2018), and that both voters' initial priors and actual information revealed on local corruption matter in the selection and sanctioning of municipal politicians (Ferraz and Finan, 2008). Considering that I focus on the latter stage of the CGU audit program from 2011 onwards, it is plausible that citizens have further updated their priors regarding local corruption as the program unfolds across the nation, even for those from municipalities that haven't been directly audited.[59] Using data from the *Latinobarómetro* survey, in Appendix B I provide suggestive evidence that audits are associated with a (locally) improved perception of progress made combatting corruption, even though audits do not seem to alter the overall high corruption perception across the nation. However, as survey measures on perceptions of rent-seeking versus non-pecuniary returns are unavailable, I cannot directly estimate the effects of audits on these perceptions per se. While I am not able to attribute all the effects to one particular channel, two pieces of indirect evidence support the *diminished rent-seeking* and *reputation deterrence effect* hypotheses.

**Immediate effects following audit announcement:** I begin by leveraging a finer timing for the baseline effects of audits on major enrollment, as illustrated by Figure 5. A time period $t$ is now a semester (half-year), and I maintain a balanced sample of municipalities observed between $[t-3, t+7]$ in this part of the analysis. The overall takeaway remains unchanged: following the audits there's a decline in the share of freshmen enrollment in business/law and an increase in engineering. A key new message conveyed in Figure 5 is that the effects kick in immediately in the semester of the audit announcement ($t+0$). Note that at this stage, audit reports containing any corruption act have not been released to the public yet, since they normally become available six to eight months post-announcement. As the information channel is shut down at this short interval, the immediate response suggests that students form priors regarding local corruption and/or

[59] In fact, Colonnelli and Prem (2022) documents large spillover effects of audits on local economic activities, which they interpret as the deterrence impact of audits in nearby municipalities by raising the salience and threat of future audits. I follow their approach and consider a municipality as "indirectly" exposed to audits if a nearby municipality in the same microregion receives an audit. I uncover spillover effects of similar magnitude for major enrollment, the results on which are reported in Appendix Table A9.

the implications of the audits on local corruption possibly based on corruption revealed in the first years of the CGU program for other municipalities. Upon announcement of the new set of municipalities to be audited, students foresee that their local government would be subject to central monitoring. This could imply that corruption opportunities will decrease in the future for corruption-prone students on one hand, or raise the salience of social norms concerning political corruption and the reputation of public-sector careers on the other. The effect is short-lived as the coefficients approach 0 at $t+1$, but bounce back in the medium run, the period when information on local corruption is made public and corrupt politicians and bureaucrats start facing legal consequences.

**Heterogeneity by detected corruption and local media:** I further examine the effects at refined timing looking at effect heterogeneity by the level of corruption uncovered, as illustrated in Panels A and B of Figure 6 (corresponding table estimates in Appendix Tables A10 and A11). Specifically, I utilize detailed information in the audit reports regarding irregularities detected and label an audited municipality as "high corruption" if the share of inspection orders with irregularities labeled as severe corruption is above the median among all *first-audited* municipalities during 2011-2014.[60] Panels A and B illustrate clear patterns that whether audits affect freshmen major enrollment is conditioned on whether the audits are effective at detecting a high level of corruption. Moreover, the immediate effects at $t+0$ and the "bounce-back" effects are more starkly exhibited in municipalities with high corruption uncovered, while the lack of reactions (positive surprise effects) from low corruption municipalities are in line with the interpretation that students hold largely correct priors regarding corruption level in their municipality.

Furthermore, a similar heterogeneous effect is uncovered for whether internet providers are located in the municipality (Panels C and D of Figure 6), suggesting the key role of the internet in disseminating information regarding audit announcements and reports.[61] I also find interesting disparities between the roles of traditional and modern means of media. Unlike previous literature that has highlighted the role of local radio (Ferraz and Finan, 2008; Avis et al., 2018) in disseminating news on audits and corruption, I do not find strong heterogeneous effects by the presence of a local AM radio station on major

[60] Irregularities are grouped into three categories: error in documents (*falha formal*), intermediate error (*falha média*), and severe error (*falha grave*), where severe error cases tend to capture unambiguous cases of corruption (Avis et al., 2018; Gonzales, 2021).

[61] One caveat with this heterogeneity analysis is that while audits are random, the amount of corruption detected or local media presence is not. Colonnelli and Prem (2022) shows that replacing actual corruption with predicted corruption using machine learning (Colonnelli et al., 2020a) also yields a large degree of heterogeneity across municipalities. In addition, the lack of pre-trends in the event-study plots of Figure 6 alleviates this concern.

enrollment (Appendix Figure A8). The probable explanation is that I focus on the later stage of the audit campaign when the role of traditional media such as the radio has been dwarfed by the emergence of modern means of media,[62] and a distinct sample of incoming college students, for whom the internet is more likely to be the main source of news consumption compared to voters in general. On one hand, students from municipalities with better media access are more likely to be informed of the CGU audit program and learn from previous audits, reinforcing the narrative that they form priors regarding the implications of audits on local corruption. On the other hand, the immediate effect one observes at $t + 0$ where internet providers are located strengthens the argument that it is the foreseeable reduced corruption rents or increased reputation costs tagged to the audit announcement itself that affects students' decision-making.

**Further discussions:** The alternative hypothesis on intrinsically motivated talent being crowded out seems unlikely to explain the evidence presented in Figures 5 and 6, for two reasons. First, descriptive evidence in Appendix B shows corruption perception is widespread in Brazil and the audit program did not shift this perception at the national level, suggesting the institutional environment in Brazil was closer to a "rent-seeking equilibrium" where corruption is attracting the corrupt (Acemoglu, 1995; Hanna and Wang, 2017). Second and more importantly, the heterogeneous effects suggest that by and large students hold correct priors on local corruption. Intrinsically motivated students would likely abstain from public sector careers in high-corruption places to begin with.[63] Nonetheless, the immediate effect following the audit announcements in high-corruption municipalities (but before the revelation of actual corruption) provides evidence against the *crowding-out* story, as there exists no information update (negative surprise shock) at the time of audit announcements. I cannot, however, rule out the possibility that more intrinsically motivated individuals are in turn attracted to the public sector following the anti-corruption audits as corruption is effectively reduced, that is, a *motivation crowding-in* channel.[64] In that case, the effects I observe on talent reallocation are the net effects of corruption- or reputation-prone talent being driven away while pro-social talent being

[62] According to the *Perfil dos Municípios Brasileiros* (see Panel A of Table 1 for summary statistics), the share of municipalities reportedly having a local AM radio station barely changed from 2001 (20.6%) to 2009 (21.3%), while the share of municipalities with an internet provider more than doubled (from 22.7% to 55.6%) during this period.

[63] Alternatively, intrinsically motivated students might join a corrupt public sector aspiring to make a change for the better. This argument more likely applies to students aspiring for federal positions than those who settle for a job in the municipal or state bureaucracy. The former accounts for a very small fraction of my sample given that federal-level positions are much more competitive and require years of exam preparation.

[64] The finding of positive selection of integrity (*honesty gain* at no expense of *brain drain*) into state organization in Hong (2023), albeit in a different context, also echoes this explanation.

attracted toward the public sector, where the former appears to dominate.[65]

Finally, I also find evidence suggesting the role of issue salience in amplifying students' behavioral responses to audits. First, the immediate effect and the "bounce-back" effect in Figures 5 and 6 correspond to key event timings of audits: audit announcements and release of audit reports. The news on audits, likely disseminated through local media, then raises the salience of local corruption among the student population. Second, the decomposition of group-specific treatment effects shows that the effect is stronger in years when major corruption scandals were revealed in Brazil. Using the estimator proposed in Callaway and Sant'Anna (2021), Appendix Figure A9 shows the treatment effects are the strongest for audit draws from the years 2011 and 2014 when political corruption was in the spotlight due to large-scale and high-profile corruption scandals.[66]

If the diminished rent-seeking channel is the main driver of the effects of audits on talent reallocation, rent-seeking motives then play a primary role in attracting talented students to the public sector in Brazil in the absence of policy interventions. This further implies that rampant corruption can distort the allocation of human capital toward unproductive activities other than its massive direct costs on the economy. It is less clear, however, what are the implications of reputation deterrence on the quality of the final hires. To the extent that reputation and other career concerns can be characterized as self-interested motives, students who are primarily concerned about reputation are more likely to be opportunistic agents rather than agents with strong public sector motivation.[67] Overall, the findings in this section suggest that when corruption is rampant, talent can be "misallocated" across the public and private sectors.

### 5.2 General Equilibrium Responses

As emphasized in section 4.1, students are considered exposed to audits if their municipality of residence receives an anti-corruption audit the year or before the year they enroll in college. By defining treatment status based on "origin", I partially abstract away from labor demand factors students face in municipalities where they pursue higher education

[65] It is important to emphasize that crowding-in is unlikely to drive the negative sorting by ability in a setting when selection is meritocratic. That is, the potential crowding-in of low-ability students can only crowd out competitors from the lower end of the ability distribution, rather than the high-ability students.

[66] 2011 is the year when Brazil's first female president, Dilma Rousseff, came into power, followed immediately by corruption scandals of several high-profile officials and nationwide anti-corruption protests. 2014 marks the beginning of Operation Car Wash, a landmark anti-corruption probe uncovering a massive corruption scheme in the Brazilian federal government.

[67] See the literature on the potential role of collective reputation in contributing to the self-reinforcing nature of corruption (Andvig and Moene, 1990; Tirole, 1996; Mauro, 2004).

and early careers. However, general equilibrium responses nonetheless play a confounding role as both the higher education market and the formal labor market in Brazil tend to be somewhat localized. In particular, in the baseline 2010 enrollment cohort, almost 40% (70%) of all students went to college in the same municipality (state) as their place of residence while about 53% (91%) found their first jobs in the same municipality (state).[68] In this section, I directly test how audits affect *education supply* in the higher education market as well as *labor demand* in the formal labor market. I show that these alternative mechanisms are inconsistent with patterns I observe from the student sample.

**Degree vacancies in higher education:** One alternative explanation behind the effects of audits on major enrollment is that audits may affect the supply of university degree vacancies. This may happen because, on the one hand, audits (especially the ones targeting the education sector) can affect the allocation of fiscal transfers to the education sector, and, on the other, audits may induce systematic reforms in universities causing personnel turnover (Gonzales, 2021). The concern, however, applies mainly to public institutions while private institutions in Brazil function distinctively and cater mainly to the market demand for degrees. I directly test this hypothesis of whether audits impact the number of degree vacancies offered by institutions. Table 6 summarizes the results, for private universities (Panel A) and public universities (Panel B) separately. Note that I estimate the same regression as equation 1, but focus on the sample of universities instead of students. As universities tend to be located only in relatively larger and more urbanized municipalities, I end up with a much smaller sample of municipalities. I also maintain a balanced panel in this analysis, eliminating periods where I have very few observations. Nevertheless, the results suggest that following the audits private universities start offering fewer vacancies in business/law and more in engineering. In the meantime, one does not see the same reactions from public institutions (the coefficients are not precisely estimated). These results are consistent with the interpretation that changes in degrees supplied by private universities reflect changes in student demand. Moreover, if public universities do start offering fewer degree programs in business/law post the audits, one would expect business/law degrees to become more competitive, where the marginal student enrolling in business/law would have a higher grade. Instead, I observe the opposite from the student sample: within business/law, high-grade students are being replaced by their lower-grade counterparts.

**Outside option in the private sector:** Another possible mechanism behind the effects

[68] This share is slightly higher for private sector workers (57%) than for civil servants (54%). I restrict to the 2010 cohort as I find suggestive evidence of selective migration out of audited municipalities for both civil servants and private sector workers (see more details on audits and out-migration in Appendix D).

is that audits affect labor demand and subsequently the attractiveness of working in the private sector. Existing studies have shown that by reducing resource misallocation across firms caused by corruption, audits boost firm activities in government-dependent sectors (Colonnelli and Prem, 2022; Colonnelli et al., 2022). I follow the same line of thought under the stacked-by-event specification but focus on the sample of firms instead. In particular, I examine total contracts of "full-time first-hires" generated as an outcome that is closely related to the demand for young talent in my setting. I also restrict *Post* as the period between $[t+4, t+7]$ to be consistent with the main analysis in section 4.3. Table 7 summarizes the results. In Panel A the sample includes years 2010-2018, while in Panel B I further include the RAIS sample dating back to 2002 to maximize power, effectively incorporating all audit waves between 2003 and 2014. Overall I find little evidence of audits increasing aggregate first hires among private sector firms.[69] This is in contrast with the surge in private employment I observe from the student sample as illustrated in Table 4, suggesting labor demand from firms directly exposed to audits is not the main driver of talent reallocation.[70] Instead, the pattern of more students entering the private sector observed in my student sample is more likely to be the downstream outcome of students switching into private-sector-oriented majors (e.g. STEM), at which stage labor demand factors are less of a consideration due to the short timespan.

**Patronage hiring in the public sector:** A similar concern applies to hiring in public sector organizations, particularly in a setting where patronage hiring[71] is prevalent (Colonnelli et al., 2020b) and bureaucratic turnover is closely tagged to political turnover (Akhtari et al., 2022). In a related paper studying the same audits, Gonzales (2021) shows evidence of increased municipal hires concentrated in high-corrupt municipalities as local politicians hire via their patronage networks to compensate for the potential loss of electoral support. Indeed, the results in column 1 of Table 7 demonstrate an increase in new contracts generated in civil service following the audits, which becomes statistically significant after incorporating previous audit waves. Although I do not find evidence of audits

[69] These results seem at odds with findings in existing literature suggesting that audits lead to a boost in the private economy. In particular, Colonnelli and Prem (2022) shows the same CGU audits not only foster firm creation but also lead to more employment and hires in the private sector in the 6-year window, even though the effect on hires is not statistically significant (Online Appendix Table A16). However, Colonnelli and Prem (2022) also uncover large heterogeneous effects of audits across firms: incumbent firms in government-dependent sectors grow the most while politically connected firms suffer. Therefore, it is not clear to what extent the boost in private firm activities translates to labor demand for students.

[70] Given Colonnelli and Prem (2022) uncovers large spillover effects on firm activities, however, it is most likely that audits have improved the career outlooks for young talent in nearby municipalities and the local labor markets (micro-regions in Brazil).

[71] Defined as a quid pro quo relationship between the party in power and its political supporters in which public jobs are used as a reward and exchanged for political support (Weingrod, 1968).

increasing aggregating career realizations in the public sector among my student sample, a remaining concern is whether patronage hiring plays a primary role in the outflow of high-ability students from the public sector following the audits (Colonnelli et al., 2020b). Two pieces of evidence suggest this is not the case. First, I observe a negative sorting of student ability into business/law majors at the college enrollment stage, when the hiring and screening process is not yet relevant. Second, temporary public workers in Brazil are potentially more susceptible to patronage hiring, for whom I also observe an increase in hiring, albeit noisily estimated. However, Panel A of Appendix Table A12 shows that in contrast to the selection of high-ability individuals out of civil service, such a pattern is not observed for temporary workers. In fact, students who become temporary public sector workers are somewhat positively selected on ability.

## 6 Conclusion

This paper provides one of the first empirical evidence that combating corruption drives a society's talented individuals away from careers in the public sector. I establish causality by leveraging the randomized nature of the CGU audit program in Brazil. I find that following the audits, high academic achieving students shy away from public-sector career paths both in terms of college majors and realized occupations. Additional evidence suggests that the perception of diminished rent-seeking returns and increased reputation costs associated with public sector careers are behind the effects of talent sorting. In sum, the results of this paper highlight yet another understudied negative consequence of corruption on the economy via the distortion of a society's talent allocation toward rent-seeking activities in the public sector. Anti-corruption initiatives in turn have the potential to help improve these allocative inefficiencies by diverting capable "rent-seekers" into potentially more productive activities, and in the meantime boost government performance through improved bureaucratic selection.

One of the main takeaways of this paper is the potential role of self-selection in pinning down the quality of public employees, which tends to be understated as there is generally excessive demand for public sector positions in the developing world. The findings of this paper suggest that even in contexts where the selection of public personnel is meritocratic and highly competitive, sorting in the candidate pool can eventually translate to the quality of the final hires. My findings thus add to a growing body of work on bureaucratic selection (Finan et al., 2017; Lim and Snyder Jr, 2021; Besley et al., 2022; Mocanu, 2022) that has put more emphasis on the screening side of public hiring. The context

of Brazil provides a unique set of policy experiments and comprehensive administrative data, although it is undeniable that the top-down approach to combat rampant corruption is rooted in a certain level of state capacity (Cuneo et al., 2023). Exploring how corruption affects talent allocation in other contexts where similar anti-corruption drives have taken place (such as China or Costa Rica) can help understand to what extent my finding on ability selection is context-specific, and what could be the relevant institutional features that drive the patterns of selection.

There are several promising avenues for future research. A key element missing from the administrative data is a measure of students' honesty or pro-sociality, the importance of which has been highlighted in the literature using experiments to study the ability and pro-sociality trade-off in the hiring of frontline providers (Dal Bó et al., 2013; Deserranno, 2019; Ashraf et al., 2020). Understanding selection by pro-sociality can help further pin down the mechanisms, particularly regarding the extent to which there could be crowding-in of pro-social talent replacing the rent-seekers in the public sector. One promising research agenda is to incorporate survey design tools to elicit key traits unavailable in administrative data, such as pro-sociality and risk aversion. It would also be interesting to understand whether the effects are driven by students from bureaucratic families and understand how anti-corruption efforts affect the intergenerational transmission of public sector jobs. Finally, it is crucial to probe into the potential productivity consequences of altered talent allocation resulting from reduced corruption, so as to gauge the overall impacts of the anti-corruption audits in addition to its direct impacts on economic activities. One potential intermediate step is to zoom in on specific occupation choices of high-ability students in the private sector (e.g. whether they become entrepreneurs or enter NGOs). Even though this paper focuses primarily on the selection margin, the use of economy-wide data proves a promising first step to understanding the implications of a society's talent allocation on both productivity growth and the efficient delivery of public services.

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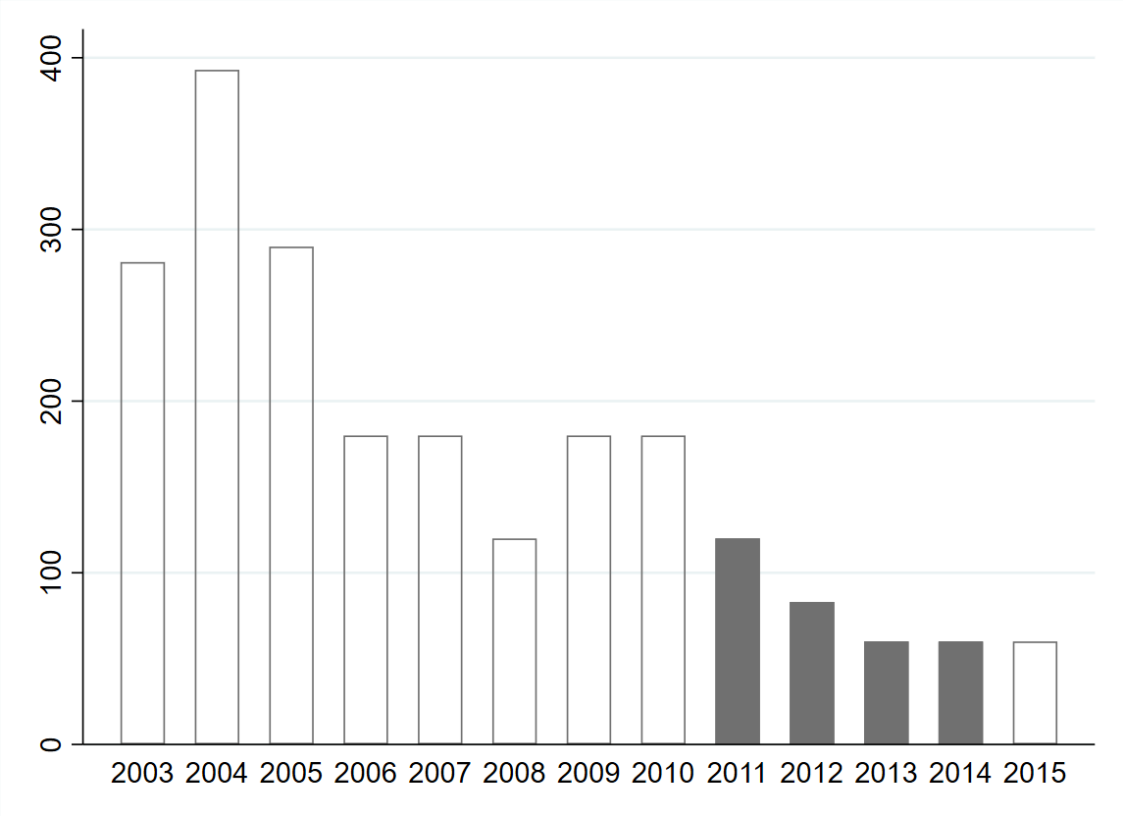
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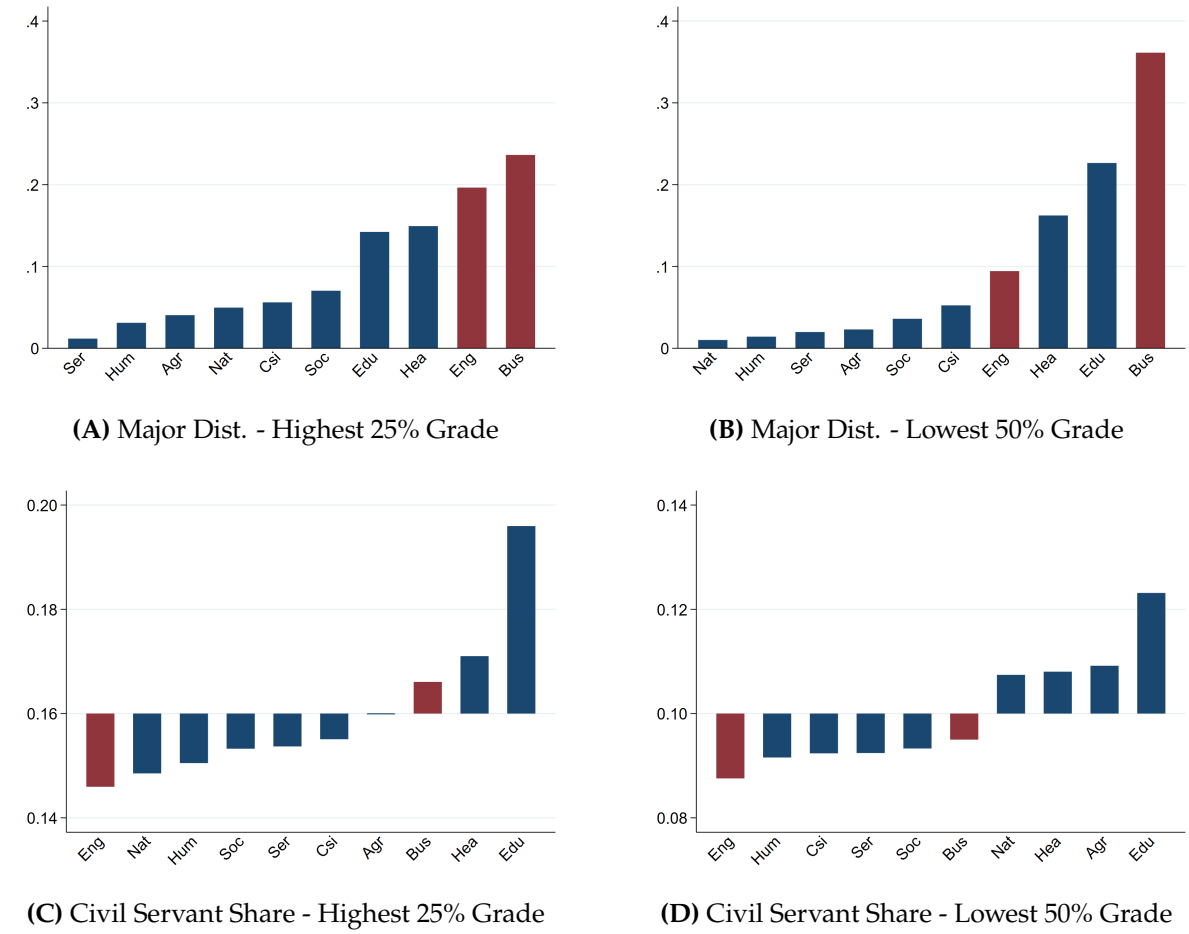
## Tables and Figures

**Figure 1:** Number of Municipalities Audited Every Year




*Notes:* This figure shows the yearly variation of the number of municipalities drawn for audits throughout the randomization phase of the program (2003-2015). The shaded bars (2011-2014) highlight the period this paper focuses on.

**Figure 2:** Patterns in Major Enrollment and Subsequent Careers




**(A)** Major Dist. - Highest 25% Grade

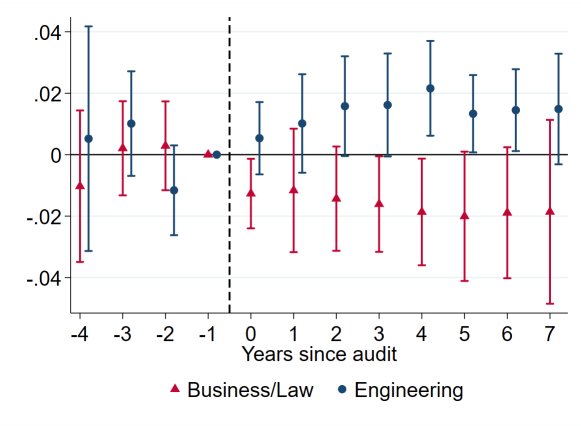
**(B)** Major Dist. - Lowest 50% Grade

**(C)** Civil Servant Share - Highest 25% Grade

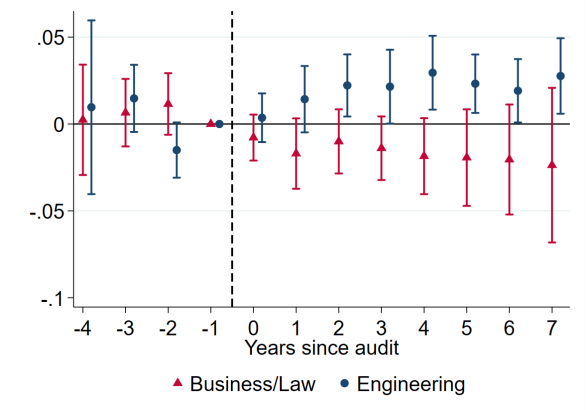
**(D)** Civil Servant Share - Lowest 50% Grade

*Notes:* This figure illustrates some descriptive patterns in terms of major enrollment and subsequent career realizations in civil service, for the baseline group of freshmen in the 2010 enrollment cohort. Panels A and B display the shares of major enrollment, separately for high-grade (highest 25%) students and low-grade (lowest 50%) students. Panels C and D report the (demeaned) shares of students finding first jobs as civil servants for each major, restricting to the sub-sample of students enrolled in higher education in the baseline year 2010 and traced to RAIS as explained in section 3.2.

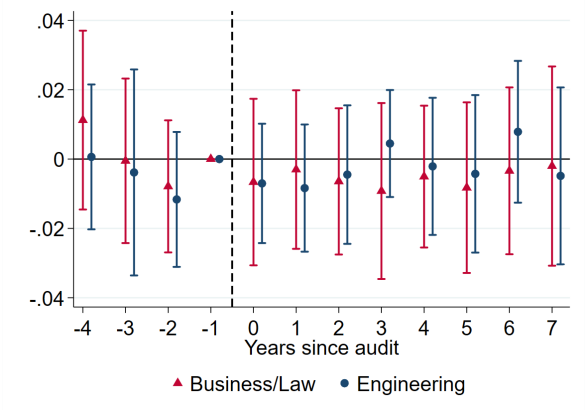
**Figure 3:** Audits and Shares of Major Enrollment




**(A)** All Universities



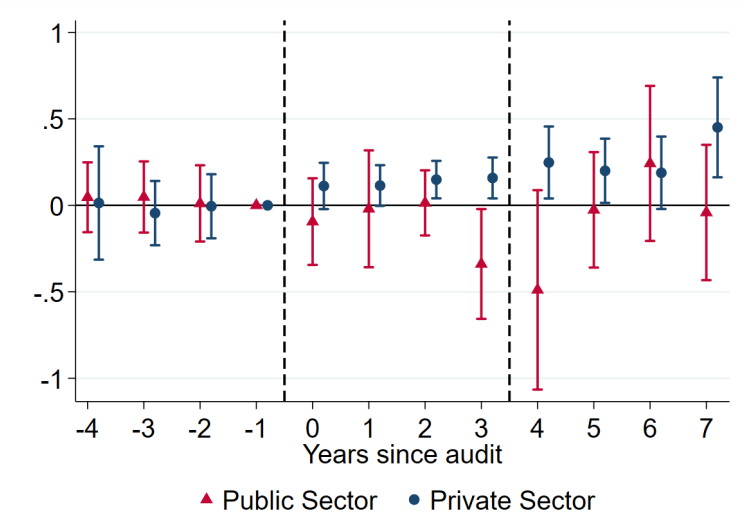

**(B)** Private Universities




**(C)** Public Universities

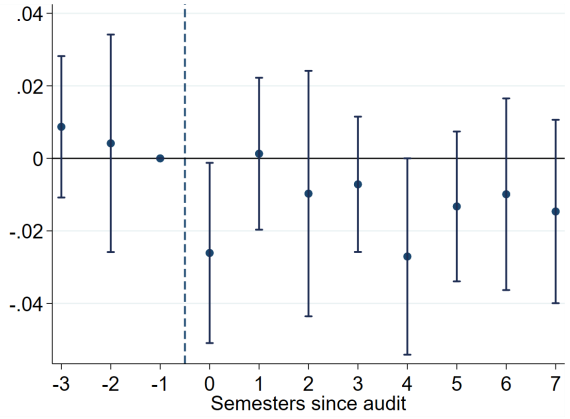
*Notes:* This figure reports coefficients obtained from the estimation of equation 2 (corresponding to Table 2), where the estimated differences between treatment and control municipalities are allowed to vary for each year around the audit. Panel A includes the sample pooling all private and public university students. Panel B and C report separately for private universities and public universities. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

**Figure 4:** Audits and Realized Careers

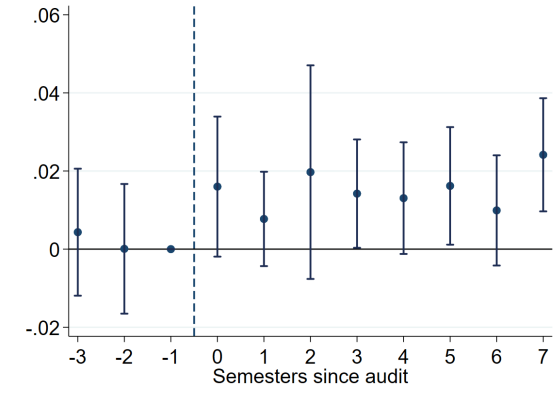



*Notes:* This figure reports coefficients obtained from the estimation of equation 2 (corresponding to estimates in Table 4), where the estimated differences between treatment and control municipalities are allowed to vary for each year around the audit. Numbers (IHS-transformed) of all students that are traced to the public and private sectors are reported separately. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

**Figure 5:** Audits and Shares of Major Enrollment - Time is Semester



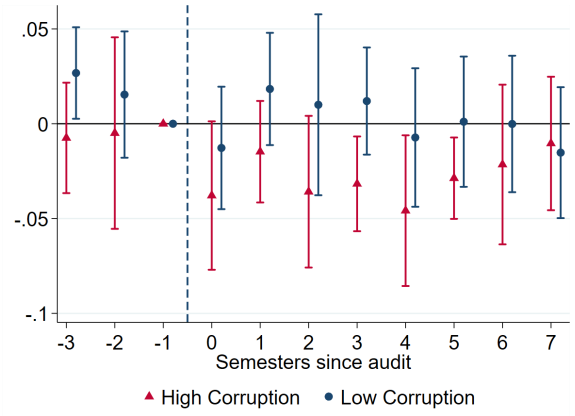

**(A)** Business and Law



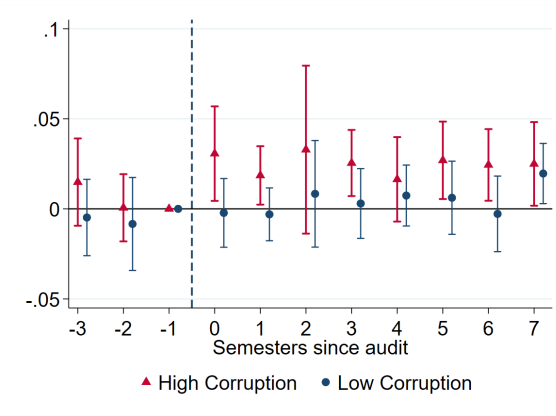

**(B)** Engineering

*Notes:* This figure reports coefficients obtained from the estimation of equation 2 for the sample pooling public and private universities, where time is now a semester instead of a year. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

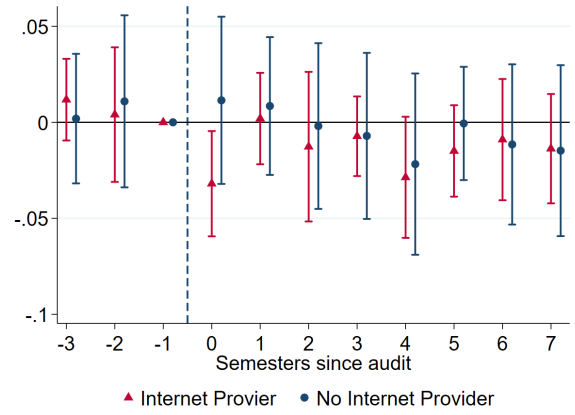
**Figure 6:** Audits and Shares of Major Enrollment - Heterogeneity



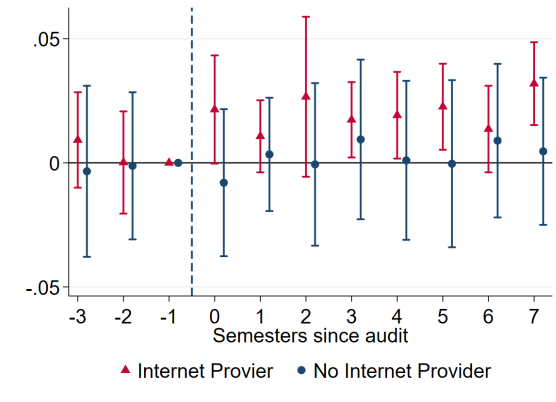

**(A)** Business/Law - Detected Corruption




**(B)** Engineering - Detected Corruption




**(C)** Business/Law - Local Media




**(D)** Engineering - Local Media

*Notes:* This figure reports coefficients obtained from the estimation of equation 2 but separately for municipalities uncovered with high versus low corruption (Panels A and B) and for municipalities with or without internet providers (Panels C and D), where time is a semester. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

**Table 1:** Mean Comparisons Between First-Audited and Never-Audited Municipalities

| | Control | | Treatment | | Difference |
|---|---|---|---|---|---|
| | Mean | Std. Dev. | Mean | Std. Dev. | |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Pre-Trement Municipal Characterisitcs** | | | | | |
| Population (logs) | 10.02 | 0.60 | 10.09 | 0.62 | 0.03 |
| | | | | | (0.04) |
| Share urban | 0.63 | 0.22 | 0.64 | 0.20 | 0.02* |
| | | | | | (0.01) |
| Share literate | 0.78 | 0.09 | 0.77 | 0.09 | 0.00 |
| | | | | | (0.00) |
| Share of population | | | | | |
| with a college degree | 0.04 | 0.02 | 0.03 | 0.02 | 0.00 |
| | | | | | (0.00) |
| Has AM radio 2009 | 0.19 | 0.39 | 0.20 | 0.40 | 0.02 |
| | | | | | (0.03) |
| Has internet provider 2009 | 0.54 | 0.50 | 0.59 | 0.49 | 0.03 |
| | | | | | (0.03) |
| **Panel B: Pre-Treatment Higher Education Market Charateristics** | | | | | |
| Num. of freshmen (logs) | 3.10 | 1.39 | 3.15 | 1.41 | 0.02 |
| | | | | | (0.09) |
| Share female | 0.49 | 0.02 | 0.49 | 0.02 | -0.00 |
| | | | | | (0.00) |
| Share in public universities | 0.34 | 0.27 | 0.35 | 0.27 | 0.01 |
| | | | | | (0.02) |
| Share enrolled in business/law | 0.27 | 0.17 | 0.27 | 0.18 | 0.01 |
| | | | | | (0.01) |
| Share enrolled in engineering | 0.11 | 0.12 | 0.10 | 0.10 | -0.00 |
| | | | | | (0.01) |
| Share enrolled in education | 0.28 | 0.21 | 0.30 | 0.21 | 0.00 |
| | | | | | (0.01) |
| Share enrolled in health | 0.17 | 0.15 | 0.18 | 0.15 | 0.00 |
| | | | | | (0.01) |
| **Panel C: Pre-Treatment Labor Market Charateristics** | | | | | |
| Num. of public sector workers (logs) | 5.75 | 1.22 | 5.96 | 1.11 | 0.05 |
| | | | | | (0.07) |
| Share of workers in public sector | 0.42 | 0.29 | 0.47 | 0.30 | 0.01 |
| | | | | | (0.02) |
| Share of workers in civil service | 0.34 | 0.26 | 0.37 | 0.27 | 0.01 |
| | | | | | (0.02) |
| Observations | 3,409 | | 221 | | |

*Notes*: This table shows means and standard deviations of various characteristics of treated and control municipalities. The treatment group contains first-audited municipalities during 2011-2014 while the control group includes never-audited yet eligible municipalities. Characteristics in Panel A are based on information from the 2010 Brazilian Population Census and the 2009 municipal survey called *Perfil dos Municípios Brasileiros*. Characteristics in Panel B are based on information from the 2010 Census of Higher Education and characteristics in Panel C are from the 2010 RAIS dataset. In Column (5) the differences and robust standard errors (in parenthesis) are based on a regression that includes state fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table 2:** Effect of Anti-Corruption Audits on Major Enrollment

| | Freshmen Major Enrollment | | | | | |
|---|---|---|---|---|---|---|
| | Business/Law | | | Engineering | | |
| | Share | Num. (asinh) | Num. (log) | Share | Num. (asinh) | Num. (log) |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A: All Universities** | | | | | | |
| Audit × Post | -0.016*** | -0.033 | -0.035 | 0.015** | 0.085 | 0.091* |
| | (0.005) | (0.029) | (0.029) | (0.007) | (0.054) | (0.055) |
| $R^2$ | 0.58 | 0.98 | 0.98 | 0.73 | 0.96 | 0.97 |
| *Mean Dep. Var.* | 0.30 | 5.35 | 4.67 | 0.16 | 4.64 | 4.04 |
| *SD Dep. Var.* | 0.08 | 1.68 | 1.66 | 0.08 | 1.83 | 1.74 |
| Observations | 155,290 | 155,290 | 150,043 | 155,290 | 155,290 | 132,374 |
| Num. of Clusters | 3,693 | 3,693 | 3,692 | 3,693 | 3,693 | 3,630 |
| **Panel B: Private Universities** | | | | | | |
| Audit × Post | -0.019*** | -0.040 | -0.041 | 0.019** | 0.135** | 0.143** |
| | (0.007) | (0.031) | (0.031) | (0.008) | (0.054) | (0.056) |
| $R^2$ | 0.51 | 0.97 | 0.98 | 0.68 | 0.96 | 0.96 |
| *Mean Dep. Var.* | 0.36 | 5.27 | 4.59 | 0.16 | 4.34 | 3.78 |
| *SD Dep. Var.* | 0.09 | 1.69 | 1.66 | 0.08 | 1.84 | 1.71 |
| Observations | 154,419 | 154,419 | 147,694 | 154,419 | 154,419 | 123,124 |
| Num. of Clusters | 3,693 | 3,693 | 3,691 | 3,693 | 3,693 | 3,596 |
| **Panel C: Public Universities** | | | | | | |
| Audit × Post | -0.005 | -0.029 | 0.014 | 0.000 | -0.017 | -0.005 |
| | (0.008) | (0.094) | (0.093) | (0.006) | (0.074) | (0.075) |
| $R^2$ | 0.52 | 0.91 | 0.91 | 0.64 | 0.94 | 0.94 |
| *Mean Dep. Var.* | 0.13 | 3.13 | 2.79 | 0.17 | 3.38 | 3.03 |
| *SD Dep. Var.* | 0.11 | 1.89 | 1.67 | 0.13 | 1.95 | 1.72 |
| Observations | 143,667 | 143,667 | 80,317 | 143,667 | 143,667 | 86,176 |
| Num. of Clusters | 3,684 | 3,684 | 2,990 | 3,684 | 3,684 | 3,018 |
| Muni. × Cohort FE | X | X | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the shares of freshmen enrolled in business and law (column 1) versus engineering (column 4), the corresponding numbers in inverse hyperbolic sine transformation (columns 2 and 5) as well as in log transformation (columns 3 and 6). The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the year is after the year of interest. Panel A includes the sample of all student pooling public and private universities. Panel B and Panel C report the estimates for private and public university students separately. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table 3:** Effect of Audits on Student Composition (Public Uni.) by Ability

| | Total Num. (log) | Shares by Quartile of ENEM Grades | | |
|---|---|---|---|---|
| | | Lowest 50% | Second Highest 25% | Highest 25% |
| | (1) | (2) | (3) | (4) |
| **Panel A: Business/Law** | | | | |
| Audit × Post | -0.028 | 0.026 | 0.036** | -0.063** |
| | (0.071) | (0.021) | (0.014) | (0.026) |
| $R^2$ | 0.90 | 0.54 | 0.30 | 0.57 |
| *Mean Dep. Var.* | 3.02 | 0.25 | 0.28 | 0.47 |
| *SD Dep. Var.* | 1.63 | 0.24 | 0.20 | 0.29 |
| Observations | 50,448 | 50,448 | 50,448 | 50,448 |
| Num. of Clusters | 1,486 | 1,486 | 1,486 | 1,486 |
| **Panel B: Engineering** | | | | |
| Audit × Post | 0.062 | 0.025** | 0.011 | -0.037** |
| | (0.069) | (0.013) | (0.011) | (0.015) |
| $R^2$ | 0.95 | 0.55 | 0.36 | 0.60 |
| *Mean Dep. Var.* | 3.48 | 0.16 | 0.21 | 0.64 |
| *SD Dep. Var.* | 1.66 | 0.18 | 0.16 | 0.25 |
| Observations | 58,210 | 58,210 | 58,210 | 58,210 |
| Num. of Clusters | 1,648 | 1,648 | 1,648 | 1,648 |
| Muni. × Cohort FE | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the share of students with ENEM grades at different quartiles of the score distribution (controlling for exam year), out of all students enrolled in public universities. Panel A reports the sample of students who enroll in business/law, and Panel B includes the sample of students who end up in engineering. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period is after the period of audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table 4:** Effect of Anti-Corruption Audits on Early Careers

| | Realizations of First Jobs by Sector | | | | | |
|---|---|---|---|---|---|---|
| | Public Sector | | | Private Sector | | |
| | Share (1) | Num. (asinh) (2) | Num. (log) (3) | Share (4) | Num. (asinh) (5) | Num. (log) (6) |
| Audit × Post | -0.023 | -0.317 | 0.024 | 0.023 | 0.232* | 0.234* |
| | (0.027) | (0.244) | (0.128) | (0.027) | (0.125) | (0.126) |
| $R^2$ | 0.66 | 0.84 | 0.85 | 0.66 | 0.95 | 0.95 |
| *Mean Dep. Var.* | 0.22 | 1.93 | 1.65 | 0.78 | 3.56 | 3.00 |
| *SD Dep. Var.* | 0.25 | 1.40 | 1.21 | 0.25 | 1.71 | 1.61 |
| Observations | 82,468 | 82,468 | 49,968 | 82,468 | 82,468 | 66,706 |
| Num. of Clusters | 2,898 | 2,898 | 2,159 | 2,898 | 2,898 | 2,525 |
| Muni. × Cohort FE | X | X | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the share of students in the public sector (column 1) versus the private sector (column 4) as well as the corresponding total number of students (reported in inverse hyperbolic sine transformations in columns 2 and 5, and in log transformations in columns 3 and 6). The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table 5:** Effect of Audits on Workforce Composition by Ability

| | Total Num. (log) | Shares by Quartile of ENEM Grades | | |
|---|---|---|---|---|
| | | Lowest 50% | Second Highest 25% | Highest 25% |
| | (1) | (2) | (3) | (4) |
| **Panel A: Public Sector (Civil Servants)** | | | | |
| Audit × Post | 0.246 | -0.079 | 0.322*** | -0.197*** |
| | (0.189) | (0.084) | (0.109) | (0.074) |
| $R^2$ | 0.82 | 0.49 | 0.35 | 0.50 |
| *Mean Dep. Var.* | 1.25 | 0.32 | 0.27 | 0.38 |
| *SD Dep. Var.* | 1.07 | 0.36 | 0.31 | 0.36 |
| Observations | 26,896 | 26,701 | 26,701 | 26,701 |
| Num. of Clusters | 1,403 | 1,395 | 1,395 | 1,395 |
| **Panel B: Private Sector** | | | | |
| Audit × Post | 0.267** | -0.050 | -0.026 | 0.062** |
| | (0.120) | (0.031) | (0.023) | (0.024) |
| $R^2$ | 0.95 | 0.42 | 0.26 | 0.55 |
| *Mean Dep. Var.* | 3.13 | 0.39 | 0.28 | 0.31 |
| *SD Dep. Var.* | 1.60 | 0.24 | 0.19 | 0.22 |
| Observations | 66,686 | 65,889 | 65,889 | 65,889 |
| Num. of Clusters | 2,524 | 2,497 | 2,497 | 2,497 |
| Muni. × Cohort FE | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the share of students with ENEM grades at different quartiles of the score distribution (controlling for exam year), out of all students from the same municipality who show up in RAIS. Panel A reports the sample of students who end up in civil service, and Panel B includes the sample of students who end up in the private sector. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table 6:** Effect of Audits on Degree Vacancies

| | Num. of Degree Vacancies (asinh) | |
|---|---|---|
| | Business/Law | Engineering |
| | (1) | (2) |
| **Panel A: Private University** | | |
| Audit × Post | -0.156* | 0.456** |
| | (0.090) | (0.200) |
| $R^2$ | 0.72 | 0.73 |
| *Mean Dep. Var.* | 5.44 | 5.38 |
| *SD Dep. Var.* | 0.68 | 0.59 |
| Observations | 14,488 | 6,161 |
| Num. of Clusters | 403 | 195 |
| **Panel B: Public University** | | |
| Audit × Post | -0.428 | -0.049 |
| | (0.327) | (0.251) |
| $R^2$ | 0.76 | 0.77 |
| *Mean Dep. Var.* | 4.60 | 4.55 |
| *SD Dep. Var.* | 1.21 | 0.83 |
| Observations | 6,346 | 5,689 |
| Num. of Clusters | 208 | 186 |
| Muni. × Cohort FE | X | X |
| State × Year × Cohort FE | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1, for a balanced panel of municipalities observed during $[-2, 4]$ where $t$ is a year. Dependent variables are (inverse hyperbolic transformed) numbers of vacancies offered for business and law (column 1) and engineering (column 2). Panel A includes the sample of all private universities and Panel B includes that of all public universities. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the year is after the year of interest. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table 7:** Effect of Audits on Municipal Employment

| | Num. of Total First Hires (asinh) | | |
|---|---|---|---|
| | Public Sector | | Private Sector |
| | Civil Service | Temporary | |
| | (1) | (2) | (3) |
| **Panel A: RAIS 2010-2018** | | | |
| Audit × Post | 0.021 | 0.187 | 0.009 |
| | (0.274) | (0.235) | (0.048) |
| $R^2$ | 0.67 | 0.80 | 0.97 |
| *Mean Dep. Var.* | 2.82 | 3.07 | 6.89 |
| *SD Dep. Var.* | 2.28 | 2.43 | 1.68 |
| Observations | 156,266 | 156,266 | 156,266 |
| Num. of Clusters | 3,693 | 3,693 | 3,693 |
| **Panel B: RAIS 2002-2018** | | | |
| Audit × Post | 0.393*** | 0.178 | 0.007 |
| | (0.116) | (0.118) | (0.027) |
| $R^2$ | 0.62 | 0.73 | 0.96 |
| *Mean Dep. Var.* | 2.69 | 2.84 | 7.03 |
| *SD Dep. Var.* | 2.27 | 2.33 | 1.66 |
| Observations | 524,351 | 524,351 | 524,351 |
| Num. of Clusters | 5,347 | 5,347 | 5,347 |
| Muni. × Cohort FE | X | X | X |
| State × Year × Cohort FE | X | X | X |

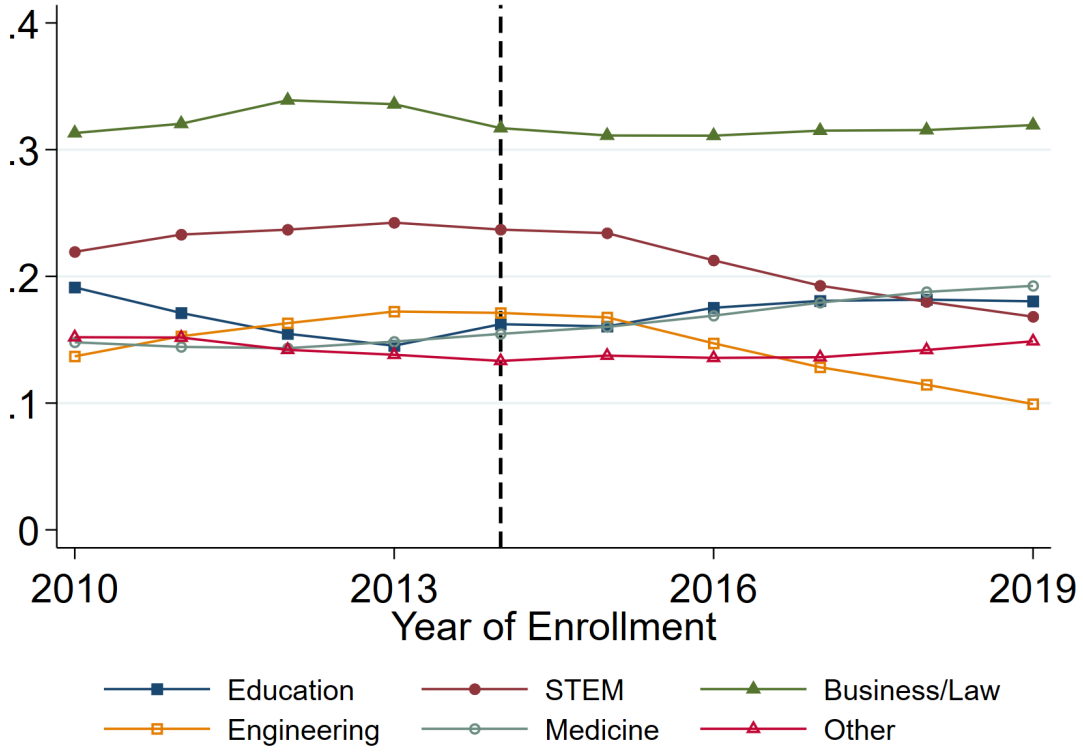
*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are (inverse hyperbolic transformed) the total number of public hires (civil servants in column 2 and temporary workers in column 3) and the total number of private hires (column 4). Panel A includes the sample of municipalities audited during 2011-2014 (with corresponding RAIS data observed during 2010-2018), and Panel B extends the sample to all municipalities audited during 2003-2014 (with corresponding RAIS data observed during 2002-2018). The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Corruption and Talent Allocation**

# Online Appendix

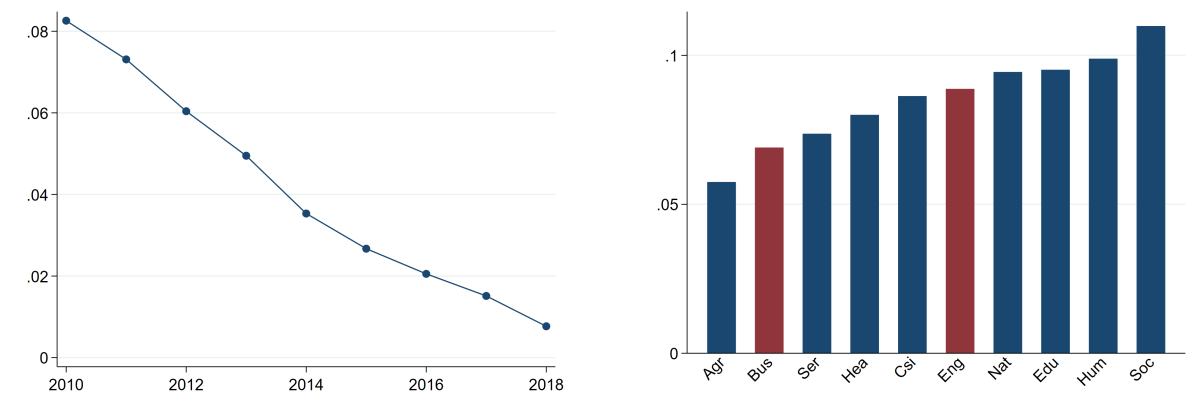
## A Additional Figures and Tables

**Figure A1:** Major Enrollment Trends in Brazil




*Notes:* This figure shows the yearly trends of college major enrollments in Brazil using data from the Census of Higher Education (2010-2019). STEM includes mathematics and natural sciences, computer science and technology, as well as engineering. "Other" includes other smaller fields of study such as arts and humanities, social sciences, agriculture, and services.

**Figure A2:** Share of Students Traced to RAIS

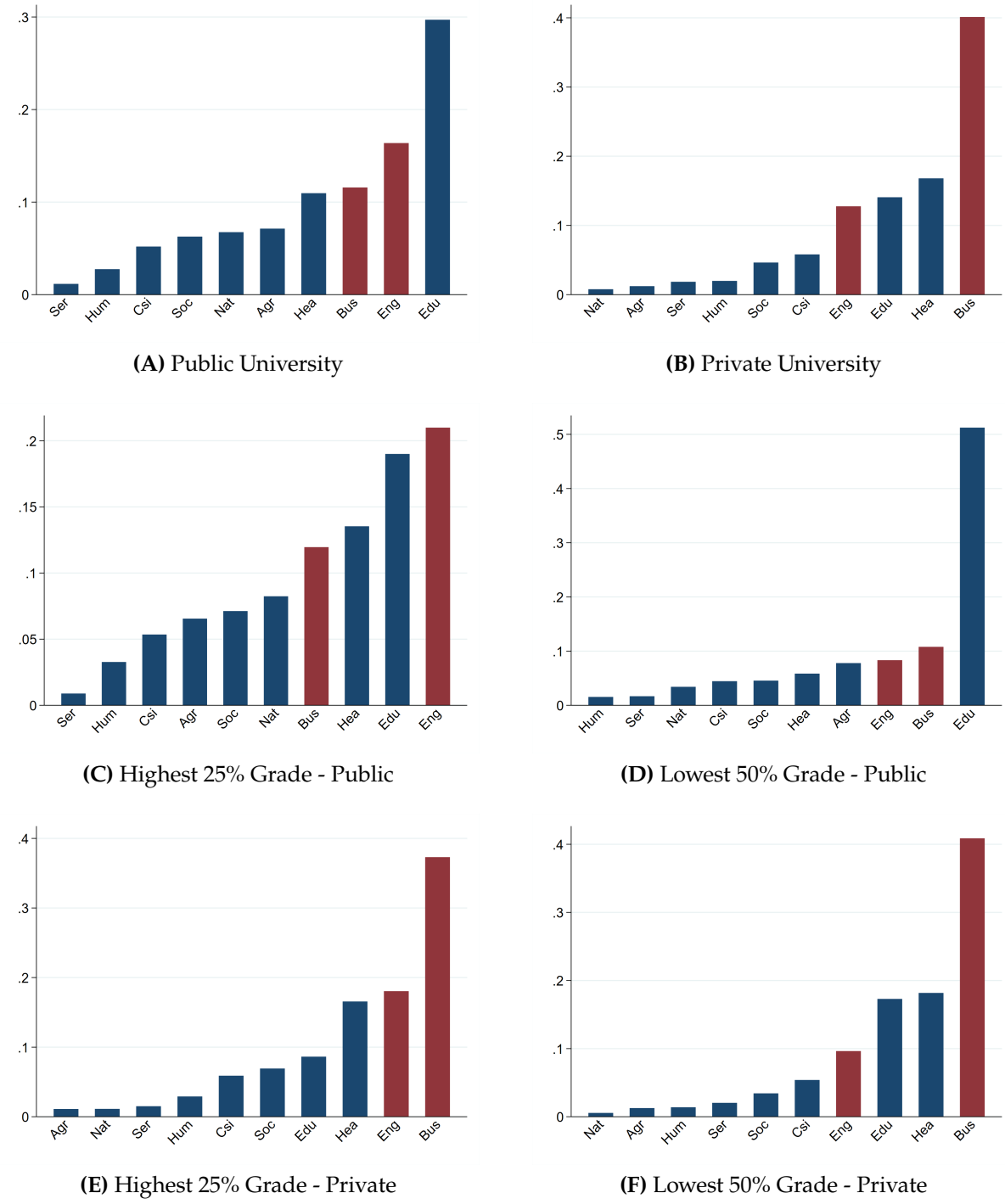



**(A)** By Year of Enrollment

**(B)** By Freshmen Major 2010

*Notes:* This figure illustrates the share of students observed in the Census of Higher Education that are traced to RAIS (2010-2018). Panel A displays the share of students traced by year of enrollment. Panel B displays the share of students traced by their major enrolled for the 2010 enrollment cohort only.

**Figure A3:** Baseline Shares of Major Enrollment by Group




**(A)** Public University

**(B)** Private University

**(C)** Highest 25% Grade - Public

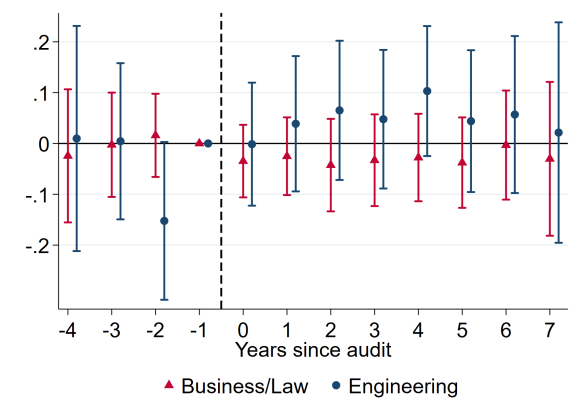
**(D)** Lowest 50% Grade - Public

**(E)** Highest 25% Grade - Private

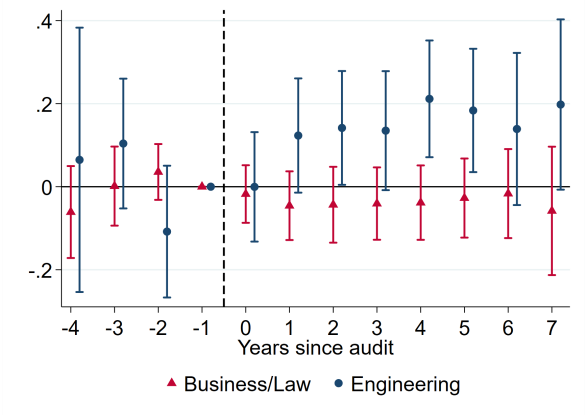
**(F)** Lowest 50% Grade - Private

*Notes:* This figure illustrates the share of major enrollment among all freshmen observed in the Census of Higher Education. Panel A reports for public university students only and panel B reports for private university students in the year 2010. Panel C reports for high-ability students (at the top quartile of the ENEM grade distribution) while Panel D reports for low-ability students (at the lowest 50% of the ENEM grade distribution), for public university students. Panels E and F repeat for private university students.

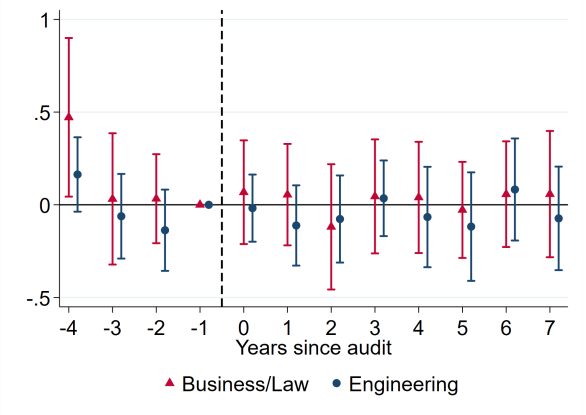
**Figure A4:** Audits and Numbers of Major Enrollment




**(A)** All Universities



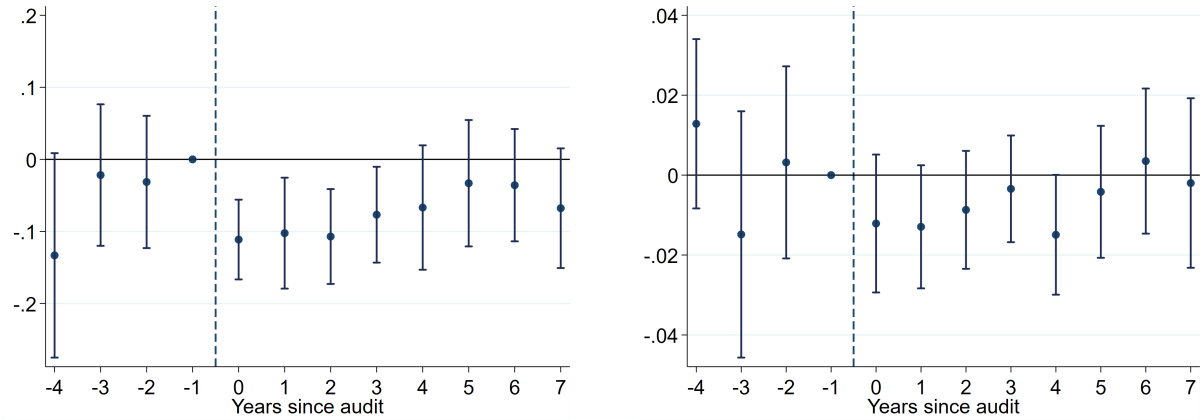

**(B)** Private Universities




**(C)** Public Universities

*Notes:* This figure reports coefficients obtained from the estimation of equation 2 (corresponding to Table 2), where the estimated differences between treatment and control municipalities are allowed to vary for each year around the audit. All outcomes are reported in inverse hyperbolic sine (IHS) transformations. Panel A includes the sample pooling all private and public university students. Panels B and C report separately for private versus public universities. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

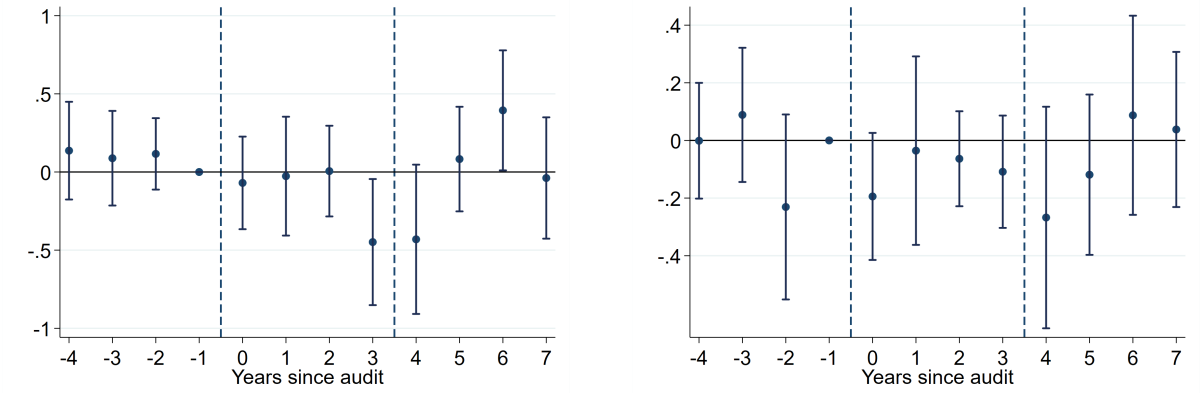
**Figure A5:** Share of High-Ability Students in Business/Law




**(A)** Public Uni. **(B)** Private Uni.

*Notes:* This figure reports coefficients obtained from the estimation of equation 2. Panel A corresponds to Table 3 and Panel B corresponds to Appendix Table A12. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

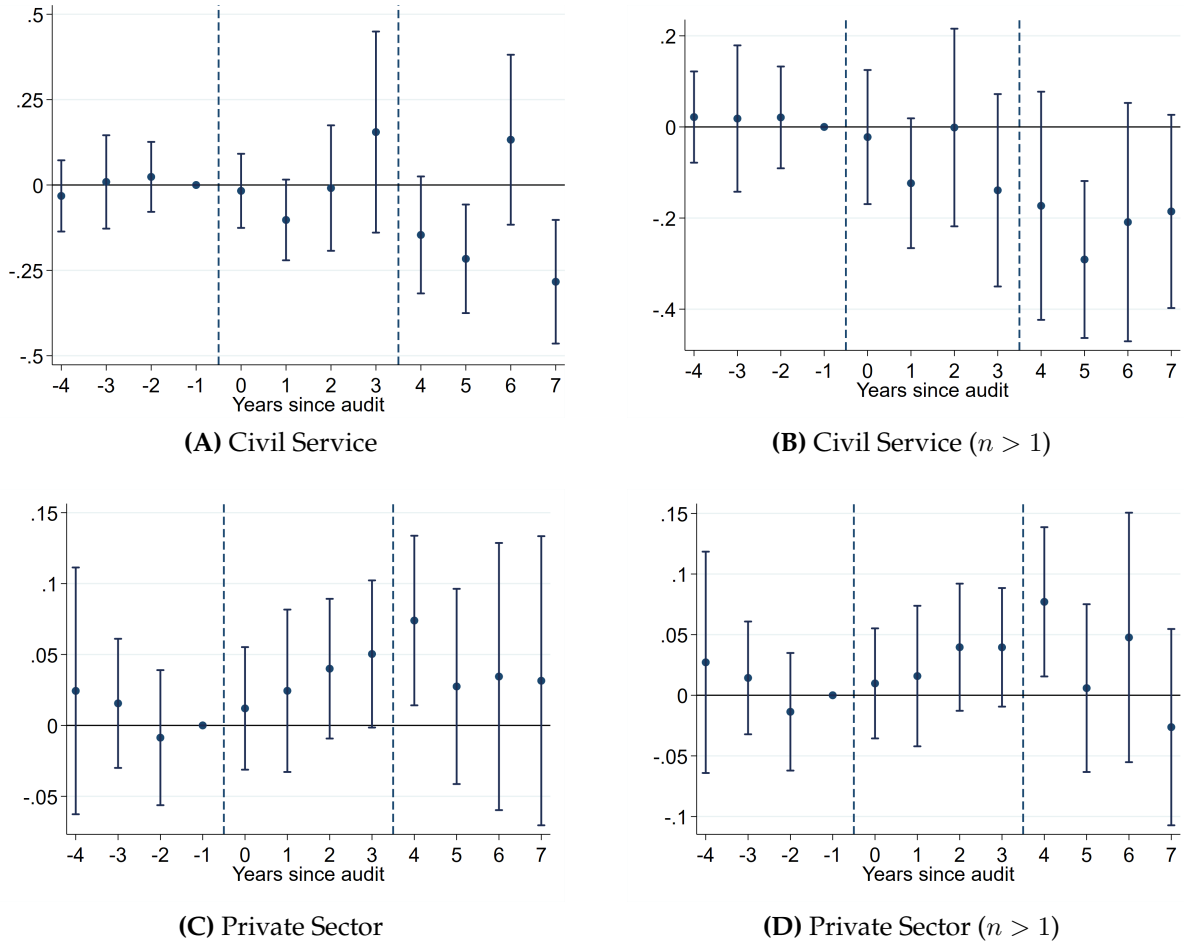
**Figure A6:** Audits and Realized Careers by Contract Type in Public Sector




**(A)** Number (asinh) in Civil Service **(B)** Number (asinh) in Temporary

*Notes:* This figure reports coefficients obtained from the estimation of equation 2, corresponding to Appendix Table A5. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

**Figure A7:** Audits and Shares of High Ability in Workforce




**(A)** Civil Service

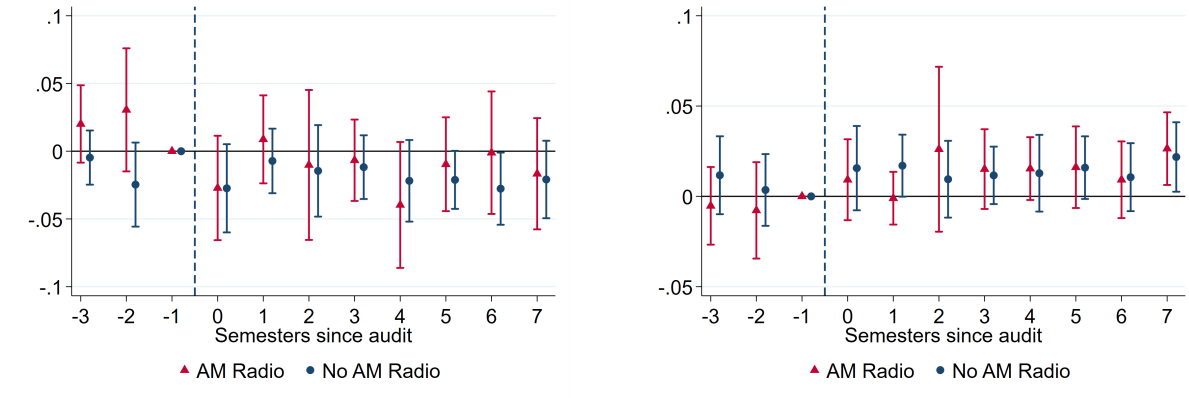
**(B)** Civil Service ($n > 1$)

**(C)** Private Sector

**(D)** Private Sector ($n > 1$)

*Notes:* This figure reports coefficients obtained from the estimation of equation 2. *Dep. Var.* is the share of workers from the top quartile of the ENEM grade distribution. Panels A and C correspond to table estimates reported in column 4 of Table 5. Panels B and D report results for the same regressions but the samples are restricted to those with $n > 1$ (at least 2 students showing up in the civil service/private sector from the municipality-year bin, see Panel B of Appendix Table E6). Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

**Figure A8:** Effect Heterogeneity by Traditional Media

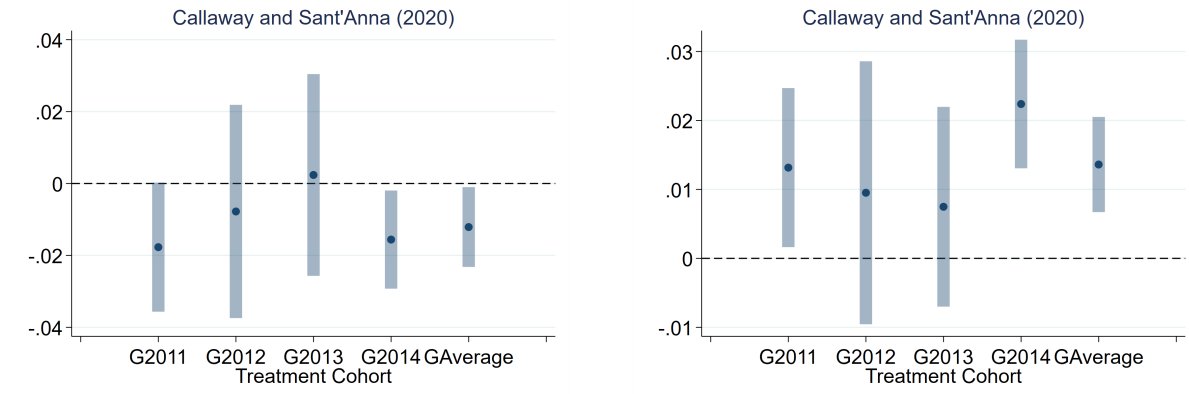



**(A)** AM Radio - Business/Law

**(B)** AM Radio - Engineering

*Notes:* This figure reports coefficients obtained from the estimation of equation 2 but separately for municipalities with and without AM radio stations as reported in the 2009 *Perfil dos Municípios Brasileiros*, where time is a semester. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

**Figure A9:** Group-Specific Treatment Effects via Callaway and Sant'Anna (2021)




**(A)** Share in Business/Law

**(B)** Share in Engineering

*Notes:* This figure presents the group-specific treatment effects using the estimator proposed in Callaway and Sant'Anna (2021). In Panel A the outcome is the share of freshmen enrollment in business/law. In Panel B the outcome is the share of freshmen enrollment in engineering.

**Table A1:** Summary Statistics of Workforce Characteristics

| | Private Sector | | Public Sector | | | |
|---|---|---|---|---|---|---|
| | | | Tenure-Track | | Temporary | |
| | Mean (1) | Mean ($t \geq 4$) (2) | Mean (3) | Mean ($t \geq 4$) (4) | Mean (5) | Mean ($t \geq 4$) (6) |
| Num. of students (log) | 3.76 | 3.19 | 1.98 | 1.73 | 1.98 | 1.75 |
| Lapse CES-RAIS (Years) | 3.69 | 5.68 | 4.70 | 6.03 | 4.53 | 5.84 |
| Share female | 0.60 | 0.60 | 0.59 | 0.59 | 0.68 | 0.70 |
| Age | 24.28 | 26.26 | 28.50 | 29.36 | 28.40 | 29.46 |
| Share with postgraduate degree | 0.01 | 0.01 | 0.02 | 0.02 | 0.01 | 0.01 |
| Share with college-educated parent | 0.36 | 0.42 | 0.26 | 0.27 | 0.21 | 0.23 |
| Share among top family income quartile | 0.19 | 0.22 | 0.16 | 0.15 | 0.10 | 0.10 |
| Share among top ENEM grade quartile | 0.46 | 0.45 | 0.48 | 0.46 | 0.31 | 0.30 |
| Avg. ENEM grade | 568.83 | 585.75 | 570.53 | 576.51 | 533.98 | 537.42 |
| Share enrolled in Business/Law | 0.27 | 0.17 | 0.20 | 0.21 | 0.17 | 0.18 |
| Share enrolled in Engineering | 0.17 | 0.20 | 0.08 | 0.08 | 0.09 | 0.08 |
| Share enrolled in Education | 0.18 | 0.18 | 0.42 | 0.38 | 0.46 | 0.42 |
| Share enrolled in Health | 0.15 | 0.19 | 0.15 | 0.17 | 0.16 | 0.19 |
| Observations | 2,331 | | 1,557 | | 1,596 | |

*Notes*: This table shows the means of various characteristics of students who enrolled in higher education in the baseline year of 2010 and were later found in RAIS during 2010-2018. In particular, odd columns report the full sample mean and even columns report the sample mean restricting to students who show up at least 4 years later. Columns 1-2 present summary statistics for students who land a first job contract labeled as private. Columns 3-4 and columns 5-6 report the same for public contracts, separately for tenure-track and temporary positions. *Lapse CES-RAIS* indicates the average years it takes for students to show up between the two datasets (from college enrollment to first job in the formal labor market). *Share with college-educated parent* is the share of students whose (either) parent received some college education. *Share among top family income quartile* is the share of students whose reported monthly family income belongs to the top quartile of the entire income distribution. *Share among top ENEM grade quartile* is the share of students whose average ENEM score belongs to the top quartile of the entire score distribution. *Avg. ENEM grade* is the average test score across all subjects for those who take the ENEM exam.

**Table A2:** Effect of Audits on Other Major Enrollment

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| **Panel A** | Education | | Humanities | | Soc. Sci. | | Nat. Sci. | |
| | Share | Num. | Share | Num. | Share | Num. | Share | Num. |
| Audit × Post | 0.008 | 0.040 | -0.002 | -0.062 | -0.002 | -0.067 | 0.003 | 0.144* |
| | (0.006) | (0.036) | (0.001) | (0.065) | (0.002) | (0.054) | (0.002) | (0.083) |
| $R^2$ | 0.71 | 0.95 | 0.62 | 0.93 | 0.52 | 0.94 | 0.63 | 0.92 |
| *Mean Dep. Var.* | 0.19 | 4.84 | 0.02 | 2.37 | 0.05 | 3.40 | 0.02 | 2.34 |
| *SD Dep. Var.* | 0.11 | 1.52 | 0.02 | 1.85 | 0.03 | 1.86 | 0.02 | 1.75 |
| Observations | 155,920 | 155,920 | 155,920 | 155,920 | 155,920 | 155,920 | 155,920 | 155,920 |
| Num. of Clusters | 3,693 | 3,693 | 3,693 | 3,693 | 3,693 | 3,693 | 3,693 | 3,693 |
| **Panel B** | Comp. Sci. and IT | | Agriculture | | Medicine | | Services | |
| | Share | Num. | Share | Num. | Share | Num. | Share | Num. |
| Audit × Post | -0.001 | -0.004 | 0.000 | -0.016 | -0.005 | -0.026 | 0.000 | -0.054 |
| | (0.002) | (0.050) | (0.003) | (0.060) | (0.005) | (0.044) | (0.001) | (0.096) |
| $R^2$ | 0.45 | 0.94 | 0.64 | 0.91 | 0.59 | 0.96 | 0.44 | 0.90 |
| *Mean Dep. Var.* | 0.05 | 3.41 | 0.04 | 3.05 | 0.15 | 4.65 | 0.02 | 2.35 |
| *SD Dep. Var.* | 0.03 | 1.78 | 0.04 | 1.55 | 0.07 | 1.63 | 0.02 | 1.69 |
| Observations | 155,920 | 155,920 | 155,920 | 155,920 | 155,920 | 155,920 | 155,920 | 155,920 |
| Num. of Clusters | 3,693 | 3,693 | 3,693 | 3,693 | 3,693 | 3,693 | 3,693 | 3,693 |
| Muni. × Cohort FE | X | X | X | X | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the share of freshmen as well as the corresponding (inverse hyperbolic sine transformed) total number of enrollments in each of the eight remaining fields of study. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the year is after the year of interest. The sample includes all students pooling public and private universities. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A3:** Effect of Audits on College Enrollment

| | Num. of Freshmen (log) (1) | Num. in Public Uni. (log) (2) | Share in Public Uni. (3) |
|---|---|---|---|
| **Panel A: All Students** | | | |
| Audit × Post | 0.019 | 0.010 | 0.004 |
| | (0.025) | (0.040) | (0.008) |
| $R^2$ | 0.99 | 0.96 | 0.85 |
| *Mean Dep. Var.* | 5.90 | 4.37 | 0.27 |
| *SD Dep. Var.* | 1.59 | 1.73 | 0.17 |
| Observations | 155,920 | 143,667 | 143,667 |
| Num. of Clusters | 3,693 | 3,684 | 3,684 |
| **Panel B: High-Ability Students (ENEM Highest 25%)** | | | |
| Audit × Post | -0.006 | 0.016 | 0.009 |
| | (0.022) | (0.038) | (0.009) |
| $R^2$ | 0.98 | 0.97 | 0.77 |
| *Mean Dep. Var.* | 4.72 | 4.01 | 0.51 |
| *SD Dep. Var.* | 1.76 | 1.80 | 0.19 |
| Observations | 136,686 | 116,295 | 116,295 |
| Num. of Clusters | 3,619 | 3,471 | 3,471 |
| **Panel C: Low-Ability Students (ENEM Lowest 50%)** | | | |
| Audit × Post | 0.024 | 0.091 | 0.003 |
| | (0.028) | (0.093) | (0.008) |
| $R^2$ | 0.98 | 0.89 | 0.80 |
| *Mean Dep. Var.* | 5.04 | 2.75 | 0.16 |
| *SD Dep. Var.* | 1.56 | 1.57 | 0.17 |
| Observations | 154,800 | 118,108 | 118,108 |
| Num. of Clusters | 3,693 | 3,521 | 3,521 |
| Muni. × Cohort FE | X | X | X |
| State × Year × Cohort FE | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are (log) total number of freshmen (column 1), (log) total number of freshmen in public universities (column 2) and share of freshmen enrolled in public universities (column 3). Panel A reports the sample of all freshmen students. Panel B and Panel C report separately for high-ability (highest 25% grade) and low-ability (lowest 50% grade) students. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the year is after the year of interest. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A4:** Effect of Audits on Student Composition (Private Uni.) by Ability

| | Total Num. (log) | Shares by Quartile of ENEM Grades | | |
|---|---|---|---|---|
| | | Lowest 50% | Second Highest 25% | Highest 25% |
| | (1) | (2) | (3) | (4) |
| **Panel A: Business/Law** | | | | |
| Audit × Post | -0.030 | 0.009 | -0.001 | -0.008 |
| | (0.031) | (0.007) | (0.005) | (0.006) |
| $R^2$ | 0.98 | 0.59 | 0.32 | 0.54 |
| *Mean Dep. Var.* | 4.71 | 0.60 | 0.26 | 0.14 |
| *SD Dep. Var.* | 1.62 | 0.14 | 0.09 | 0.09 |
| Observations | 130,798 | 130,798 | 130,798 | 130,798 |
| Num. of Clusters | 3,156 | 3,156 | 3,156 | 3,156 |
| **Panel B: Engineering** | | | | |
| Audit × Post | 0.181*** | 0.031*** | 0.002 | -0.033*** |
| | (0.068) | (0.011) | (0.009) | (0.012) |
| $R^2$ | 0.96 | 0.47 | 0.21 | 0.46 |
| *Mean Dep. Var.* | 4.15 | 0.47 | 0.30 | 0.23 |
| *SD Dep. Var.* | 1.64 | 0.17 | 0.12 | 0.13 |
| Observations | 82,007 | 82,007 | 82,007 | 82,007 |
| Num. of Clusters | 2,058 | 2,058 | 2,058 | 2,058 |
| Muni. × Cohort FE | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the share of students with ENEM grades at different quartiles of the score distribution (controlling for exam year), out of all students enrolled in private universities (in comparison with Table 3). Panel A reports the sample of students who enroll in business/law, and Panel B includes the sample of students who end up in engineering. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period is after the period of audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A5:** Effect of Audits on Major Enrollment by Ability Group

| | Num. of Enrollment (asinh) in Broad Major Fields | | | | |
|---|---|---|---|---|---|
| | Business/Law | STEM | Education | Medicine | Hum. & Soc. Sci. |
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: High-Ability Students (ENEM Highest 25%)** | | | | | |
| Audit × Post | -0.093*** | 0.049 | -0.042 | -0.005 | -0.020 |
| | (0.029) | (0.040) | (0.039) | (0.041) | (0.052) |
| $R^2$ | 0.96 | 0.97 | 0.94 | 0.95 | 0.95 |
| *Mean Dep. Var.* | 3.90 | 4.27 | 3.26 | 3.50 | 3.01 |
| *SD Dep. Var.* | 1.73 | 1.83 | 1.70 | 1.71 | 1.87 |
| Observations | 136,686 | 136,686 | 136,686 | 136,686 | 136,686 |
| Num. of Clusters | 3,619 | 3,619 | 3,619 | 3,619 | 3,619 |
| **Panel B: Low-Ability Students (ENEM Lowest 50%)** | | | | | |
| Audit × Post | -0.026 | 0.084 | 0.070* | -0.055 | -0.022 |
| | (0.035) | (0.054) | (0.042) | (0.052) | (0.064) |
| $R^2$ | 0.96 | 0.94 | 0.92 | 0.94 | 0.91 |
| *Mean Dep. Var.* | 4.57 | 3.83 | 4.13 | 3.85 | 2.58 |
| *SD Dep. Var.* | 1.71 | 1.77 | 1.50 | 1.62 | 1.76 |
| Observations | 154,800 | 154,800 | 154,800 | 154,800 | 154,800 |
| Num. of Clusters | 3,693 | 3,693 | 3,693 | 3,693 | 3,693 |
| Muni. × Cohort FE | X | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the number of enrollments (inverse hyperbolic sine transformed) in the corresponding fields of study, where some fields are grouped into broad categories such as STEM (nat. sci., engineering, and comp. sci.). Panel A reports the sample of high-ability students (top 25% ENEM performance), and Panel B includes the sample of low-ability students (bottom 50% ENEM performance). The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period is after the period of audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A6:** Effect of Audits on Early Careers in Public Sector

| | Realizations of First Jobs in Public Sector by Contract Type | | | | | |
|---|---|---|---|---|---|---|
| | Civil Service | | | Temporary | | |
| | Share | Num. (asinh) | Num. (log) | Share | Num. (asinh) | Num. (log) |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Audit × Post | -0.021* | -0.241 | 0.146 | -0.002 | -0.151 | 0.003 |
| | (0.011) | (0.223) | (0.201) | (0.024) | (0.179) | (0.151) |
| $R^2$ | 0.51 | 0.81 | 0.82 | 0.63 | 0.83 | 0.84 |
| *Mean Dep. Var.* | 0.09 | 1.38 | 1.35 | 0.12 | 1.16 | 1.26 |
| *SD Dep. Var.* | 0.15 | 1.30 | 1.09 | 0.21 | 1.28 | 1.13 |
| Observations | 82,468 | 82,468 | 26,906 | 82,468 | 82,468 | 33,401 |
| Num. of Clusters | 2,898 | 2,898 | 1,404 | 2,898 | 2,898 | 1,564 |
| Muni. × Cohort FE | X | X | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1, zooming into different types of public sector careers as reported in columns 1-3 of Table 4. Dependent variables are the share of students in the civil service (column 1) versus the temporary public workers (column 6) as well as the corresponding total number of students (reported in inverse hyperbolic sine transformations in columns 2 and 5, and in log transformations in columns 4 and 6). The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A7:** Effect of Audits on Workforce Composition (Other Characteristics)

| | Demographic and Socioeconomic Characteristics | | | |
|---|---|---|---|---|
| | Share Female | Share College-Educated Parent(s) | Share Family Income (top 25%) | Avg. ENEM Grades (Std.) |
| | (1) | (2) | (3) | (4) |
| **Panel A: Public Sector (Civil Servants)** | | | | |
| Audit × Post | 0.046 | 0.090 | -0.008 | -0.039 |
| | (0.138) | (0.111) | (0.124) | (0.215) |
| $R^2$ | 0.39 | 0.43 | 0.43 | 0.48 |
| *Mean Dep. Var.* | 0.56 | 0.28 | 0.23 | 0.47 |
| *SD Dep. Var.* | 0.35 | 0.32 | 0.30 | 1.01 |
| Observations | 26,896 | 26,896 | 26,896 | 26,896 |
| Num. of Clusters | 1,403 | 1,403 | 1,403 | 1,403 |
| **Panel B: Private Sector** | | | | |
| Audit × Post | 0.004 | 0.074** | 0.042* | 0.048 |
| | (0.029) | (0.031) | (0.022) | (0.080) |
| $R^2$ | 0.26 | 0.45 | 0.58 | 0.49 |
| *Mean Dep. Var.* | 0.59 | 0.35 | 0.31 | 0.31 |
| *SD Dep. Var.* | 0.20 | 0.21 | 0.22 | 0.67 |
| Observations | 66,686 | 66,686 | 66,686 | 66,686 |
| Num. of Clusters | 2,524 | 2,524 | 2,524 | 2,524 |
| Muni. × Cohort FE | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the share of female students (column 1), the share of students with college-educated parent(s) (column 2), the share with family income at the top quartile of the distribution (column 3) and the standardized ENEM grades (column 4) for all students from the municipality who show up in RAIS. Panel A reports the sample of students who end up in civil service, and Panel B includes the sample of students who end up in the private sector. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A8:** Effect of Audits on Workforce Composition (Degree Background)

| | Share of Employee's Degree Background | | | |
|---|---|---|---|---|
| | Business/Law | Engineering | Education | Health |
| | (1) | (2) | (3) | (4) |
| **Panel A: Public Sector (Civil Servants)** | | | | |
| Audit × Post | -0.003 | 0.109 | -0.047 | -0.017 |
| | (0.056) | (0.127) | (0.114) | (0.049) |
| $R^2$ | 0.36 | 0.31 | 0.38 | 0.34 |
| *Mean Dep. Var.* | 0.21 | 0.09 | 0.43 | 0.13 |
| *SD Dep. Var.* | 0.29 | 0.20 | 0.35 | 0.23 |
| Observations | 26,896 | 26,896 | 26,896 | 26,896 |
| Num. of Clusters | 1,403 | 1,403 | 1,403 | 1,403 |
| **Panel B: Private Sector** | | | | |
| Audit × Post | -0.099*** | 0.090*** | -0.034** | 0.014 |
| | (0.026) | (0.022) | (0.017) | (0.025) |
| $R^2$ | 0.29 | 0.32 | 0.44 | 0.26 |
| *Mean Dep. Var.* | 0.31 | 0.18 | 0.16 | 0.13 |
| *SD Dep. Var.* | 0.19 | 0.15 | 0.18 | 0.14 |
| Observations | 66,686 | 66,686 | 66,686 | 66,686 |
| Num. of Clusters | 2,524 | 2,524 | 2,524 | 2,524 |
| Muni. × Cohort FE | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the share of students from business/law backgrounds (column 1) versus those from engineering backgrounds (column 2), education (column 3) or health (column 4) among all students from the same municipality who show up in RAIS. Panel A reports the sample of students who end up in civil service, and Panel B includes the sample of students who end up in the private sector. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A9:** Effect of Audits on Shares of Major Enrollment - Spillovers

| | Share in Business/Law (1) | Share in Engineering (2) |
|---|---|---|
| **Panel A: Spillover effects** | | |
| Audit × Post | -0.017* | 0.010* |
| | (0.009) | (0.005) |
| $R^2$ | 0.65 | 0.76 |
| *Mean Dep. Var.* | 0.29 | 0.16 |
| *SD Dep. Var.* | 0.08 | 0.07 |
| Observations | 21,128 | 21,128 |
| Num. of Clusters | 690 | 690 |
| **Panel B: Excluding spillover effects** | | |
| Audit × Post | -0.015** | 0.018*** |
| | (0.008) | (0.005) |
| $R^2$ | 0.64 | 0.77 |
| *Mean Dep. Var.* | 0.29 | 0.16 |
| *SD Dep. Var.* | 0.08 | 0.07 |
| Observations | 20,732 | 20,732 |
| Num. of Clusters | 647 | 647 |
| Muni. × Cohort FE | X | X |
| State × Year × Cohort FE | X | X |

*Notes*: This table decomposes the direct versus indirect effects of audits on the baseline shares of major enrollment for the pooled sample (see Panel A in Table 2) when geographic spillovers are taken into account following Colonnelli and Prem (2022). Panel A reports coefficients obtained via the estimation of equation 1 but for the impacts on nearby municipalities (defined as municipalities in the same micro-region). Panel B reports coefficients from the baseline specification where the sample excludes never-audited municipalities with at least one nearby municipality audited in the past 5 years. Dependent variables are the share of freshmen enrolled in business and law (column 1) versus engineering (column 2). The unit of observation is municipality-year-cohort. Post is a dummy that is 1 if the period is after the period of the audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A10:** Effect Heterogeneity by Level of Uncovered Corruption

| | Business/Law | | Engineering | |
|---|---|---|---|---|
| | Share | Num. (asinh) | Share | Num. (asinh) |
| | (1) | (2) | (3) | (4) |
| Audit × Post × High | -0.020** | -0.118** | 0.019*** | 0.056 |
| | (0.008) | (0.050) | (0.007) | (0.057) |
| Audit × Post × Low | -0.015 | -0.031 | 0.010 | 0.053 |
| | (0.010) | (0.042) | (0.009) | (0.074) |
| $R^2$ | 0.43 | 0.96 | 0.55 | 0.95 |
| *Mean Dep. Var.* | 0.31 | 5.12 | 0.16 | 4.37 |
| *SD Dep. Var.* | 0.10 | 1.81 | 0.09 | 1.89 |
| Observations | 375,672 | 375,672 | 375,672 | 375,672 |
| Num. of Clusters | 3,871 | 3,871 | 3,871 | 3,871 |
| Muni. × Cohort FE | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation $Y_{mct} = \beta_1 Audit_{mc} \times Post_{ct} + \beta_2 Audit_{mc} \times Post_{ct} \times High_m + \delta_{mc} + \lambda_{tc} + \epsilon_{mct}$ for a balanced panel of municipalities within the time window [-3, 7], where t is a semester. $High_m$ equals 1 for municipalities with above median level of corruption uncovered. Dependent variables are the share of freshmen enrolled in business and law (column 1) versus engineering (column 3) as well as the corresponding (inverse hyperbolic sine transformed) total number of enrollments (columns 2 and 4). The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period is after the period of the audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A11:** Effect Heterogeneity by Local Media

| | Business/Law | | Engineering | |
|---|---|---|---|---|
| | Share | Num. (asinh) | Share | Num. (asinh) |
| | (1) | (2) | (3) | (4) |
| **Panel A: Internet Provider** | | | | |
| Audit × Post × Z | -0.019** | -0.072* | 0.015** | 0.067 |
| | (0.008) | (0.038) | (0.007) | (0.056) |
| Audit × Post | -0.013 | -0.059 | 0.006 | -0.008 |
| | (0.009) | (0.047) | (0.008) | (0.060) |
| $R^2$ | 0.43 | 0.96 | 0.55 | 0.95 |
| *Mean Dep. Var.* | 0.31 | 5.12 | 0.16 | 4.37 |
| *SD Dep. Var.* | 0.10 | 1.81 | 0.09 | 1.89 |
| **Panel B: AM Radio Station** | | | | |
| Audit × Post × Z | -0.026** | -0.104** | 0.016 | 0.066 |
| | (0.010) | (0.052) | (0.010) | (0.081) |
| Audit × Post | -0.007 | -0.028 | 0.011* | 0.039 |
| | (0.006) | (0.031) | (0.006) | (0.041) |
| $R^2$ | 0.43 | 0.96 | 0.55 | 0.95 |
| *Mean Dep. Var.* | 0.31 | 5.12 | 0.16 | 4.37 |
| *SD Dep. Var.* | 0.10 | 1.81 | 0.09 | 1.89 |
| Observations | 375,200 | 375,200 | 375,200 | 375,200 |
| Num. of Clusters | 3,866 | 3,866 | 3,866 | 3,866 |
| Muni. × Cohort FE | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation $Y_{mct} = \beta_1 Audit_{mc} \times Post_{ct} + \beta_2 Audit_{mc} \times Post_{ct} \times Z_m + \delta_{mc} + \lambda_{tc} + \epsilon_{mct}$ for a balanced panel of municipalities within the time window [-3, 7], where t is a semester. $Z_m$ equals 1 for municipalities where local media (AM radio station or internet provider) was reportedly available in 2009. Dependent variables are the share of freshmen enrolled in business and law (column 1) versus engineering (column 3) as well as the corresponding (inverse hyperbolic sine transformed) total number of enrollments (columns 2 and 4). The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period is after the period of the audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table A12:** Effect of Audits on Composition of Temporary Public Workers

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Panel A** | Total Num. (log) | Shares by Quartile of ENEM Grades | | |
| | | Lowest 50% | Second Highest 25% | Highest 25% |
| Audit × Post | 0.018 | -0.041 | 0.035 | 0.011 |
| | (0.133) | (0.108) | (0.109) | (0.068) |
| $R^2$ | 0.85 | 0.45 | 0.33 | 0.45 |
| *Mean Dep. Var.* | 1.37 | 0.51 | 0.27 | 0.21 |
| *SD Dep. Var.* | 1.19 | 0.34 | 0.28 | 0.26 |
| Observations | 33,011 | 32,619 | 32,619 | 32,619 |
| Num. of Clusters | 1,562 | 1,548 | 1,548 | 1,548 |
| **Panel B** | Demographic and Socioeconomic Characteristics | | | |
| | Share Female | Share College-Educated Parent(s) | Share Family Income (top 25%) | Avg. ENEM Grades (Std.) |
| Audit × Post | 0.065 | 0.034 | -0.029 | 0.214 |
| | (0.069) | (0.052) | (0.066) | (0.145) |
| $R^2$ | 0.37 | 0.37 | 0.39 | 0.41 |
| *Mean Dep. Var.* | 0.66 | 0.21 | 0.14 | -0.05 |
| *SD Dep. Var.* | 0.33 | 0.27 | 0.21 | 0.92 |
| Observations | 33,011 | 33,011 | 32,990 | 31,814 |
| Num. of Clusters | 1,562 | 1,562 | 1,561 | 1,516 |
| Muni. × Cohort FE | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the share of female students (column 1), average age (column 2), the share of students with college-educated parent(s) (column 3), share with family income at the top quartile of the distribution (column 4) and share with ENEM grades at the top quartile of the distribution (column 5) for all students from the municipality. The sample includes students who end up working in the public sector under temporary contracts. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

## B Audits and Corruption Percetion

In this appendix, I provide some suggestive evidence of how the anti-corruption audits in Brazil affect the perception of corruption among the Brazilian population.

To the best of my knowledge, the only representative socioeconomic survey that asks questions on the perception of corruption in Brazil is the *Latinobarómetro*. For instance, the following question was asked in all available *Latinobarómetro* survey waves during 2004-2020, except for the year 2018:

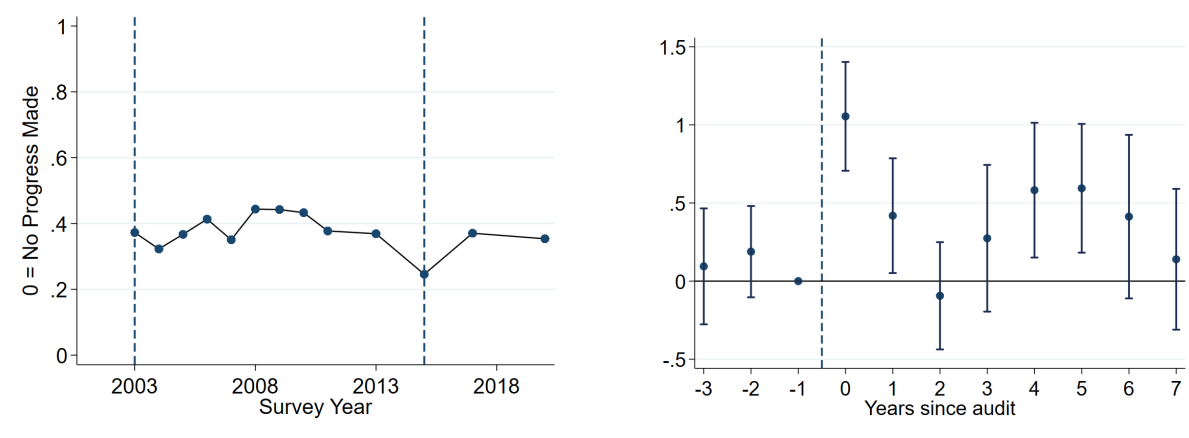
*How much progress do you think has been made on reducing corruption in the state institutions during the last 2 years?*

I follow the same estimation strategy as outlined in section 4.1, where the outcomes are replaced by standardized answers recorded in *Latinobarómetro* spanning from 2001 to 2020. One challenge with the *Latinobarómetro* survey is that the geolocators provided for Brazil are the names of municipalities as well as the broadly defined regions (north, northeast, central-west, southeast and south), the combination of which does not uniquely identify municipalities. To deal with this problem, I remove ambiguous observations (municipalities located in the same region who share the same name) and eventually obtain an unambiguous sample of 54 municipalities (30 *never-audited* and 24 *first-audited* municipalities during 2003-2015) for this part of the analysis. Note that I have a much smaller sample of treated municipalities even after expanding the period of analysis to as early as 2003, as the *Latinobarómetro* only sample survey respondents from about 90 municipalities each survey year. Most of the surveyed municipalities are large state capitals which were not eligible for the CGU audit program. Nevertheless, I present suggestive results using the stacked difference-in-difference for this subsample of municipalities.

As shown in Panel A of Figure B1, the overall perception of progress made in combating corruption at the national level remained low and relatively unchanged throughout the CGU audit campaign (2003 to 2015). However, audits do seem to have altered corruption perception at the local level. Panel B illustrates the event study plot on how anti-corruption audits affect the perception of progress made combatting corruption in the last two years. One can see a positive jump at the $t+0$ period, indicating an impression of more progress made in fighting corruption following the audit announcement. The coefficient drops to 0 at $t+2$ when local corruption scandals are unveiled, but quickly reverses back to positive when the corrupt politicians and public officials start facing legal consequences. I complement the visual evidence with the table estimates (Table B1) from the stacked

difference-in-difference estimation over a wider range of questions on both the perception of corruption and trust in institutions. The coefficients are imprecisely estimated due to the small sample size. Regardless, the signs of the estimates are as expected: audits are associated with perceptions of lower corruption. Columns 3-5 suggest audits are also associated with a lower level of trust in institutions. Overall, the evidence presented in this appendix illustrates the conceptual first stage for the main analysis of the paper: not only did information regarding the audits reach the general population, but they also led to a (local) reduction in the perception of corruption in state institutions. This evidence corroborates the conjecture that the perception of reduced corruption is a likely driver of talent shifting away from public sector trajectories as illustrated in section 4.

**Figure B1:** Perception of Progress Made Combatting Corruption




**(A)** Yearly Trend

**(B)** Stacked Event-Study

*Notes:* Panel A presents the yearly variation of the average response to the question "perception of progress made combatting corruption" (0 indicates no progress made and 1 indicates much progress made) as recorded in survey *Latinobarómetro*. Panel B figure presents event study estimators for the effects of audits on perceptions of progress made combatting corruption from the estimation of equation 2. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

**Table B1:** Audits and Social Attitudes in Latinobarómetro

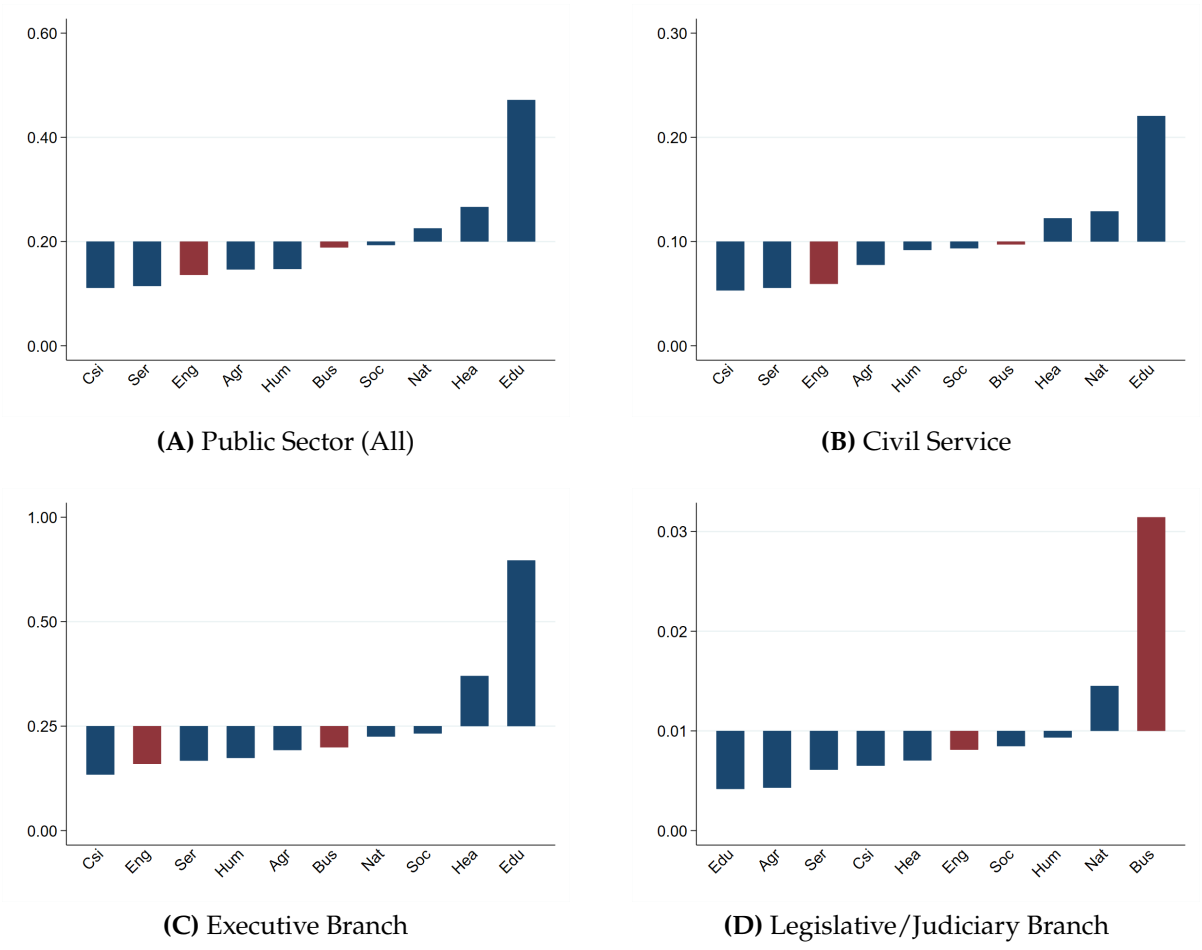
| | Perception of Corruption | | Trust in Institutions | | |
|---|---|---|---|---|---|
| | (1) Problem | (2) Progress | (3) Congress | (4) Fed. Gov. | (5) Judiciary |
| Audit × Post | -0.072 | 0.167 | -0.058 | -0.075 | -0.008 |
| | (0.097) | (0.127) | (0.113) | (0.125) | (0.095) |
| $R^2$ | 0.72 | 0.70 | 0.71 | 0.63 | 0.61 |
| *Mean Dep. Var.* | 0.11 | 0.40 | 0.38 | 0.42 | 0.48 |
| *SD Dep. Var.* | 0.14 | 0.15 | 0.15 | 0.16 | 0.13 |
| Observations | 484 | 371 | 484 | 470 | 484 |
| Num. of Clusters | 36 | 26 | 36 | 33 | 36 |
| Muni. × Cohort FE | X | X | X | X | X |
| Year × Cohort FE | X | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1, where t is a *Latinobarómetro* survey year. The unit of observation is municipality-year-cohort. The dependent variables are standardized outcomes from the *Latinobarómetro* survey. *Problem* in Column 1 indicates the share of survey respondents who think corruption is the most important problem faced by the country. *Progress* in Column 2 is the answer to the question of whether there was progress made in reducing corruption in the past 1-2 years (scale of 0 to 1, 0 means no progress made and 1 means much progress made). Columns 3-5 report levels of trust in institutions (the Congress, the federal government and the judiciary respectively), where 0 means no confidence at all and 1 means a lot of confidence. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period is after the period of the audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

# C Major-Career Mapping

In this appendix, I discuss in greater detail the mapping of different fields of study to public sector careers among Brazilian students. I focus on the baseline group of students enrolled in higher education in the year 2010 who were not exposed to audits in 2011-2014 before college enrollment. Note that due to data availability of RAIS and data attrition as explained in footnote 32, I trace about 70,000 students (8.3%) of the 2010 enrollment cohort to the formal labor market.

**Figure C1:** Mapping of Majors to Early Careers




**(A)** Public Sector (All)

**(B)** Civil Service

**(C)** Executive Branch

**(D)** Legislative/Judiciary Branch

*Notes:* This figure illustrates the shares among students enrolled in each major who end up finding their first job in the public sector, calculated using the sub-sample of students enrolled in higher education in the baseline year 2010 and traced to RAIS as explained in section 3.2. Panel A displays the shares for all public sector workers. Panel B displays shares as civil servants. Panel C and Panel D display the executive branch and non-executive (legislative or judiciary) branch separately.

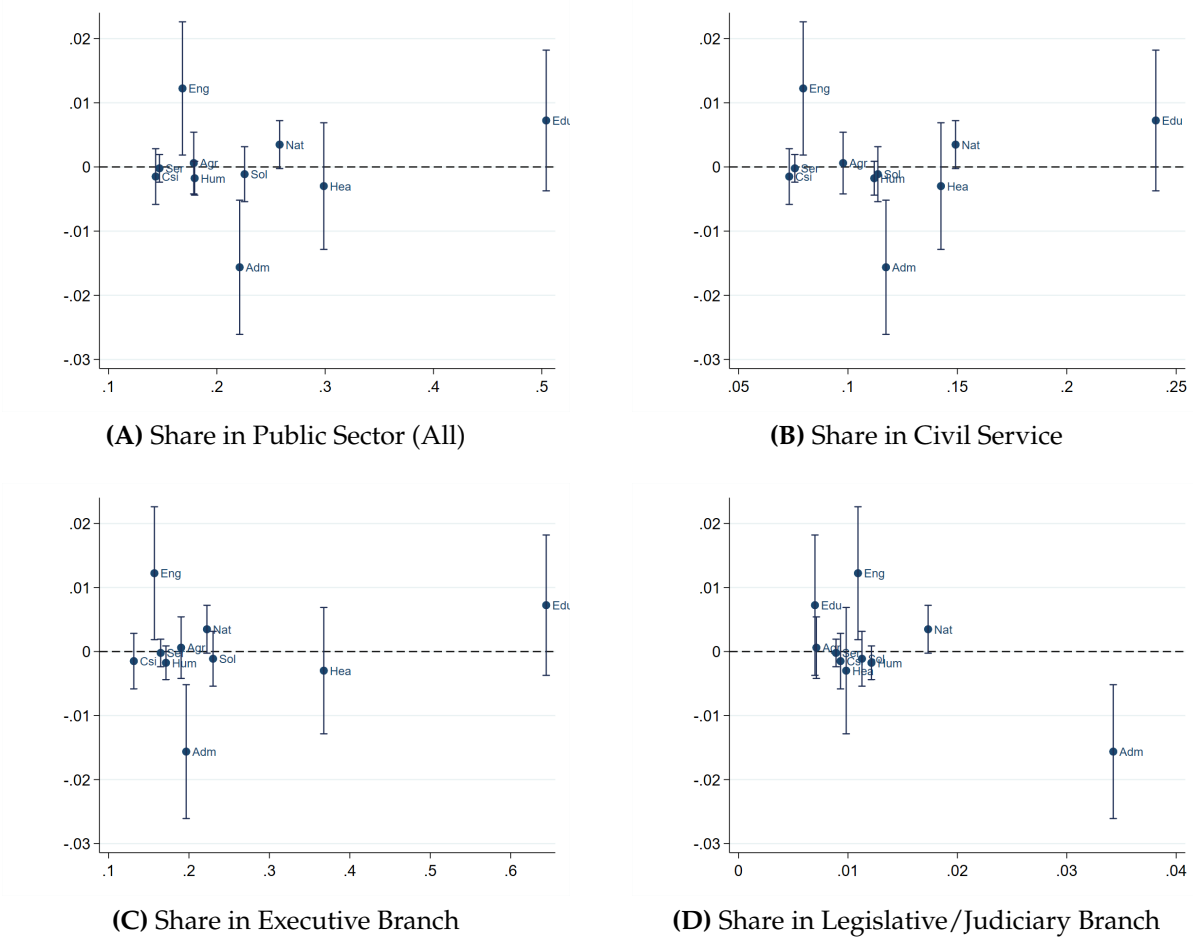
Using the sample of students enrolled in higher education in the baseline year 2010 and later appearing in RAIS, I construct a mapping from majors to early careers (demeaned shares of students who end up in the corresponding public sector positions by major enrolled) as illustrated in Figure C1. One can see that degrees in education and medicine are in general well-represented in public sector careers (Panels A, B and C) while business/law degrees stand out particularly for the legislative or judiciary branches of the government (Panel D). As a comparison, engineering degrees are overall under-represented across different public sector positions.

In Figure C2 below I further plot these shares against the difference-in-differences estimates obtained via equation 1 when the outcome of interest is the enrollment share in the corresponding major. Among the four figures, Panel D illustrates the sharpest negative correlation: majors that are more represented in the legislative or judiciary career (such as business/law) see more of a brain drain following anti-corruption audits. On the contrary, fields that are under-represented in any public sector careers (engineering in particular) see the largest growth in terms of size of enrollment after the audits. From an ex-post point of view, these patterns of correlation provide additional justification for the focus on the comparison between enrollment in business/law versus engineering in the main analysis in section 4.2.

However, it is worth noting that education as a field of study stands out as an exception. As highlighted by Panels A, B and C, students with backgrounds in education are generally well-represented in public sector careers. Results from Appendix Table A2 show that audits have a slight positive effect on enrollment in education, although the coefficient is imprecisely estimated. Several reasons could justify the "outlier" behavior of the education major. From the perspective of students, a large fraction of students studying education presumably end up becoming public school teachers, who are civil servants in Brazil and are (de jure) selected based on meritocratic exams (similarly for health care providers). Bureaucratic corruption involving misappropriation or embezzlement of fiscal transfers might be less relevant for frontline providers such as public school teachers, whose main source of income is the contractual wage. If anything, students who aspire to become public school teachers could benefit from a reduction in bureaucratic corruption due to improved allocation of school funds (Ferraz et al., 2012). An alternative explanation is that compared to other fields, degrees in education are widely available (Appendix Figure A3 shows education is one of the most popular major choices) and serve as closer substitutes for degrees in business/law. Lastly, education is also more susceptible to changes on the hiring side. In the case of Brazil, existing research (Gonzales, 2021; Akhtari et al.,

2022) has documented that patronage hiring is prevalent among public school personnel (such as school principals and teachers). All the reasons listed above highlight that degrees in education should be treated as a special case as opposed to other fields of study. Unfortunately, I do not observe the specific occupations (such as teachers or health care workers) in the current CES-RAIS linked sample to rigorously examine the effects of audits on talent allocation toward careers in specific sectors, and thus cannot further disentangle these possible explanations.

**Figure C2:** Effects of Audits on Major Enrollment and Career Prospects




**(A)** Share in Public Sector (All)

**(B)** Share in Civil Service

**(C)** Share in Executive Branch

**(D)** Share in Legislative/Judiciary Branch

*Notes:* This figure plots the shares among students enrolled in each major who end up finding their first job in the public sector, against stacked difference-in-difference coefficients together with 95% confidence intervals estimated via equation 1 for the corresponding major. Panel A displays the shares for all public sector workers. Panel B displays shares as civil servants. Panel C and Panel D display the executive branch and non-executive (legislative or judiciary) branch separately.

## D Audits and Out-Migration

In this appendix, I discuss anti-corruption and students' decisions on out-migration. As emphasized in the main paper, throughout the empirical analysis whether students are exposed to CGU audits is defined by whether they enroll in higher education after an audit occurs in their reported municipality of residence at the end of high school (subsequently referred to as a student's "home" municipality). The definition of treatment status is irrespective of the locations where students go to university or work.

While ensuring consistency of analysis on higher education and labor market outcomes, a related concern remains whether the effects I observe on talent sorting can be a mechanical outcome following selective out-migration driven by the audits. Specifically, if students simply leave their home municipalities after an anti-corruption out of reasons such as a distaste for local corruption, the spatial relocation itself might induce changes in major preferences because students might choose majors that could maximize their labor market prospects (such as STEM majors) facing an alien labor market in the new location.

To examine to what extent this claim can be true, I provide some reduced-from evidence on audits and migration using the stacked-by-event estimation method elaborated in section 4.1. First, I do find evidence of selective out-migration for work after the audits, as summarized in Table D1. Column 1 suggests students are less likely to end up working in their home municipality following an audit, and the out-migration occurs for both civil servants and those who end up in the private sector. Students also tend to work outside of their home state (column 2), even though the estimates are less precise. The results should be interpreted with caution as migration could occur prior to career realization (such as during the college enrollment phase), or it could be a byproduct of career allocation itself which is also endogenously responding to the audits.

Next, I re-produce my baseline results on major enrollment when migration is taken into account, to examine whether the effects are driven by selective migration following the audits. The results are presented in Table D2. Reassuringly, the major switching pattern I observe at the baseline persists when I look at non-migrants ("stayers") and migrants ("movers") separately. If anything, the reduction in business/law enrollment is sharper for stayers, suggesting that out-migration is unlikely to be driving the changes in talent sorting across fields of studies following the audits.

**Table D1:** Effect of Audits on Out-Migration for Work

| | Workplace Muni. and Residence Muni. | |
|---|---|---|
| | In the Same Muni. | In the Same State |
| | (1) | (2) |
| **Panel A: Public Sector (Civil Servants)** | | |
| Audit × Post | -0.112** | -0.060 |
| | (0.056) | (0.103) |
| $R^2$ | 0.74 | 0.65 |
| *Mean Dep. Var.* | 0.30 | 0.47 |
| *SD Dep. Var.* | 0.39 | 0.48 |
| Observations | 26,906 | 26,906 |
| Num. of Clusters | 1,404 | 1,404 |
| **Panel B: Private Sector** | | |
| Audit × Post | -0.058* | -0.015 |
| | (0.033) | (0.022) |
| $R^2$ | 0.61 | 0.53 |
| *Mean Dep. Var.* | 0.49 | 0.86 |
| *SD Dep. Var.* | 0.27 | 0.27 |
| Observations | 66,706 | 66,706 |
| Num. of Clusters | 2,525 | 2,525 |
| Muni. × Cohort FE | X | X |
| State × Year × Cohort FE | X | X |

*Notes*: This table evaluates the effects of audits on the probability of out-migration, conditioning on the type of occupation. The table reports coefficients obtained from the estimation of equation 1. The dependent variable for column 1 is the share of workers working in the same municipality as their home municipality (defined as place of residence the year before college enrollment) out of all workers from the same origin municipality who appear in RAIS. Column 2 reports for the same indicator but for states. Panel A reports the sample of students who end up in civil service, and Panel B includes the sample of students who end up in the private sector. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

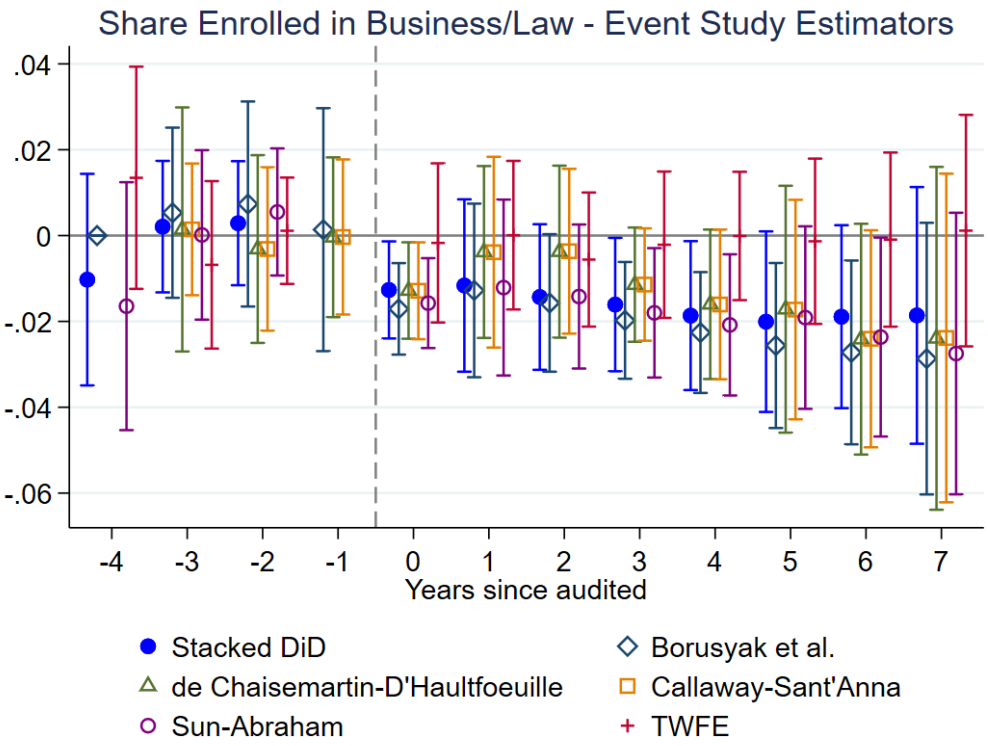
**Table D2:** Effect of Audits on Major Enrollment by Migration Status

| | Total Num. (log) (1) | Share in Business/Law (2) | Share in Engineering (3) |
|---|---|---|---|
| **Panel A: Work Muni. Same as Residence (Stayers)** | | | |
| Audit × Post | 0.145 | -0.136** | 0.095** |
| | (0.144) | (0.054) | (0.045) |
| $R^2$ | 0.92 | 0.29 | 0.32 |
| *Mean Dep. Var.* | 2.53 | 0.31 | 0.16 |
| *SD Dep. Var.* | 1.43 | 0.22 | 0.17 |
| Observations | 56,917 | 56,917 | 56,917 |
| Num. of Clusters | 2,271 | 2,271 | 2,271 |
| **Panel B: Work Muni. Different Than Residence (Movers)** | | | |
| Audit × Post | 0.223 | -0.061 | 0.073*** |
| | (0.156) | (0.038) | (0.028) |
| $R^2$ | 0.93 | 0.27 | 0.29 |
| *Mean Dep. Var.* | 2.59 | 0.27 | 0.17 |
| *SD Dep. Var.* | 1.52 | 0.21 | 0.17 |
| Observations | 65,660 | 65,660 | 65,660 |
| Num. of Clusters | 2,529 | 2,529 | 2,529 |
| Muni. × Cohort FE | X | X | X |
| State × Year × Cohort FE | X | X | X |

*Notes*: The table reports coefficients obtained from the estimation of equation 1. The dependent variable for column 1 is the (log) total number of students showing up in RAIS. Columns 2 and 3 report results on the shares of enrollment in business/law and engineering separately. Panel A reports the sample of stayers (those who work in their residence municipality at the time of the college enrollment) while Panel B includes the sample of students who migrated for work to a different municipality. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

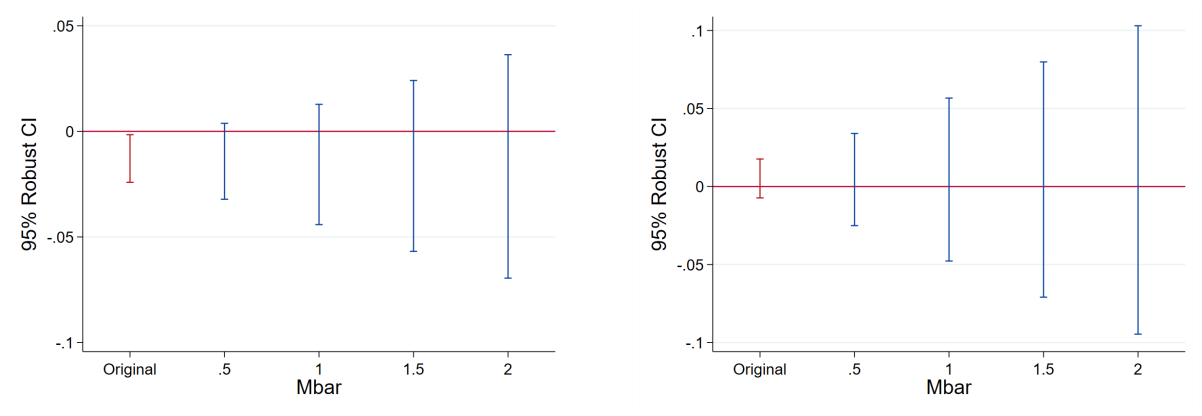
# E Robustness Checks

**Figure E1:** Audits and Share of Major Enrollment - Alternative Estimators




*Notes:* This figure presents event study estimators for the effects of audits on shares of freshmen major enrollment (pooling public and private universities), comparing the convential TWFE estimator and stacked DiD estimator with alternative estimators following Callaway and Sant'Anna (2021), Sun and Abraham (2021), Borusyak et al. (2021), and De Chaisemartin and d'Haultfoeuille (2020).

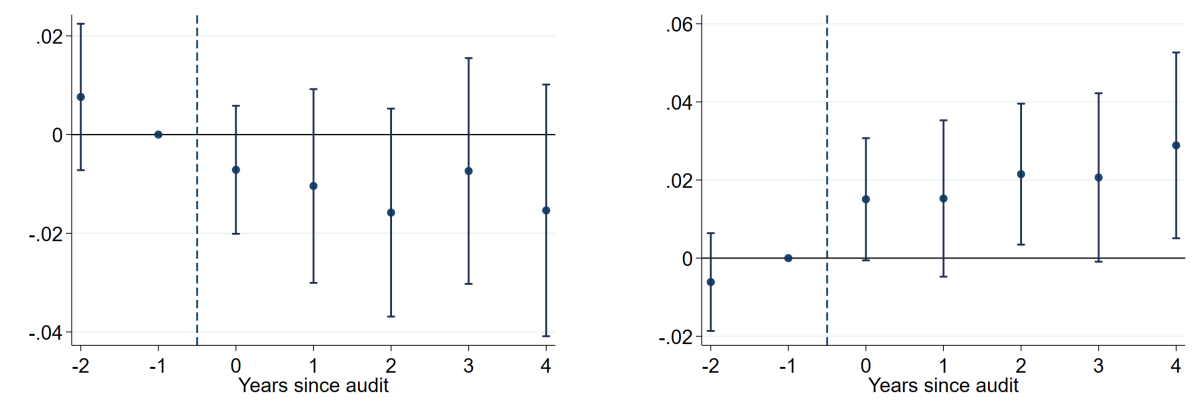
**Figure E2:** Parallel Trends Sensitivity Analysis via Rambachan and Roth (2023)




**(A)** Share in Business/Law **(B)** Share in Engineering

*Notes:* This figure reports coefficients obtained from the estimation of equation 2 and confidence sets under varying restrictions on possible differences in trends, applying the HonestDiD package provided in Rambachan and Roth (2023) to the estimator proposed in Callaway and Sant'Anna (2021). The sample includes all students pooling public and private universities. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

**Figure E3:** Audits and Major Enrollment - Balanced Panel




**(A)** Share in Business/Law **(B)** Share in Engineering

*Notes:* This figure reports coefficients obtained from the estimation of equation 2, where the sample is restricted to the balanced panel and the time window is [-2,4]. The sample includes all students pooling public and private universities. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

**Table E1:** Poisson Regression and Implied Proportional Effects

| | (1) | (2) | (3) |
|---|---|---|---|
| **Panel A: Major Enrollment** | All Students | Business/Law | Engineering |
| Audit × Post | 0.086*** | -0.012 | 0.337*** |
| | (0.031) | (0.032) | (0.071) |
| Implied Prop. Effect | 0.090*** | -0.012 | 0.401*** |
| | (0.034) | (0.032) | (0.100) |
| *Mean Dep. Var.* | 982.01 | 300.59 | 171.25 |
| *SD Dep. Var.* | 1338.89 | 409.65 | 241.89 |
| Observations | 155,920 | 155,920 | 155,240 |
| Num. of Clusters | 3,693 | 3,693 | 3,674 |
| **Panel B: Career Realization** | All Workers | Public Sector | Private Sector |
| Audit × Post | 0.314*** | -0.204 | 0.376*** |
| | (0.116) | (0.235) | (0.139) |
| Implied Prop. Effect | 0.427*** | 0.013 | 0.522*** |
| | (0.169) | (0.344) | (0.216) |
| *Mean Dep. Var.* | 64.38 | 9.26 | 55.37 |
| *SD Dep. Var.* | 102.35 | 18.18 | 89.67 |
| Observations | 82,468 | 76,627 | 80,590 |
| Num. of Clusters | 2,898 | 2,548 | 2,771 |
| Muni. × Cohort FE | X | X | X |
| State × Year × Cohort FE | X | X | X |

*Notes*: Compared to the baseline estimates reported in Table 2 and 4, where dependent variables are numbers reported in inverse hyperbolic sine transformation, in this table dependent variables are the raw numbers and the coefficients are estimated using Poisson quasi-maximum likelihood estimation (QMLE). The second row shows the implied estimate of the proportional effect $E[Y(1) - Y(0)]/E[Y(0)]$, calculated as $exp(\hat{\beta}) - 1$ and interpreted as the percentage change in the average outcome between treatment and control (Chen and Roth, 2022). Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period is after the period of audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table E2:** Effect of Audits on Major Enrollment - Tracked Sample

| | Business/Law | | | Engineering | | |
|---|---|---|---|---|---|---|
| | Share (1) | Num. (asinh) (2) | Num. (log) (3) | Share (4) | Num. (asinh) (5) | Num. (log) (6) |
| Audit × Post | -0.071*** | -0.045 | -0.045 | 0.069*** | 0.556*** | 0.681*** |
| | (0.024) | (0.120) | (0.140) | (0.017) | (0.154) | (0.188) |
| $R^2$ | 0.29 | 0.91 | 0.92 | 0.33 | 0.89 | 0.88 |
| *Mean Dep. Var.* | 0.29 | 2.59 | 2.22 | 0.16 | 2.08 | 1.95 |
| *SD Dep. Var.* | 0.19 | 1.57 | 1.37 | 0.14 | 1.55 | 1.28 |
| Observations | 82,468 | 82,468 | 45,833 | 82,468 | 82,468 | 28,975 |
| Num. of Clusters | 2,898 | 2,898 | 1,979 | 2,898 | 2,898 | 1,343 |
| Muni. × Cohort FE | X | X | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X | X | X |

*Notes*: This table illustrates the effects of the audit on major enrollment, restricting the sample to students tracked to the labor market as described in section 3.2. The table reports coefficients obtained from the estimation of equation 1. The unit of observation is municipality-time-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table E3:** Effect of Audits on Shares of Major Enrollment - Robustness

| | Balanced Panel | | Time is Semester | | Hybrid-Included | |
|---|---|---|---|---|---|---|
| | Bus./Law (1) | Eng. (2) | Bus./Law (3) | Eng. (4) | Bus./Law (5) | Eng. (6) |
| Audit × Post | -0.015* | 0.023** | -0.018*** | 0.014** | -0.015*** | 0.011* |
| | -0.008 | -0.009 | (0.007) | (0.006) | (0.006) | (0.006) |
| $R^2$ | 0.58 | 0.73 | 0.43 | 0.55 | 0.63 | 0.75 |
| *Mean Dep. Var.* | 0.3 | 0.16 | 0.31 | 0.16 | 0.30 | 0.16 |
| *SD Dep. Var.* | 0.08 | 0.08 | 0.10 | 0.09 | 0.08 | 0.08 |
| Observations | 154,828 | 154,828 | 375,672 | 375,672 | 163,726 | 163,726 |
| Num. of Clusters | 3,600 | 3,600 | 3,871 | 3,871 | 3,836 | 3,836 |
| Muni. × Cohort FE | X | X | X | X | X | X |
| State × Time × Cohort FE | X | X | X | X | X | X |

*Notes*: This table illustrates the robustness of the main effects of the audit on shares of major enrollment, for business/law and engineering separately. The table reports coefficients obtained from the estimation of equation 1. The unit of observation is municipality-time-cohort, where the unit of time is the year for columns 1-2 and 5-6, and the semester (half-year) for columns 3-4. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the year (semester) is after the year (semester) of audit. In columns 1 and 2, the sample is restricted to the balanced panel and the time window is [-2,4]. In columns 3 and 4 the time unit is semester and the panel is balanced with the time window [-3, 7]. In columns 5 and 6, municipalities audited in the hybrid phase (2015-2018) are included in the control group. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table E4:** Effect of Audits on Early Careers - Full Post

| | Realizations of First Jobs by Sector | | | | | |
|---|---|---|---|---|---|---|
| | Public Sector | | | Private Sector | | |
| | Share (1) | Num. (asinh) (2) | Num. (log) (3) | Share (4) | Num. (asinh) (5) | Num. (log) (6) |
| Audit × Post | -0.015 | -0.171 | -0.064 | 0.015 | 0.173** | 0.170** |
| | (0.020) | (0.126) | (0.099) | (0.020) | (0.069) | (0.072) |
| $R^2$ | 0.66 | 0.84 | 0.85 | 0.66 | 0.95 | 0.95 |
| *Mean Dep. Var.* | 0.22 | 1.93 | 1.65 | 0.78 | 3.56 | 3.00 |
| *SD Dep. Var.* | 0.25 | 1.40 | 1.20 | 0.25 | 1.71 | 1.61 |
| Observations | 83,034 | 83,034 | 50,355 | 83,034 | 83,034 | 67,182 |
| Num. of Clusters | 2,927 | 2,927 | 2,198 | 2,927 | 2,927 | 2,561 |
| Muni. × Cohort FE | X | X | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1, where Post is a dummy that equals 1 for the full period after the time of treatment as opposed to $[t+4, t+7]$ only as in main Table 4. Dependent variables are the share of students in the public sector (column 1) versus the private sector (column 6) as well as the corresponding total number of students (reported in inverse hyperbolic sine transformations in columns 2 and 5, and in log transformations in columns 4 and 6). The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table E5:** Audits and Civil Servants - Alternative Timespans

| | full post (1) | 3 years + (2) | 4 years + (3) | 5 years + (4) |
|---|---|---|---|---|
| **Panel A: Num. of Civil Servants (asinh)** | | | | |
| Audit × Post | -0.211 | -0.374* | -0.241 | 0.304* |
| | (0.147) | (0.223) | (0.223) | (0.160) |
| $R^2$ | 0.81 | 0.81 | 0.81 | 0.81 |
| *Mean Dep. Var.* | 1.38 | 1.38 | 1.38 | 1.38 |
| *SD Dep. Var.* | 1.30 | 1.30 | 1.30 | 1.30 |
| Observations | 83,034 | 82,609 | 82,468 | 82,351 |
| Num. of Clusters | 2,927 | 2,907 | 2,898 | 2,887 |
| **Panel B: Share of High-Ability (Top 25%) Among Civil Servants** | | | | |
| Audit × Post | -0.035 | 0.023 | -0.197*** | -0.218** |
| | (0.049) | (0.133) | (0.074) | (0.092) |
| $R^2$ | 0.50 | 0.50 | 0.50 | 0.50 |
| *Mean Dep. Var.* | 0.38 | 0.38 | 0.38 | 0.38 |
| *SD Dep. Var.* | 0.36 | 0.36 | 0.36 | 0.36 |
| Observations | 26,946 | 26,760 | 26,701 | 26,663 |
| Num. of Clusters | 1,434 | 1,409 | 1,395 | 1,386 |
| Muni. × Cohort FE | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1, where the definition of Post varies across columns. Column 1 reports results when no restrictions are made on the post period ([t+0, t+7]). Columns 2, 3 and 4 report the estimates when Post is restricted to be from 3 years, 4 years and 5 years onwards since the audit, respectively. The dependent variable in Panel A is the total number of students becoming civil servants, reported in inverse hyperbolic sine (IHS) transformations. In Panel B the dependent variable is the share of civil servants from the top quartile of the ENEM grade distribution. The unit of observation is municipality-year-cohort. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table E6:** Audits and Civil Servants - Alternative Sample Restrictions

| | full sample | $n > 0$ | $n > 1$ | $n > 2$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel A: Num. of Civil Servants (asinh)** | | | | |
| Audit × Post | -0.241 | 0.151 | 0.083 | 0.284** |
| | (0.223) | (0.187) | (0.238) | (0.120) |
| $R^2$ | 0.81 | 0.82 | 0.82 | 0.84 |
| *Mean Dep. Var.* | 1.38 | 2.11 | 2.35 | 2.67 |
| *SD Dep. Var.* | 1.30 | 1.03 | 0.96 | 0.83 |
| Observations | 82,468 | 26,906 | 13,591 | 7,493 |
| Num. of Clusters | 2,898 | 1,404 | 715 | 408 |
| **Panel B: Share of High-Ability (Top 25%) Among Civil Servants** | | | | |
| Audit × Post | | -0.197*** | -0.247*** | -0.215* |
| | | (0.074) | (0.096) | (0.123) |
| $R^2$ | | 0.50 | 0.57 | 0.63 |
| *Mean Dep. Var.* | | 0.38 | 0.42 | 0.44 |
| *SD Dep. Var.* | | 0.36 | 0.30 | 0.27 |
| Observations | | 26,701 | 11,369 | 6,438 |
| Num. of Clusters | | 1,395 | 615 | 362 |
| Muni. × Cohort FE | X | X | X | X |
| State × Year × Cohort FE | X | X | X | X |

*Notes*: This table reports coefficients obtained from the estimation of equation 1, where the samples differ regarding the number of civil servants observed in this municipality-year bin. The unit of observation is municipality-year-cohort. Column 1 reports results when no restrictions are made on the sample (taking into account the extensive margin). Column 2 reports results conditioning on having at least one civil servant from this municipality-year bin (the intensive margin only). Columns 3 and 4 report the estimates when the sample is further restricted to those with more than 1 and 2 civil servants. The dependent variable in Panel A is the total number of students becoming civil servants, reported in inverse hyperbolic sine (IHS) transformations. In Panel B the dependent variable is the share of civil servants from the top quartile of the ENEM grade distribution. Audit is a dummy that is 1 if the municipality was audited for the first time in the audited cohort, and 0 otherwise. Post is a dummy that is 1 if the period belongs to $[t+4, t+7]$. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.