Corruption and Talent Allocation*

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December, 2025

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Abstract

Leveraging an anti-corruption audit program in Brazil, I investigate whether corruption shifts the allocation of talent. I link administrative data on program rollout with education and employer-employee registers. Following audits, high-ability students change their college major to be less aligned with public sector careers, and they become less likely to enter public sector careers later on. Investigating mechanisms, I argue that these results are driven by the perception of lower returns to public sector careers owing to reduced rents and the risk of reputation loss. My findings highlight an understudied consequence of corruption, namely, the distortionary impacts on talent sorting.

JEL classification: D73, H83, I25, J24, O15
Keywords: Corruption, Audits, Talent Allocation, Major Choice, Public Sector

*I am grateful to Monica Martinez-Bravo, Manuel Arellano, and Cauê Dobbin for their guidance and support. I would also like to thank Dmitry Arkhangelsky, Sonia Bhalotra, Samuel Bentolila, Qianmiao Chen, Irma Clots-Figueras, Leander Heldring, Justin J. Hong, Gianmarco León-Ciliotta, Ursula Mello, Stelios Michalopoulos, Pedro Mira, Mateo Montenegro, Gerard Padró i Miquel, Mounu Prem, Rodrigo Soares, Andreas Stegmann, Liyang Sun, Silvia Vannutelli, Pedro Rey-Biel, Pedro Vincente, Noam Yuchtman, Tom Zohar, and other seminar participants at CEMFI, NEUDC, EWMES, MWIEDC, UiS Business School, SDU, Insper, EAYE, UB & IEB, and the CGU for valuable feedback. I thank the INEP center of the Ministry of Education of Brazil and the Data & AI center in Insper for their assistance in data access. The results and interpretations presented in the paper are the sole responsibility of the author and do not represent the official view of INEP. I acknowledge financial support from the Maria de Maeztu Unit of Excellence CEMFI MDM-2016-0684, funded by MCIN/AEI/10.13039/501100011033, and CEMFI. All errors are my own.

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1 Introduction

Talent is 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 can 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. However, 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 during 2003-2015. 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 in the labor market.

[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.

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 2010-2019 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 in which students are enrolled.[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 onwards, I restrict my analysis to audits conducted during 2011-2014, the later stage of the randomized phase of the entire anti-corruption program. 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 are included as "clean" controls.[7]

I begin my main analysis by documenting two empirical facts on baseline patterns of major enrollment and subsequent careers. First, among all degree fields, business ad-

[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]In my setting, not-yet-treated units within the time window of analysis are equivalent to never-treated units. 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.

ministration 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 5.7% less likely to major in business/law and 11.3% 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 60 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 muted response in aggregate public university enrollment masks significant underlying changes in student composition. Decomposition by student ability reveals a relative decline of 14.8% in the number 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 translates to early career outcomes. Overall, audits are associated with more students landing first jobs in the private sector, yet do not significantly impact the aggregate number of students entering the public sector. A closer examination of workforce composition, however, sheds light on heterogeneous responses to audits by student ability: audits lead to a 29.5% relative decline in the number of high-ability students embarking on public sector careers, driven primarily by state and municipal careers, and a 17.3% relative increase in those entering the private sector. This result mirrors the recoil of high-ability students from public-sector-oriented majors in higher education. Together, these findings illustrate a *brain drain* out of

[8]Referred to as "business/law" for simplicity in the rest of the paper. It is classified by the Ministry of Education as one of the ten broad degree fields in Brazil. Specifically, it includes two subfields of business administration and law, where the former can be further disaggregated into accounting and taxation, management and administration, finance, banking and insurance, secretary and clerical work etc.

the public sector following government anti-corruption audits.

Why would anti-corruption audits divert high-ability students away from the public sector trajectories? One 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 prosocial 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 behind the changes in talent allocation. First, at the major enrollment stage, I leverage finer event timing and find an immediate effect of audits on enrollment following audit announcements, even before the revelation of corruption in the audit reports. Moreover, these immediate effects are most salient in municipalities where the audits end up detecting a high level of corruption. The evidence is consistent with the interpretation that students hold largely accurate priors regarding the level of local corruption. The occurrence of an audit then alters the perceived rent-seeking opportunities in public sector careers via both channels of reduced corruption and increased monitoring,[12] while the implications of corruption 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 prosocial students hanker for a public career

[9] prosociality can be equated with a certain kind of intrinsic motivation where agents undertake prosocial 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.

[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.

either because they underestimate local corruption or because they aspire to make a difference, they receive no negative surprise shock at the time of the audit announcement before corruption revelations.[13] Second, at the career realization stage, I adopt a revealed preference approach and examine talent sorting into careers in Brazil that are classified as more or less prosocial. Overall, I do not find evidence of high-ability students disproportionately entering high-prosocial jobs in the private sector, providing direct evidence against motivation crowding-out. Lastly, I discuss general equilibrium considerations 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 prosocial 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 talent 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 provides empirical evidence to the long-standing theoretical literature on rent-seeking and talent allocation (Baumol, 1990; Murphy et al., 1991, 1993; Acemoglu, 1995; Torvik, 2002).[14] Shaped by a society's reward structure, talent allocation into rent-seeking activities (such as corruption) versus productive activities (such as entrepreneurship) could have long-run implications on

[13] It is important to note, however, that there could be simultaneous crowding-in of prosocial students expecting a cleaner public sector post the audits. Disentangling this channel requires information on student prosociality, which is rarely available in administrative 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 in China (Fang et al., 2023; Bai et al., 2025), the allocation of immigrant (Birinci et al., 2021) and female talent (Ashraf et al., 2022; Lee, 2024) and the macroeconomy, as well as talent allocation within organizations (Haegele, 2022).

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 a sequence of papers that document the effects of one specific manifestation of rents – natural resources such as oil or mineral rents – on political selection and talent allocation (Ebeke et al., 2015; Asher and Novosad, 2023; Balza et al., 2025), as well as Brassiolo et al. (2021), in which the authors experimentally vary "corruption" opportunities in the lab among college students in Colombia.[15] In this paper, I overcome the identification challenge by leveraging randomized audits in Brazil as a source of exogenous policy shock to (perceived) rent-seeking opportunities in the public sector, providing one of the first causal evidence of political corruption shaping the allocation of talent in a natural experiment setting.

A concurrent study by Hong (2023) examines a closely related question in the context of China's anti-corruption inspections. Using applicant data for state organizations in China, Hong (2023) finds evidence of positive selection by integrity, but no differential selection by ability into the state sector following inspections. A key feature of the Brazilian setting is that the anti-corruption audits are implemented through randomized lottery draws across local municipalities, in contrast to the nationwide crackdown in China that drastically shifted perceptions of integrity in the state sector at the societal level.[16] Moreover, I utilize economy-wide data linking the labor market records in tandem with the higher education census, and investigate sorting that occurs at an earlier stage of choosing college majors. Nonetheless, the differences in our findings regarding ability selection suggest that the extent to which corruption attracts a society's most talented individuals into the public sector is context-dependent and hinges on specific institutional environments, including baseline patterns of selection on ability and integrity into public sector careers, as well as the correlation between these traits.

By underscoring the role of self-selection in shaping talent allocation toward the public sector, this paper also 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

[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] This distinction in the institutional background implies that young people in China expected widespread enforcement ultimately "treating" everyone, while in Brazil the risk of audits became much more salient only after a municipality was drawn for audit, a relatively small ex-ante probability (60 out of more than 3,000 municipalities per wave on average during 2011-2014)

and public performance (Dal Bó et al., 2013; Deserranno, 2019; Ashraf et al., 2020; Dahis et al., 2020; Weaver, 2021; Mocanu, 2024). 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 realized careers.

This paper also speaks to the vast literature on political corruption and the effects of anti-corruption policies.[17] Several recent studies emphasize the pernicious impacts 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 both elected politicians (Ferraz and Finan, 2008; Cavalcanti et al., 2018) and bureaucrats (Santos and Leon, 2024),[18] reduces subsequent corruption (Avis et al., 2018) and clientelism (Bobonis et al., 2025), influences public personnel rotation via patronage ties (Gonzales, 2021; Bourlès et al., 2025) 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

[17] Some examples are Mauro (1998), Ehrlich and Lui (1999), Olken (2007), Fisman and Miguel (2007), Barr and Serra (2010), Olken and Pande (2012), Niehaus and Sukhtankar (2013), Bobonis et al. (2016), Detkova et al. (2021), Decarolis et al. (2025), and Rexer (2025).

[18] Santos and Leon (2024) finds that the same CGU audits in Brazil led to an improvement in the overall quality of municipal bureaucrats, driven by a reduction in patronage hiring. The seemingly inconsistency between their result and my finding of negative sorting on ability into the public sector can be explained by two factors. First, they examine the entire stock of the bureaucracy, while I focus on recent cohorts of college students and emphasize the role of talent supply. In particular, they find no effects on the age of new qualified hires, suggesting their results are driven by politically accountable mayors reshuffling the public workforce rather than the recruitment of new generations of talent. Second, their measure of quality is educational attainment, while I use standardized test scores to proxy for ability, focusing on a distinct margin of ability sorting conditioning on some tertiary education.

that could alter student major choice (Wiswall and Zafar, 2015, 2021; Ebeke et al., 2015; Shu, 2016; Conlon and Patel, 2022; Ersoy and Speer, 2025), 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 focusing on the prospects of extractable rents in the public sector and showing 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 downstream effects on 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.

2 Institutional Background

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 democratic country with ample state capacity to carry out top-down anti-corruption initiatives (Cuneo et al., 2023). In May 2003, the Lula government announced an anti-corruption campaign to be implemented by the CGU (*Controladoria-Geral da União*), the main anti-corruption body in Brazil established by the central government earlier that year to combat nationwide corruption. The program, named *Programa de Fiscalização por Sorteios Públicos*, aimed to audit local municipal governments for their use of federal funds. A unique feature of the audit program is that audited municipalities are randomly selected through publicly aired lotteries to ensure fairness and transparency. On average, approximately 60 municipalities are selected each

audit round, with replacement, and one or multiple rounds can be drawn each year.[19] As the program targets small and medium sized municipalities, only non-capital municipalities with a population below 500,000 are eligible for the lottery draws.[20]

From the official website of CGU, I obtain lists of municipalities drawn in all 40 lotteries conducted between 2003 and 2015.[21] Panel A in Figure 1 illustrates the yearly variation of the number of municipalities audited during the randomized phase. Evidently the program was more intense during the first half of the campaign, with 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 2011-2014.[22] In total, my sample consists of 6 lottery draws and 323 municipalities, out of 1,949 municipalities that are audited at least once during 2003-2015. 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 before 2011.

Once a municipality is announced to be audited, the CGU gathers information on all federal funds transferred to the municipal government in the past 3-4 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.[23] Importantly, auditors also meet with members of the local community to gather direct complaints about any malfeasance. Within weeks of the inspection, a detailed report containing all irregularities found is submitted to the central CGU office and further distributed to other federal and state agencies responsible for investigating and punishing illicit acts in the political and public spheres. Panel B of Figure 1 shows the total number of corruption irregu-

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

[20] 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.

[21] It is worth noting that, as of 2015, the program was upgraded and renamed the Inspection Program in Federative Entities (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 purely randomized lottery draws.

[22] 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.

[23] 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.

larities detected over the years, where I follow Avis et al. (2018) and measure corruption based on whether irregularities are classified as moderate or severe.[24] Finally, the report is disclosed to the public media approximately six to eight months after the audit announcement, followed by anti-corruption enforcement activities. Common consequences of the audits include temporary or indefinite suspension of officials from public posts, the impeachment of the mayor by the local office, or the mayor's loss of mandate (Colonnelli and Prem, 2022).

The CGU audit program have been studied extensively, both in terms of how the information obtained in the audits influences 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, previous studies have documented the role of local media as a key channel for citizens to learn about audit results as well as subsequent legal action against corrupt politicians and officials in their home and surrounding municipalities.[25] Although there is no direct evidence showing that citizens learned about the audits, 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. (2025) 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 previous literature, in Online Appendix B, I provide some suggestive evidence using *Latinobarómetro* surveys that the 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 the local municipal 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 represent a much larger share of the higher education market, with a total number of 2,100 universities and

[24] Irregularities are typically classified by auditors into three categories: mismanagement (such as errors in documents), moderate corruption, and severe corruption. Although all audit reports are posted online, starting with lottery 20 in March 2006, the CGU began to code the information used for the reports.

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

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 tend to be 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 Brasília, for instance). In other words, students potentially take into account career paths to pursue (a lawyer, economist, engineer, teacher etc.) at the time of their college application. A bachelor's degree generally takes about 4 to 6 years to finish, with the exact duration varying across fields of study.[26]

Before 2010, college admissions in Brazil were fully decentralized, in which students applied for degree programs months before institution-specific exams called *Vestibular*. In 2010, the Ministry of Education carried out centralization reforms, introducing the digital college application platform called the Unified Selection System (SiSU). Public (mostly 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, may 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). In sum, *Vestibular* exams are tailored to specific institutions, while ENEM is more general-purpose and tests general knowledge across subjects.

The academic year in Brazil typically runs from March to December. In general, students take the ENEM exam in November or December of the year when they are about to graduate high school. They can then use the test scores to enroll in universities in the following academic year, which normally begins in February or March. Under SiSU, candidate students can select up to two course (institution-major) options while the system calculates and updates daily the cut-off score based on the number of vacancies and the total number of applicants.[27] Some universities also open up second rounds of admissions in July and August, with the exact admission dates varying by institution.

[26] 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.

[27] Unlike the centralized system using ENEM and SiSU, students who take the *Vestibular* must choose their majors by the time they sign up for the exam. In other words, SiSU grants students more flexibility in modifying their institution-major choice.

2.3 Public Sector Careers in Brazil

As of 2018, public employees (federal, state and municipal) made up about 19% of the entire Brazilian workforce. The majority of the public sector workforce are selected via a highly competitive public contest (*Concurso Público*), which generally consists of a screening stage of basic academic credentials as well as both written and oral exams. These public contests 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 offer greater job security and a significant wage premium (Cavalcanti and Santos, 2021; Jales et al., 2024). As a consequence, public sector jobs are highly competitive, especially for state and federal level positions, with an average probability of being hired around 4% (Mocanu, 2024).

There are, however, distinct types of contracts for public employment. Public sector workers recruited through the merit-based *concursos* are called tenure-track civil servants. They can 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.[28] A different group of public sector workers can be directly appointed without a civil service exam.[29] This type of 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; Toral, 2024). At the end of 2018, workers hired via discretionary contracts accounted for only about 13.6% of the total public workforce and yet 51% of all new public contracts generated.

3 Data & Descriptive Statistics

3.1 Data Sources

To construct my main dataset, I combine several sources of individual-level data listed as follows.

[28] See example news reports 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 and https://www.gov.br/cgu/pt-br/assuntos/noticias/2019/01/governo-federal-expulsa-643-servidores-em-2018-por-praticas-ilicitas.

[29] I follow Colonnelli et al. (2020b) and classify meritocratic or discretionary jobs based on the variable Contract Type (*Tipo de Vinculo*) in RAIS. Discretionary jobs can include temporary public sector jobs as well as appointmented-based jobs such as commissioned posts or positions of trust, but there is lack of information to disaggregate the specific categories.

ENEM: This dataset includes the universe of students who participate in the annual national high school exit exam called ENEM (*Exame Nacional do Ensino Médio*), with records on subject-specific test scores along with a socioeconomic questionnaire. Participation in ENEM is not mandatory yet has become increasingly prevalent after the 2009 reform, which made ENEM scores a requirement for applying to public universities as well as for soliciting loans and scholarships to attend private universities. In principle, students can use ENEM test scores from a given academic year to apply for university admission in the following year. I observe the universe of students taking the exam for the period 2009-2018, which corresponds to university enrollment seasons in 2010-2019. I also observe students' geographic location (municipality of residence) at the time of participating in the exam. Lastly, given standardized format of the exam, I use 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 the specific institutions and degree programs in which they are enrolled, as well as their status of enrollment (actively enrolled, dropped out, or graduated). In line with ENEM, I observe the census data for the period 2010-2019. The data is considered to be of very high quality, as most institutions have their systems 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 degree courses 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*), available from 2010 to 2019. 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, covering both the private and public sector, with information on contract details, hiring and firing dates, detailed occupations, and wages. By linking students from the higher education data to RAIS, I create a mapping of degree enrollment and student demographics to their early career employment outcomes.

All the individual-level datasets listed above are available at the National Institute of Educational Studies and Research (INEP), under the Brazilian Ministry of Education.[30] Individual identifiers (pseudo-social security number) are provided to merge across datasets,

[30]The individual level 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.

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 during the hybrid phase (2015-2018). I focus on municipalities audited during 2011-2014 (corresponding to lotteries numbered 34-39), together with detailed audit reports for each audited municipality. The reports contain information on the total amount of federal transfers audited, the sectors audited, and an itemized list describing each irregularity uncovered. Following Avis et al. (2018), I classify each irregularity as either an act of mismanagement or corruption. I then merge the municipal-level panel with the occurrence of CGU audits to 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 datasets are made publicly available by IBGE, the Brazilian Institute of Geography and Statistics. Finally, I use the *Latinobarómetro* survey (2001-2020) which records a range of public opinions on corruption and trust in institutions for a representative sample of Brazilian population in each survey year.

3.2 Sample and Descriptive Statistics

To start with, I focus on the sample of freshmen (first-year) students who enrolled in higher education in Brazil during 2010-2019. The sample is restricted to students who took the ENEM exam, for whom I observe key information such as their geolocation (municipality of residence) and measure of ability (ENEM test scores).[31] For this subset of students, I observe the major(s) they are enrolled in higher education and calculate the shares of each major enrollment among all students from the same municipality.[32] The classification of majors is based on the 2018 edition of the International Standard Classification of Education Adapted for Undergraduate and Sequential Courses.[33] Appendix Figure A1 plots the yearly trend of major enrollment in higher education in Brazil. Overall, the period 2010-2014 witnessed a slight rise in enrollment in STEM degrees, mainly driven by engineering, followed by a decline post-2015. Enrollment in other popular degree choices,

[31] Despite being driven by data limitation, this sample restriction also effectively excludes students who take only the *Vestibular* exam to enroll in higher education, as they tend to have less elastic major choices.

[32] Students who are enrolled in more than one major are counted multiple times when calculating the aggregate major enrollment.

[33] 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).

such as business/law, remains relatively stable over the period of study.

The final sample consists of nearly 18 million observations at the student-major level that I observe beginning the high school exit exam. For the full sample of freshmen students, I classify students as *high-ability* or *low-ability* based on whether their ENEM performance falls into the top 25% or bottom 50% among their cohort. Lastly, all individual-level datasets are aggregated to a panel of 3,630 municipalities observed over the period 2010-2019.

For the second part of the analysis, I trace students to RAIS and observe their early career outcomes in the formal labor market. Given the available timespan of the data, I can observe a subset of students who enrolled in universities during 2010-2019 and subsequently appeared in RAIS. Appendix Figure A2 illustrates the share of students traced to RAIS by year of enrollment (Panel A) and by degree enrolled (Panel B). One can see that more than 25% of students who enrolled in universities in 2010 entered the formal labor market within nine years, whereas less than 2% of students enrolled in 2019 are found in RAIS by the end of 2019.[34] In addition, Panel B shows that the attrition rate is similar for business/law and engineering degrees in the baseline. In total, I manage to trace about 11.7% of the sample from the first part of the analysis.[35] I then classify public versus private sector workers based on contract types recorded in RAIS and calculate the share of students who obtain their first formal job in the public versus private sector among those from the same origin municipality. Appendix Table A1 summarizes characteristics of the 2010 student cohort by whether they eventually enter the public or private sector. The second row suggests that, on average, it takes more time for students to prepare and embark on careers in the public (4.7 years) than the private sector (3.7 years).

Table 1 presents the summary statistics comparing treatment and control municipalities for the period 2011-2014. Panel A reports characteristics from the 2010 population census as well as the 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, as the treated group. In compari-

[34] To capture students' intended major choice 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 implies that students traced in RAIS can include college dropouts as well as students working part-time.

[35] Out of the 11.7%, about 4.3% (37% of traced students) are first-hire contracts defined by whether the variable Type of Admission (*Tipo de Admissão*) in RAIS equals one. Several factors could explain the seemingly low fraction of students traced to RAIS. First, students could still be pursuing college or postgraduate education, or simply experiencing unemployment spells after graduation. In particular, it can be strenuous to prepare for and succeed in public sector exams (Jales et al., 2024). Second, students who work in the informal sector or abroad do not appear in RAIS, which covers only domestic and formal sector employees.

son, the control group includes *never-audited* municipalities, meaning those eligible yet never received an audit throughout 2003-2018. Importantly, I exclude municipalities audited later in the hybrid phase during 2015-2018 to avoid the confounding role they play as the "later-treated".[36] Column 5 of Table 1 reports differences in group means and the corresponding standard errors. Out of 16 characteristics, only one (the 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, but the differences are not statistically significant. Overall, first-audited municipalities look very similar to never-audited municipalities across a range of baseline characteristics.

4 Anti-Corruption Audits and Talent Allocation

4.1 Empirical Strategy

The central part of my analysis investigates the impact of anti-corruption audits on student major enrollment and career allocation. I address this empirical question under a generalized difference-in-difference framework, exploiting the staggered nature of the randomized audit program across municipalities and years. Differently from previous studies on CGU audits using the standard two-way fixed-effect (TWFE) regression setup (Gonzales, 2021; Colonnelli and Prem, 2022), I implement a "stacked" difference-in-difference design, which estimates treatment effects based on the comparison of units switching into treatment to not-yet-treated units within the time window of interest (Cengiz et al., 2019; Deshpande and Li, 2019; Vannutelli, 2022).[37] In Appendix Figure F1, I present event-study estimated using the imputation approach (Borusyak et al., 2024) and show that the main results are robust to alternative estimation methods.

Specifically, I consider each "treatment cohort" as a separate sub-experiment. A "treatment cohort" c includes all *first-audited* municipalities at time c, together with *never-audited* eligible municipalities as "clean" controls. In the baseline specification, I consider the year as the timespan and focus on four treatment cohorts audited in 2011, 2012, 2013, and

[36] A caveat here is that municipalities audited in the hybrid phase are selected based on their propensity for corruption. The balance checks in Table 1 help alleviate this concern. In addition, I provide robustness checks when such municipalities are included in the control group in Appendix Table F2. Baseline results are similar in either case.

[37] 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, 2023), Callaway and Sant'Anna (2021), Sun and Abraham (2021), Roth et al. (2023), and Borusyak et al. (2024) for more recent discussions and reviews in the applied econometrics literature.

2014.[38] 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 municipality m for treatment cohort c measured at time t (for instance, the share of freshmen enrolled in engineering). $Audit_{mc}$ is the cohort-specific treatment indicator equal to 1 for municipalities audited in year c, and $Post_{ct}$ is the cohort-specific event time dummy equal to 1 for all periods t after audit announcement in year c.[39]

Importantly, exposure to audits is defined based on students' municipality of residence at the time they take the high school exit exam, prior to college enrollment.[40] Thus, outcomes for municipality m at year t (such as the share of freshmen enrolled in engineering) are calculated using all students who reportedly resided in municipality m at $t-1$ (the year before college enrollment).

The key parameter β captures the average treatment effect of local government audits.[41] δ_{mc} are cohort-specific municipality fixed effects that absorb any time-invariant differences in municipal characteristics. λ_{cst} are cohort-specific state-by-year fixed effects to capture common shocks within states and effectively 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.[42] Finally, standard errors are clustered at the municipality level.

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

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

[38] The same is true if the timespan is semester-based (half-year), in which case each yearly cohort could be further divided into "winter" and "summer" cohorts.

[39] The "audit announcement" corresponds to the date of the lottery draw and the subsequent arrival of auditors within weeks. Audit reports are generally made public six to eight months later (see Section 5.1 for more details). In my sample (lottery draws 2011–2014), the median time between announcement and report release is eight months, with a minimum of five months (for the 2014 wave only).

[40] The timing 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. While birthplace municipality could be used as the alternative geolocator, it is poorly recorded and about 50% of students do not reside in their birthplace by the time of the exam, according to ENEM 2010.

[41] Note that β 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).

[42] I use the baseline 2010 weights to allay the concern about endogenous weighting.

where, as before, Y_{mct} is the outcome for municipality m, treatment cohort c, and time t. The post-treatment indicator is now replaced with a series of event-time dummies D^{τ}_{ct}, spanning from k periods before to k periods after the audit. The period $\tau = -1$ is omitted as the reference period. In my main specification, I focus on $k = -4, ..., 7$, using years as the time unit.[43]

It is worth emphasizing that, I follow the exisiting literature and define the treatment based on the audits, rather than corruption per se (Ferraz and Finan, 2008; Colonnelli and Prem, 2022; Bobonis et al., 2025). This is because even though audits are random, the amount of corruption is not given that corruption is only observed conditional on audits taking place.[44] I therefore focus on the policy-relevant reduced-form impacts of the audits in the main analysis, assuming that audits are effective in detecting and reducing corruption (Avis et al., 2018), and highlight the importance of capturing effect heterogeneity by the level of corruption detected in Section 5.

The underlying identifying assumption is that, conditional on the set of municipality and time fixed effects, the timing of the audit is unrelated to municipal outcomes (such as the shares of freshmen major enrollment). Potential threats to identification include violations of the parallel trends 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. In my setting, 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. 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 baseline characteristics of *first-audited* versus *never-audited* municipalities and find few differences between the two. 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 trends assumption by analyzing the dynamics in the β_{τ} coefficients of equation 2, as I illustrate in the main

[43] This is the longest window observable given the data, although it results in an unbalanced panel. Alternatively, restricting the sample to a shorter balanced panel with $k = -1, ..., 4$ produces similar results (see Appendix Figure F3 and Appendix Table F2).

[44] By studying the early waves of audits during 2003-2005, Ferraz and Finan (2008) first documents a muted average effect of audits on electoral outcomes, before uncovering the differetial impacts by whether the corruption level exceeds voters' priors. Similarly, Colonnelli and Prem (2022) first documents a boom in economic activities following anti-corruption audits on average, and then confirms the effects are concentrated in areas with higher detected levels of corruption.

results.[45]

4.2 Audits and College Majors

Among incoming college students in Brazil, the most popular fields of study are business/law, education, health, and engineering, accounting for 30%, 22%, 15%, and 14% of total freshman enrolment in 2010, respectively.[46] In my main analysis, I focus on the comparison between changes in enrollment in business/law versus engineering following anti-corruption audits. This is motivated by two empirical patterns observed in major enrollment and subsequent career realization among university freshmen in the baseline year of 2010.

First, high-ability students (defined as those in the top 25% of the ENEM grade distribution) in Brazil face a clear choice between studying business/law or engineering. As shown in Panel A of Figure 2, these two fields are the most popular among high-ability students, together accounting for more than 40% of total high-ability enrollment in 2010. Meanwhile, low-ability students (bottom 50% of the ENEM distribution) are much more likely to study business/law, education, and health degrees compared to 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 imply that high-ability students in business/law are approximately 14.3% more likely to join civil service than those majoring in engineering, whereas the difference for low-ability students is smaller (around 8.9%). Notably, both education and medicine degrees stand out, constituting large shares of enrollment and exhibiting high propensities for careers in civil service. In Online Appendix C, I elaborate on why degrees like education, which are closely tied to frontline providers such as public school teachers, warrant additional consideration in the discussion of bureaucratic corruption.[47]

Although the decision to focus on the juxtaposition of business/law vs. engineering

[45] 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 F2.

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

[47] In Online Appendix C, I also conduct an auxiliary analysis by creating a detailed mapping from majors to alternative public sector careers and provide additional justification for focusing the main analysis on enrollment in business/law versus engineering. In particular, business/law stands out for careers in the legislative/judiciary branch of the government.

may seem ad hoc in this context, it echoes with existing literature linking rent-seeking to talent allocation in higher education (Murphy et al., 1993; Abebe et al., 2021; Balza et al., 2025).[48] Nonetheless, to address residual concerns about arbitrariness, I also construct an alternative "public sector employability" index, grouping all majors by their baseline propensity for entry into civil service. I replicate the analysis using this broader classification and discuss the results as a robustness check in Online Appendix C.

Aggregate major enrollment: Table 2 presents the main results of the effects of anti-corruption audits on freshmen major enrollment. The coefficients 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, Panel A suggests 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 experienced a decline in business/law enrollment by about 1.7 percentage points (pp) and an increase in engineering enrollment by about 1.7 pp. Relative to the mean shares of enrollment, these estimates correspond to a 5.7% decline in business/law enrollment and a 11.3% increase in engineering enrollment. A simple back-of-the-envelope calculation suggests that about 1 in 60 incoming college students eventually switch majors, with the average number of freshmen across all municipalities in 2010 being around 70 (Table 1).[49] Considering that I examine enrollment outcomes in alternative majors that are likely interdependent with each other, I also report p-values adjusted for multiple hypothesis testing (MHT) using the Romano-Wolf correction (Romano and Wolf, 2005; Clarke et al., 2020). The effects are robust to adjusting for multiple hypothesis testing across enrollment in all ten majors.[50] These results suggest that audits divert freshmen students toward relatively less public-sector-oriented majors.

Furthermore, splitting the sample into students in private and public universities (Panels B and C of Table 2) demonstrates that the effects are mainly driven by private univer-

[48] Murphy et al. (1993) documents a cross-country correlation between economic growth and relative concentrations in law vs. engineering degrees. Relatedly, Abebe et al. (2021) and Balza et al. (2025) show that natural resource rents are associated with higher enrollment in fields of study related to law, business, and social sciences, as opposed to engineering or STEM in general, using cross-country data and within-country evidence from Colombia, respectively.

[49] The magnitude is moderate compared to existing literature. For instance, exploiting an affirmative action policy in a large public Brazilian university, Estevan et al. (2019) finds that affected students are about 10% more likely to choose competitive majors (STEM and medicine), with downstream effects on actual enrollment. Another study by Balza et al. (2025) finds that local oil exploitation (proxied by the number of wells around a school) in Colombia causes a 7.6 pp decrease in the likelihood of STEM enrollment and a 7.4 pp increase in enrollment in social sciences, business, and law.

[50] As a comparison, I show the results for enrollment in other majors in Appendix Table A3. One can see a small negative effect on enrollment share in humanities, which becomes insignificant after multiple hypothesis correction (column 3), while no significant effects overall in other fields of study.

sity enrollment. This is consistent with the interpretation that public universities in Brazil are highly competitive and oversubscribed. Given that number of degree vacancies are pre-determined, total enrollment would not be affected unless audits systematically drive students driven away from public universities.[51] Meanwhile, private universities are generally undersubscribed and can flexibly accommodate students' demand for degrees.[52] As shown in Panel B, I find that the effects on major enrollment are amplified for private institutions.

I provide additional evidence that the effects on enrollment shares reflect major-switching behavior, rather than the inflow of new students. On the extensive margin, Panel A in Appendix Table A4 shows that audits have no significant impacts on overall university enrollment or on public university enrollment per se. This suggests that the observed changes in enrollment result from reallocation across fields of study within the same type of institution and the same municipality.[53] To complement the results on enrollment shares, columns 2 and 4 of Table 2 also reports estimates on the actual number of students enrolled. The dependent variables are reported with the inverse hyperbolic sine (IHS) transformation to account for the extensive margin, given the presence of zero enrollment.[54] 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 dynamic effect of audits on major enrollment, estimated from equation 2. Panel A presents results for the pooled sample, showing both enrollment shares in business/law and engineering. I observe little difference between the trends of audited and control municipalities prior to audits, supporting the parallel trends assumption. After an audit is announced, there is an immediate decline in the share of enrollment in business/law among students from audited municipalities compared to their counterparts from never-audited municipalities, while the positive ef-

[51] Appendix Table A4 shows this is not the case. In other words, students are not disproportionately selecting in or out of public universities as a response to the audits.

[52] A potential concern in interpreting the effects on overall major enrollment is that, in the presence of binding capacity constraints, the treatment mechanically displaces students across majors. If students are induced by the audits to shift toward a particular major (e.g., engineering), the fixed number of vacancies could crowd out other students and mechanically amplifying the observed treatment effect. However, this concern appears limited in the context of public universities, where enrollment quotas are rigid and no significant shifts in enrollment shares are observed. By contrast, shifts in enrollment shares are more apparent in private universities, where capacity is less constrained.

[53] An additional concern relates to changes in student composition across and within types of institutions. Panels B and C of Appendix Table A4 report the effects of audits on aggregate enrollment by ability group and find no differential effects for either subgroup.

[54] I acknowledge caveats regarding the interpretation of average treatment effects under inverse hyperbolic sine transformation as approximating percentage changes, as highlighted in Chen and Roth (2024). I provide robustness checks using alternative methods (Poisson quasi-maximum likelihood estimation or simple log transformations) in Table F1 in Online Appendix F, and the results are largely consistent.

fect on engineering enrollment emerges more gradually. Importantly, I document both short-run effects and long-run effects on younger birth cohorts, suggesting the results are not only driven by last-minute major switching. In particular, the gaps in enrollment shares persist till at least seven years after the audit, which imply long-lasting impacts of anti-corruption efforts on the local distribution of human capital across different fields of specialization.[55] Splitting the sample by institution type (Panels B and C) further strengthens the argument that the dynamic effects observed in the pooled sample can be mainly attributed to enrollment changes in private universities.

Student ability composition: A major limitation of the analysis on aggregate enrollment is that it could obscure underlying changes in within-major student composition. This concern is particularly relevant for public universities, where higher quality and prestige attracts more high-ability students, but the oversubscription feature masks shifts in student characteristics. To the extent that major choice serves as a proxy for career aspirations, my empirical setting provides an opportunity to examine how candidate pools for public and private sector career trajectories shift following anti-corruption audits. In the next step, I investigate how audits affect the ability composition of students by type of institution and by major.

Table 3 summarizes the main results for public universities, highlighting compositional changes even in a case where little changes are observed in aggregate enrollment (corresponding event-study plots reported in Appendix Figure A5). Conditional on observing positive enrollment in the institution-major category, column 1 of Panel A shows a modest decline in total business/law enrollment following the audits. However, columns 2-4 reveal significant shifts in student ability composition. Specifically, there is a 14.8% relative decline in the number of high-ability students enrolling in business/law (roughly 1 in every 6-7 high-ability students), largely compensated by a 14.1% relative increase in students from the lowest ability quartile. Panel B shows that more students are entering engineering degree programs in public universities across the ability distribution, although the estimates are less precise.[56] Results for private universities are separately reported in the Appendix Table A6, where similar patterns are observed, but the effects

[55] One possible driver of the long-run effects is the altered perception of corruption opportunities among parents and other family members (Hauk and Saez-Marti, 2002; Hong, 2023; Ang et al., 2025), who may influence student major choices. Unfortuntely, the ENEM student survey does not document whether their parents work in the public sector. Another related explanation is that audits shift social norms and stigma associated with corruption (Corbacho et al., 2016; Stephenson, 2020), with individual preferences further shaped by peer exposure and social interactions.

[56] I also report results when outcomes are measured in shares instead of IHS-transformed numbers, as in Appendix Table A5. The results are largely consistent but become less informative due to changes in the denominator. For instance, the share of high-ability students enrolling in engineering dropped, not owing to a drop in actual enrollment numbers but rather a simultaneous surge in lower-ability enrollment.

on business/law (engineering) enrollment are less (more) pronounced. Lastly, Appendix Table A7 provides a more comprehensive illustration of changes in enrollment patterns by ability groups, pooling all universities. In particular, Panel A suggests that high-ability students are leaving business/law and, by and large, reallocating toward engineering and other smaller fields (including other STEM fields).[57] In sum, these results illustrate that high-academic-performing students shy away from public-sector-oriented majors following anti-corruption audits at local municipal governments, as reflected in the changing enrollment patterns in business administration and law degrees.

4.3 Audits and Realized Careers

Given that major choice is nonetheless an imperfect proxy for career preferences, I next investigate the more first-order question of interest: do anti-corruption audits actually shift talent across public and private careers? As discussed in Section 3.2, I observe labor market outcomes for the subgroup of students who subsequently appeared in RAIS during 2010-2019. I focus specifically on the first full-time jobs students obtain in the formal labor market and define whether they work in the public or private sector based on the contract type.[58] 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.[59] One caveat in interpreting the results is that all outcomes I observe are general equilibrium outcomes, conditioning on students finding a job. Even though I do not directly observe talent supply (e.g., job application data), in Section 5.2 I provide a thorough discussion of why demand factors alone are insufficient to account for my empirical findings.

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

[57] The results underscore a strong aversion to business/law degrees following the audits, with engineering partly absorbing the outflow of talent. Relatedly, an interesting pattern is observed in Panel B, where lower ability students are increasingly crowding into engineering. This appears to be a stronger driver in the overall shift towards engineering enrollment. I interpret this result as being driven by an increasing demand for engineering degrees from private universities, as elaborated in Section 5.2.

[58] Since RAIS only records formal labor market employees, I am not able to track students who enter the informal sector, which is one reason behind the sample attrition. The informal sector accounts for a substantial share of the Brazilian economy (Ulyssea, 2018), yet existing research on anti-corruption activities and the informal sector is limited due to data availability. An exception is Colonnelli and Prem (2022), who provides suggestive evidence that the CGU audits have limited impacts on transitions between informal and formal employment.

[59] It is worth reiterating that students' exposure to audits is defined based on whether they enroll in higher education after an audit occurs in their municipality of residence, while students' ability is proxied by the ENEM scores taken before college enrollment. These key definitions are kept consistent throughout the empirical analysis.

obtaining their first jobs in the private sector, while the effect on public sector career realizations is negative yet statistically insignificant. As shown in column 1, audits do not appear to affect the fraction of students who end up in the public sector. Column 2 reports the estimates for the number of students working in the public sector, using IHS transformation to address cases where no students from a municipality are observed in the labour market. The results suggest that fewer students sort into public sector careers following the audits, although the estimates are noisy. Dividing by contract type, however, reveals hetereogeneous effects between tenure-track civil servants and temporary workers hired under discretion. Appendix Table A8 shows a marginally significant negative effect among tenure-track civil servants, while the size of temporary workerforce in fact increased.[60] On the other hand, the opposite pattern is clearly observed for students entering the private sector. Column 4 of Table 4 shows a significant increase in the number of students working in the private sector. The coefficient suggests that, in audited municipalities approximately 15% more students end up undertaking their first job in the private sector.

Complementing the estimates in Table 4, Figure 4 explores the dynamic effects of audits on realized careers. The effects of audits on the number of students employed in the public and private sectors are reported separately. One can see that a positive effect on private employment sets in among enrollment cohorts immediately following audit exposure.[61] These positive effects also persist in the "longer run" for younger enrollment cohorts. For public sector employment, however, the pattern is less clear-cut. Figure 4 shows that audits do not affect public employment until around three years after audit exposure when a negative effect kicks in. A similar pattern is observed when I focus separately on tenure-track civil servants and temporary workers (Appendix Figure A6).[62] All in all, the evidence presented in this section implies that anti-corruption audits lead to a rise in private sector employment, while the effect on public sector employment appears negative yet ambiguous. It is important to note, however, any null effect on aggregate public sector employment (quantity) could mask underlying changes in the type of individuals

[60] Furthermore, Appendix Table A9 explores alternative categorizations of public sector careers, by branch of government (executive, legislative, or judiciary), level of hierarchy (federal, state, or municipal), and occupation (bureaucrats or frontline providers), and does not uncover strong differential patterns along these dimensions.

[61] As I do not restrict the RAIS sample to students who have completed their degree, the immediate effects are likely driven by students who drop out of university or students who work full-time but enroll in part-time programs.

[62] One caveat to interpret the noisy estimates is that different event-study estimates are obtained based on slightly different samples. Thus, the estimates reflect both dynamic treatment effects and changes in cohort composition: younger enrollment cohorts have less time to appear in RAIS and are subject to higher sample attrition. I present results estimated from a balanced panel in Appendix Figure F3 as a robustness check.

selecting into the public sector (quality). In the next step, I closely examine whether audits altered the composition of both the private and public sector workforce, with a focus on differential selection by student ability.

Workforce ability composition: Given that public sector jobs in Brazil, and tenure-track positions in particular, are highly competitive and oversubscribed, they are eventually filled provided that there are no dramatic changes in government hiring practices.[63] Echoing the ealier analysis on the ability composition of students across different majors, I now explore whether audits also affect the ability composition of students entering different career paths.

Table 5 summarizes the effects of audits on the composition of the private and public sector workforce in terms of student ability.[64] Column 1 first recapitulates the effect of audits on aggregate career allocation, conditional on a positive number of students observed. Columns 2-4 then report effects separately for the numbers (IHS-transformed) of students in each career category by their relative position in the ability distribution. As shown in column 4 of Panel A, audits lead to a significant drop in the number of public sector workers coming from the top quartile of the ability distribution: a relative decline of 29.5%. Columns 2-3 reveal that this decline is much less drastic among other lower ability groups. In contrast, among students embarking on their career in the private sector, audits are associated with a relative increase of about 17.3% in the number of students from both the top two quartiles.[65] A back-of-the-envelope calculation suggests that roughly 1 in every 3-4 high-ability students are leaving the public sector.[66] In Appendix Figure A7, I provide complementary dynamic evidence with the event-study. Panels A and B show the number by sector and ability groups, while Panels C and D report corresponding shares of high-ability students. One can observe that the treatment effects are persistent and pronounced even for younger enrollment cohorts in the longer run.

Lastly, Appendix Tables A13 and A14 further explore the sorting of high-ability students into alternative categories of public careers (by contract type, branch, hierarchy, and

[63] I discuss and rule out the impact of audits on public sector hiring as a confounding mechanism in section 5.2.

[64] I also explore workforce composition in terms of demographic and socioeconomic characteristics such as gender, parental education, and family income (Appendix Table A12), but do not find strong patterns of sorting along these dimensions.

[65] It is worth emphasizing that workforce composition is observed conditioning on there being a positive number of students traced to RAIS in the corresponding year-municipality. To alleviate the concern that the effects on ability composition are driven by the subsample where outcomes are noisily measured, I provide robustness checks in Appendix Table F3 where alternative sample restrictions are applied.

[66] The coefficients in column 4 suggest $1.66 \times 0.295 = 0.49$ "missing" top students in the public sector, which by and large corresponds to $3.10 \times 0.173 = 0.54$ "extra" top students in the private sector. Effect magnitudes appear larger than those on major enrollment (Table 3) because they incorporate both the extensive margin (career-switch due to major-switch) and the intensive margin (career-switch holding majors fixed).

occupation type). In particular, the effects seem to concentrate in state and municipal careers, which also occupy larger shares of the public workforce in my sample.[67] This result is consistent with the interpretation that municipal audits raised salience about corruption in the local government, while the effects on state careers can also be explained by possible spillover effects.[68]

Overall, both results on student and workforce composition highlight the sorting of high-ability students out of public sector career trajectories in audited municipalities, illustrating a public-sector *brain drain*.

5 Drivers of Talent Allocation - Mechanisms

Talent allocation is shaped by both the supply of and demand for talented individuals pursuing public sector careers. This section discusses plausible mechanisms behind the effects of audits on talent allocation. I first lay out the main hypotheses that could be driving students' behavioral responses to audits and provide suggestive evidence in support of or against these hypotheses. I then discuss general equilibrium considerations and rule out alternative explanations related to changes in education supply or labor demand.

5.1 Talent Supply: Perceived Career Returns

Arguably, both ability and prosociality (or honesty) are key dimensions that characterize the overall quality of public personnel.[69] 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 prosocial 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

[67] The shares of municipal, state, and federal workers in my sample are 55.2%, 38.7%, and 6.1%. Typically, exams to enter federal careers are more competitive and require longer preparation, so the caveat here is that I may not observe all students who aspire to become federal workers due to higher attrition.

[68] 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 some evidence of effect spillovers for major enrollment (Appendix Table A15).

[69] An extensive literature has elaborated on the important role of these traits. 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 public service delivery 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.

corruption in the public sector would attract *rent-seekers* at the expense of *prosocial* 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 prosociality 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, the reduced-form results illustrate a brain drain from public sector careers. However, the lack of measures on prosociality or honesty from administrative data makes it challenging to pin down the mechanisms behind the ability crowd-out. On one hand, audits could lead to a perception of reduced corruption, deterring high-ability students who join the public sector for rent extraction.[70] 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 prosocial 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 been previously exposed to the audits via news reports and have updated their priors regarding the effectiveness of the audits in reducing local corruption as the program unfolds across the nation and over time.[71] However, as information on perceptions of rent-seeking and other non-pecuniary returns is 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, several pieces of evidence support the *diminished rent-seeking* and *reputation deterrence effect* hypotheses.

Immediate and heterogeneous effects following audit announcement: I begin by leveraging a finer timing to examine the baseline effects of audits on major enrollment, as illustrated by Figure 5. A time period t is now a semester, and I maintain a balanced

[70] An implicit assumption is that high academic achieving students have a comparative advantage in rent-extraction.

[71] Using survey data from the *Latinobarómetro*, Online Appendix B provides suggestive evidence that audits are associated with a (locally) improved perception of progress made combating corruption, even though audits do not seem to alter the overall high corruption perception nationwide. This suggests that the institutional environment in Brazil more closely resembles a "rent-seeking equilibrium" in which corruption attracts the corrupt (Acemoglu, 1995; Hanna and Wang, 2017)

sample of municipalities observed between $[t-3, t+7]$. The overall takeaway remains unchanged: following the audits, there is a decline in the share of freshmen enrolled in business/law and an increase in engineering. A key new message conveyed in Figure 5 is that the effects appear immediately in the semester of audit announcement ($t+0$). Note that at this stage, audit reports detailing any acts of corruption have not been released to the public, as they typically become available six to eight months after the announcement. With the information channel effectively shut down at this short interval, the immediate response suggests that students form priors about local corruption and/or the implications of the audits based on corruption revealed in earlier years of the CGU program. Upon announcement of the new set of municipalities to be audited, students anticipate that their local government will be subject to central monitoring. This could imply reduced corruption opportunities for students drawn to rent-seeking, or increased salience of social norms around corruption and the reputational costs of public-sector careers.

Moreover, if major-switching behaviour reflects students' behavioural response, one would expect that the effects are concentrated in municipalities where high-profile corruption cases are revealed.[72] In Panels C and D in Figure 5, I further explore the heterogeneous effects of major enrollment at refined timing by the level of corruption uncovered. As elaborated in Section 2, I can utilize information from the audit reports regarding detected irregularities and define a municipality as "high corruption" if the share of inspection orders with any irregularity classified as severe corruption is above the median among all first-audited municipalities during 2011-2014.[73] First, the absence of pre-trends in the event study alleviates the concern of endogeneity in detected corruption. Second, both Panels C and D show clear patterns where audits affect enrollment only if the audits are effective at detecting a high level of corruption. In contrast, the lack of reactions in low-corruption municipalities aligns with the interpretation that students hold largely accurate priors regarding the corruption level in their locality. Corresponding table estimates reported in Appendix Tables A16 also confirm that the differences between the

[72] Many existing studies on the CGU audits have highlighted the importance of capturing effect heterogeneity by the level of corruption (Ferraz and Finan, 2008; Colonnelli and Prem, 2022; Santos and Leon, 2024). In relation to this, Colonnelli and Prem (2022) discusses the additional caveat with this heterogeneity analysis where, unlike the audit itself, the amount of corruption detected may not be random. They show that replacing actual corruption with predicted corruption based on machine learning methods (Colonnelli et al., 2020a) reveals a similar degree of heterogeneity.

[73] The median value is 0.262. Unlike Colonnelli and Prem (2022) who use the total number of irregularities scaled by the number of firms at the time of audit, I adopt this slightly different measure so as to capture the "salience" of the most effective audits. I also exclude moderate irregularities, as most inspection orders in my sample include at least some moderate irregularities, rendering little heterogeneity to exploit. The ambiguity between moderate and severe corruption, as pointed out by Avis et al. (2018), is likely less of a concern for the later waves of audits (2011-2014) I focus on.

coefficients are statistically significant.

Sorting into rent-seeking vs. prosocial careers: Although I do not observe individual-level measures of prosociality, I can adopt a revealed preference approach by examining the specific types of occupations students take up in the labor market. To this end, I classify all occupations in Brazil ex-ante based on their job descriptions, and examine whether students disproportionately sort out of public sector occupations that are prone to rent-seeking, and/or sort into private sector occupations that are considered more prosocial.[74] Columns 1-2 of Panel A in Table 6 show that high-ability students are leaving both public sector careers with relatively high and low rent-extraction opportunities, with little power to detect effect heterogeneity (p-value = 0.247). More importantly, columns 3-4 suggest that high-ability students are also sorting into both relatively high- and low-prosocial careers, with a slightly larger coefficient for the former, but the difference is statistically insignificant (p-value = 0.926). As a contrast, Panel B shows that the sorting patterns are muted for lower-ability students. Overall, I do not find evidence of high-ability students disproportionately sorting into prosocial occupations after being driven out of the public sector.

Selection by ability or ability to select? Why would anti-corruption audits disproportionately affect high-ability students? Two possible explanations can account for this empirical finding. First, high-ability students may possess a "comparative advantage" in public sector careers in Brazil. Appendix Table A2 shows a high baseline presence of high-ability students in most public sector careers in Brazil.[75] They therefore face a higher risk of reduced rents and reputation loss associated with audits. Second, in a system of selection by meritocracy (both in terms of entry into prestigious public universities and civil service careers), high-ability students possess a "first-mover advantage". Even if audits alter the preferences of all students equally, low-ability students are mechanically crowded out by the action of their high-ability peers. The muted effects for low-ability students in Table 6, especially in private careers where the capacity constraints are less binding, suggest the latter explanation is second-order. Nonetheless, both channels could play a role.[76]

Further discussion: The alternative hypothesis of *motivation crowding-out* is therefore

[74] Details on the categorization of Brazilian occupations are elaborated in Online Appendix E.

[75] This is consistent with existing literature documenting the public sector wage premium in Brazil (Cavalcanti and Santos, 2021), as well as the implicit assumption that outside option is increasing with ability.

[76] A remaining concern is differential attrition by ability, given that labor market outcomes are observed conditioning on finding a job. First, Table 5 does not show evidence of overall higher attrition for high-ability students. Second, if anything, attrition can be mainly attributed to unemployment, informality, or public exam preparation (Section 3). High-ability students tend to have better outside options, rendering the first two reasons unlikely. Alternatively, high-ability students shying away from local governments may switch to federal careers, the exams for which take longer to prepare. Despite observing a smaller sample of federal workers, evidence in Appendix Tables A9 and A14 shows this is also unsupported.

less likely to hold for the reasons above. First, there is no negative surprise shock at the time of audit announcements (before the revelation of actual corruption). Second, downstream career outcomes do not exhibit strong patterns of talent sorting into prosocial jobs. I cannot, however, rule out the possibility that intrinsically motivated individuals are attracted to the public sector following the audits as corruption is effectively reduced, that is, a *motivation crowding-in* channel.[77] In that case, the effects I observe on talent reallocation reflect net effects of rent-seeking or reputation-prone talent being discouraged from entering the public sector, and prosocial students being drawn in, where the former appears to dominate.

Lastly, I also find evidence suggesting the role of issue salience in amplifying students' behavioral responses to audits. In particular, the decomposition of group-specific treatment effects shows that effects are 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 audits drawn in 2011 and 2014, when political corruption was in the spotlight due to large-scale, high-profile scandals.[78] Furthermore, heterogeneous effects are observed with respect to local internet access (Appendix Figure A8 and Appendix Table A17), highlighting the role of local media in disseminating information regarding audit announcements and reports.[79]

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 right before 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 attend university or initiate their

[77] Alternatively, intrinsically motivated students might join a corrupt public sector, aspiring to make a change for the better. The finding of positive selection on integrity (*honesty gain* at no expense of *brain drain*) into state organization in Hong (2023), albeit in a different context, also echoes this explanation.

[78] 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 federal government.

[79] I also find interesting disparities between traditional and modern forms of media. Unlike previous literature that emphasizes the role of local radio (Ferraz and Finan, 2008; Avis et al., 2018) in spreading information on audits and corruption, the heterogeneous effects by the presence of a local AM radio station on major enrollment are less striking (Panels C and D in Appendix Figure A8). One 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. According to the *Perfil dos Municípios Brasileiros* (Panel A of Table 1), 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.

careers. However, general equilibrium responses may still confound the results, as both the higher education and formal labor markets in Brazil are relatively localized. In the baseline 2010 cohort, nearly 40% (70%) of all students attended college in the same municipality (state) as their place of residence while about 53% (91%) found their first jobs in the same municipality (state).[80] In this section, I directly test whether audits affect *education supply* in the higher education market as well as *labor demand* in the formal labor market. I show that these mechanisms are inconsistent with patterns observed in my main analysis.

Degree vacancies in higher education: One possible explanation for the changes in major enrollment is that audits affect the supply of university degrees. This may occur if audits, especially those targeting the education sector, alter fiscal transfers to education or lead to university-level reforms that disrupt staffing and planning (Gonzales, 2021). The concern, however, applies mainly to public institutions while private institutions in Brazil operate distinctively and primarily adapt to market demand. I directly test whether audits affect the number of degree vacancies offered, estimating equation 1 using the sample of universities. Appendix Table A18 summarizes the results. As universities tend to be located in larger and more urbanized municipalities, the sample size is considerably smaller. I also restrict to a balanced panel to reduce noise from sparsely populated periods. Nonetheless, the results indicate that following audits, private universities reduce vacancies in business/law and increase them in engineering, which may in turn explain the flock of low-ability students into engineering observed in Appendix Table A6. In the meantime, one does not see such reactions from public institutions, where coefficients are imprecisely estimated. These findings are consistent with the interpretation that changes in degree supply in private universities reflect changes in student demand. Moreover, if public universities had reduced business/law vacancies, 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: within business/law, high-ability students are being replaced by their lower-ability peers.

Outside option in the private sector: The parallel concern regarding the labor market is that audits may increase labor demand in the private sector, making private sector careers more attractive. The concern is particularly relevant, given prior work has shown that audits indeed boost firm activities in government-dependent sectors (Colonnelli and Prem, 2022; Colonnelli et al., 2022). I explore this channel using firm-level data from RAIS and focusing on the total number of new full-time contracts, an outcome closely tagged

[80]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 (see more details on audits and out-migration in Appendix Online D).

to demand for young talent. Appendix Table A19 summarizes the results. Panel A includes audits between 2011-2014, while Panel B extends the sample back to 2002 to maximize power, effectively incorporating all audit waves between 2003-2014. Overall, I find little evidence of audits increasing aggregate first hires in private sector firms.[81] This is in contrast with the rise in private employment observed in the student sample (Table 4), suggesting that labor demand responses from firms are not the primary driver of talent reallocation. Instead, the increase in private sector employment is plausibly a downstream outcome of students switching into private-sector-oriented majors, at which stage labor demand factors are less of a consideration due to the short timespan.

Patronage hiring in the public sector: A related concern arises in the context of public employment, particularly in a setting where patronage hiring[82] is prevalent (Colonnelli et al., 2020b) and bureaucratic turnover is closely tagged to political turnover (Akhtari et al., 2022). The results in column 1 of Appendix Table A19 demonstrate an increase in new tenure-track hires following the audits, particularly when earlier audit waves are included. This pattern contrasts with what is observed in the student sample, where I find a reduction in public sector career realizations. However, a contending hypothesis is that patronage hiring can also cause the outflow of high-ability students from the public sector (Colonnelli et al., 2020b). Even though Santos and Leon (2024) suggests that the same audits led to, if anything, a reduction in patronage hiring practices, two additional pieces of evidence are against this hypothesis. First, I observe negative sorting by ability into business/law majors at the college enrollment stage, before the hiring process becomes relevant. Second, temporary public workers in Brazil are potentially more susceptible to patronage hiring. However, Appendix Table A13 reveals that the exit of high-ability students is observed for both tenure-track and temporary workers.

5.3 Efficiency implications

If the diminished rent-seeking channel is the main driver behind the effects of audits on talent reallocation, rent-seeking motives then play a central 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 un-

[81] This result appears at odds with findings in Colonnelli and Prem (2022), which shows the same CGU audits foster firm creation and lead to more private sector employment and hires 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 heterogeneity: incumbent firms in government-dependent sectors grow the most while politically connected firms suffer. Therefore, it is unclear to what extent the boost in private firm activities translates to labor demand for fresh college graduates.

[82] 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).

productive 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 reflect self-interested motives, students who are primarily concerned with reputation may behave as opportunistic agents rather than individuals with strong public service motivation.[83] Overall, the findings in this section suggest that when corruption is pervasive, talent can be misallocated across the public and private sectors. This misallocation of talent can manifest itself as early as the stage of choosing fields of specialization in higher education, which have been documented to correlate with long-term economic growth (Murphy et al., 1993; Maloney and Valencia Caicedo, 2022).

6 Conclusion

Widespread corruption may not only reduce the efficiency of government but also distort the allocation of a society's most talented individuals. This paper provides one of the first pieces of empirical evidence that combating corruption can drive talented individuals away from careers in the public sector. I establish causality by leveraging the randomized rollout of the CGU audit program in Brazil and show 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 this shift is driven by a combination of perceived reductions in rent-seeking opportunities and heightened reputation costs associated with public employment. Together, the findings highlight an understudied channel via which corruption harms the economy: the distortion of a society's talent allocation toward rent-seeking activities in the public sector. Anti-corruption initiatives, such as government audits, have the potential to help adjust 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 key takeaways of this paper is the role of self-selection in shaping the quality of public personnel, an aspect that tends to be overlooked especially in settings where public sector jobs are heavily oversubscribed. The findings suggest that even in contexts where the selection of public personnel is merit-based and highly competitive, sorting within the candidate pool can eventually translate to the quality of the final hires. The paper therefore complements a growing literature on bureaucratic selection (Finan et al.,

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

2017; Lim and Snyder Jr, 2021; Besley et al., 2022; Mocanu, 2024) that has put more emphasis on the screening side of recruitment. While the context of Brazil provides a unique set of policy experiments and comprehensive administrative data, it is important to acknowledge that the top-down approach of combating 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 assess the external validity of the finding on ability selection and clarify which institutional features shape patterns of selection.

There are several promising avenues for future research. A key limitation of administrative data is the lack of information on student traits beyond academic ability, such as honesty or prosociality. Prior work has highlighted the ability and prosociality trade-off in attracting talent to the public sector (Dal Bó et al., 2013; Deserranno, 2019; Ashraf et al., 2020). Understanding selection by prosociality can help further pin down the mechanisms, particularly the extent to which there could be crowding-in of prosocial talent replacing rent-seekers in the public sector. With a growing body of experimental studies on corruption perceptions (Colonnelli et al., 2024; Rivera et al., 2024; Ajzenman et al., 2025), one promising research agenda is to incorporate survey tools and information experiments to elicit key traits unavailable in administrative data, such as prosociality and risk preferences, and understand how corruption (or the perception thereof) affects sorting along these dimensions. Another intriguing direction is to explore intergenerational dynamics: whether corruption rents disproportionately attract students from bureaucratic families and how anti-corruption efforts affect the intergenerational transmission of public sector employment (Ang et al., 2025). Finally, it is crucial to examine the potential productivity consequences of altered talent allocation resulting from reduced corruption, and to gauge the overall impacts of the anti-corruption audits in addition to its direct impacts on economic activities (Colonnelli and Prem, 2022). One potential intermediate step is to zoom in on specific occupation choices of high-ability students in the private sector, for example, whether they create firms and become entrepreneurs. While this paper focuses on primarily the selection margin, the use of economy-wide data offers a valuable first step toward understanding the broad implications of a society's talent allocation for productivity growth and state effectiveness.

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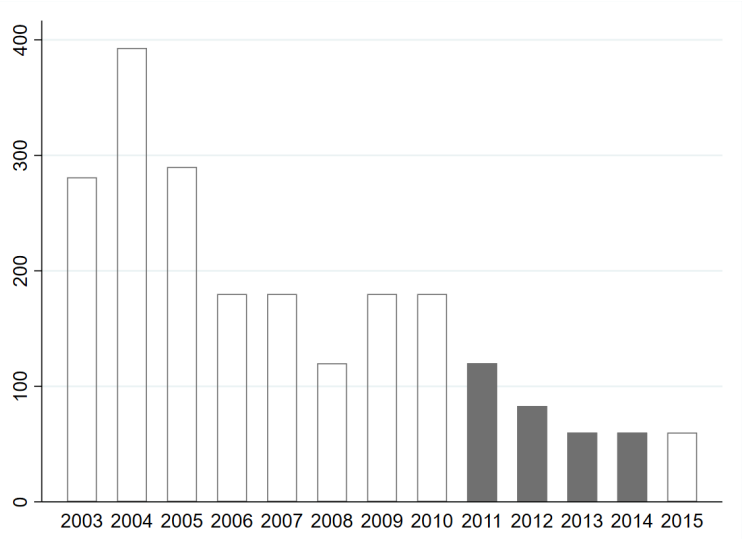
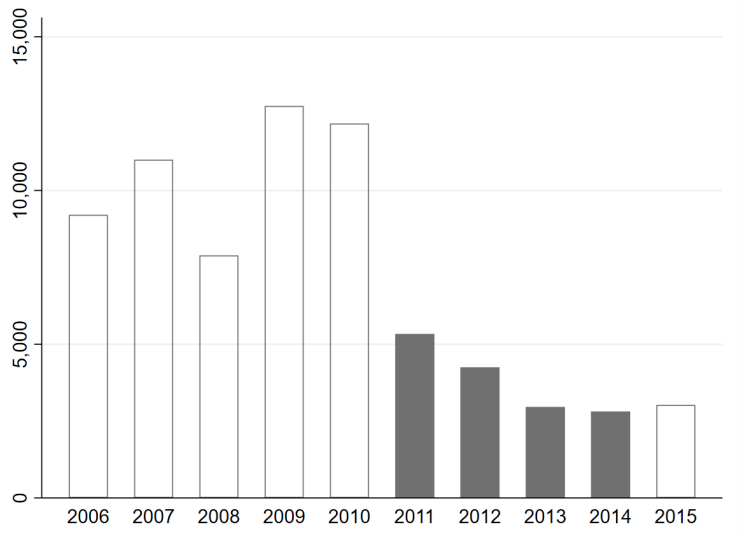
Tables and Figures



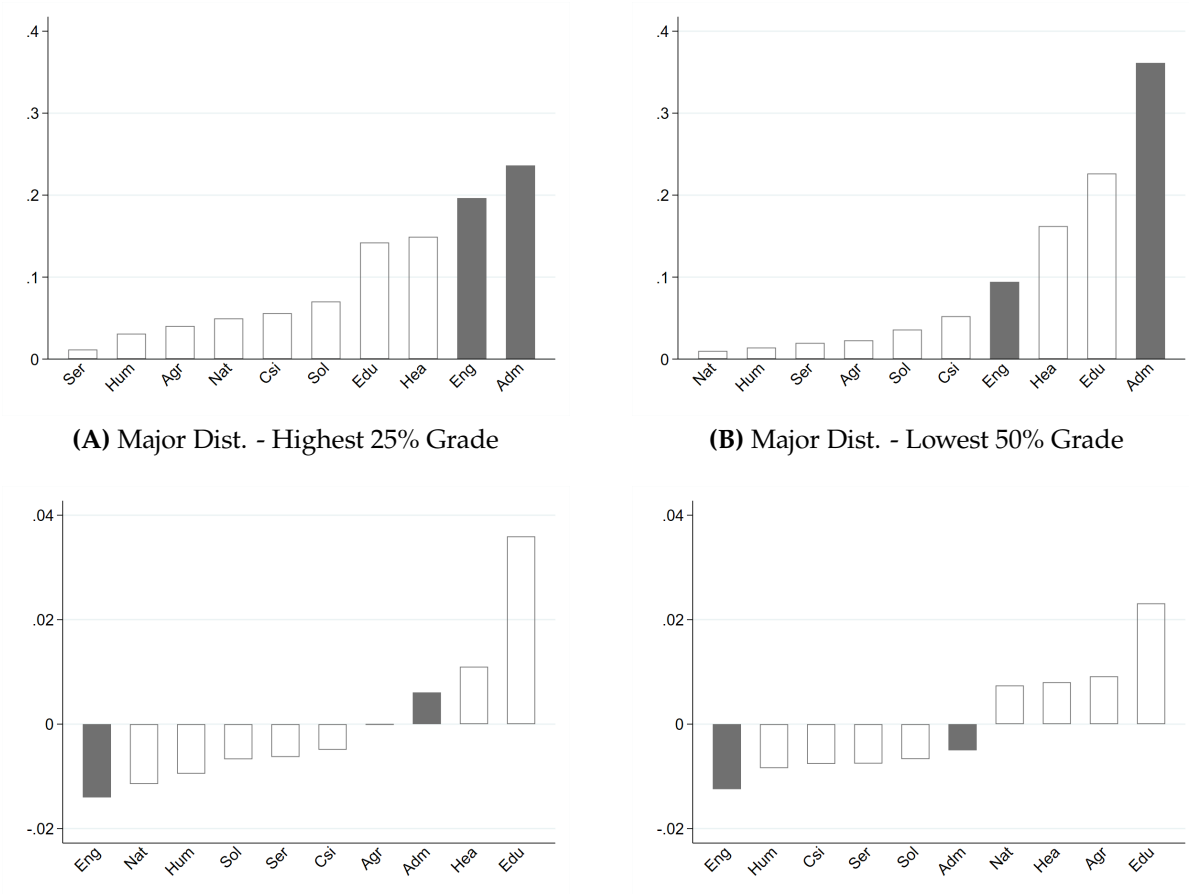
(A) Audited Municipalities



(B) Corruption Irregularities

Figure 1: Anti-corruption audits over time

Notes: This figure plots the yearly variation of the CGU audit program. Panel A shows the number of audited municipalities throughout 2003-2015. Panel B shows the reported number of corruption-related irrgularities during 2006-2015. The shaded bars (2011-2014) highlight the period this paper focuses on.



(A) Major Dist. - Highest 25% Grade

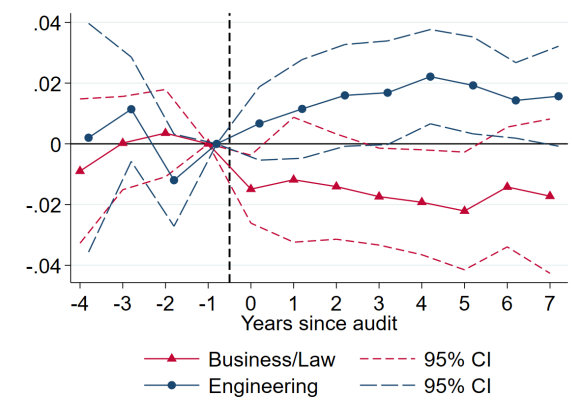
(B) Major Dist. - Lowest 50% Grade

(C) Civil Servant Share - Highest 25% Grade

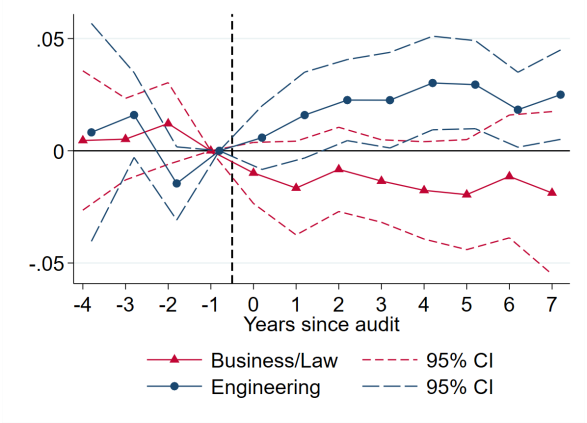
(D) Civil Servant Share - Lowest 50% Grade

Figure 2: Patterns in major enrollment and early careers

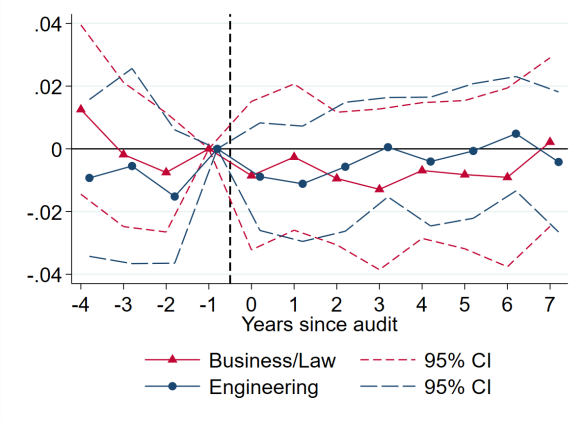
Notes: This figure illustrates descriptive patterns of major enrollment and subsequent career realizations in civil service for the baseline group of freshmen enrolled in 2010. Panels A and B display the shares of major enrollment separately for high-grade students (top 25% of the ENEM distribution) and low-grade students (bottom 50%). Panels C and D report the (demeaned) shares of students obtaining their first jobs as civil servants across majors, restricting the sample to students who enrolled in higher education in 2010 and who were successfully traced to RAIS, as described in Section 3.2.



(A) All Universities



(B) Private Universities



(C) Public Universities

Figure 3: Audits and shares of major enrollment

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.

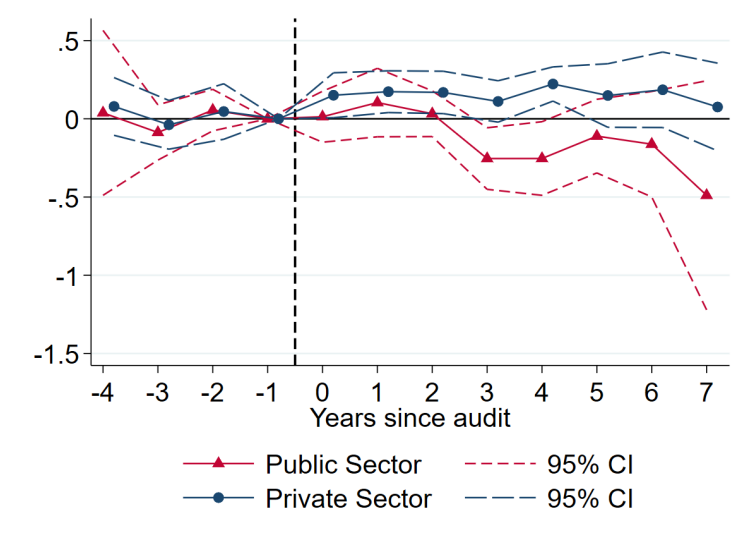
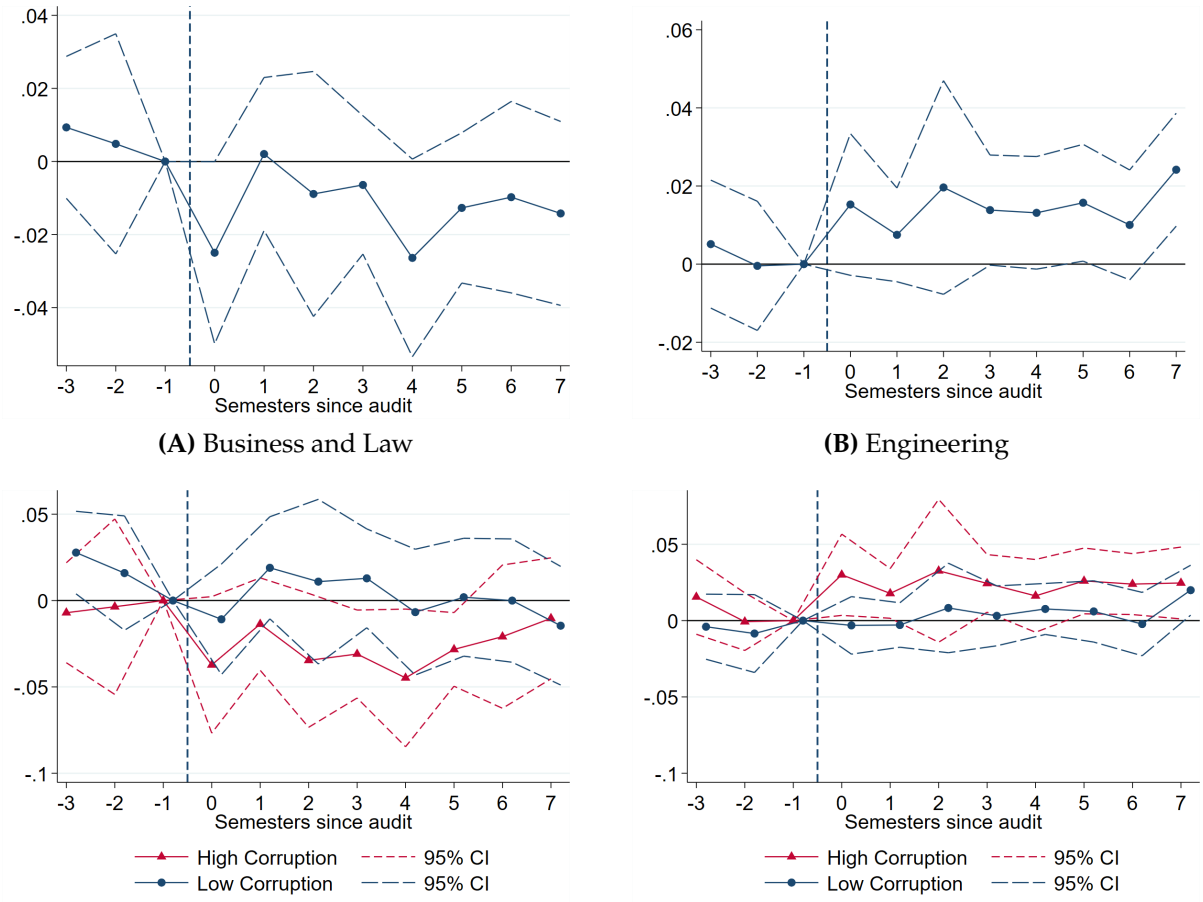


Figure 4: Audits and realized careers

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.



(A) Business and Law

(B) Engineering

(C) Business/Law - Detected Corruption

(D) Engineering - Detected Corruption

Figure 5: Audits and shares of major enrollment - time is semester

Notes: Panels A and B 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. Panels C and D report separately for municipalities uncovered with high versus low corruption. 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 (1)	Std. Dev. (2)	Mean (3)	Std. Dev. (4)	(5)
Panel A: Pre-Trement Municipal Characterisitcs					
Population (in thousands)	28.55	32.59	31.61	39.09	2.04
					(2.51)
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	71.31	208.15	78.28	198.17	8.61
					(13.10)
Share female	0.60	0.18	0.60	0.18	-0.00
					(0.01)
Share in public universities	0.34	0.27	0.35	0.26	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	572.93	956.39	657.36	959.74	15.73
					(60.47)
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

	Business/Law		Engineering	
	Share (1)	Num. (2)	Share (3)	Num. (4)
Panel A: All Universities				
Audit × Post	-0.017***	-0.040	0.016**	0.091
	(0.006)	(0.029)	(0.007)	(0.056)
R^2	0.58	0.98	0.73	0.96
Romano-Wolf p-value	0.008	0.375	0.024	0.327
Mean Dep. Var.	0.30	5.43	0.16	4.68
SD Dep. Var.	0.08	1.68	0.08	1.82
Observations	169,835	169,835	169,835	169,835
Num. of Clusters	3,693	3,693	3,693	3,693
Panel B: Private Universities				
Audit × Post	-0.018***	-0.039	0.021**	0.140**
	(0.007)	(0.031)	(0.008)	(0.056)
R^2	0.51	0.97	0.69	0.96
Romano-Wolf p-value	0.004	0.108	0.008	0.016
Mean Dep. Var.	0.36	5.36	0.15	4.38
SD Dep. Var.	0.09	1.68	0.08	1.83
Observations	168,476	168,476	168,746	168,476
Num. of Clusters	3,693	3,693	3,693	3,693
Panel C: Public Universities				
Audit × Post	-0.006	-0.045	0.001	-0.008
	(0.009)	(0.088)	(0.006)	(0.078)
R^2	0.52	0.91	0.64	0.94
Romano-Wolf p-value	0.749	0.912	0.968	0.996
Mean Dep. Var.	0.13	3.16	0.17	3.42
SD Dep. Var.	0.11	1.88	0.13	1.94
Observations	157,506	157,506	157,506	157,506
Num. of Clusters	3,691	3,691	3,691	3,691
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. The dependent variables are the shares of freshmen enrolled in business/law (column 1) and engineering (column 3), as well as the corresponding numbers in inverse hyperbolic sine transformation (columns 2 and 4). Adjusted p-values are reported based on Romano-Wolf multiple hypothesis correction across all ten majors, implemented using rwolf2 command in Stata where the number of boostrap replication is set at 250. The unit of observation is municipality-year-cohort. Regressions are weighted by the total number of students in the baseline year 2010. 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 reports estimates for all students, pooling public and private universities. Panels B and C report estimates separately for students attending private and public universities, respectively. 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.	Num. by Quartile of ENEM Grades		
		Lowest 50%	Second Highest 25%	Highest 25%
	(1)	(2)	(3)	(4)
Panel A: Business/Law				
Audit × Post	-0.044	0.141	-0.044	-0.148**
	(0.075)	(0.149)	(0.120)	(0.070)
R^2	0.91	0.79	0.85	0.92
Mean Dep. Var.	3.76	2.17	2.43	2.95
SD Dep. Var.	1.60	1.52	1.55	1.69
Observations	56,317	56,317	56,317	56,317
Num. of Clusters	1,526	1,526	1,526	1,526
Panel B: Engineering				
Audit × Post	0.077	0.054	0.300	0.044
	(0.073)	(0.139)	(0.189)	(0.068)
R^2	0.95	0.84	0.88	0.95
Mean Dep. Var.	4.21	2.10	2.54	3.72
SD Dep. Var.	1.63	1.57	1.62	1.70
Observations	64,070	64,070	64,070	64,070
Num. of Clusters	1,661	1,661	1,661	1,661
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 number (IHS-transformed) of students with ENEM grades at different quartiles of the score distribution (controlling for exam year). 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. Regressions are weighted by the total number of students in the baseline year 2010. 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

	Public Sector		Private Sector	
	Share (1)	Num. (2)	Share (3)	Num. (4)
Audit × Post	0.001	-0.095	-0.001	0.149**
	(0.017)	(0.115)	(0.017)	(0.060)
R^2	0.67	0.84	0.67	0.96
Romano-Wolf p-value	0.941	0.248	0.941	0.010
Mean Dep. Var.	0.18	2.33	0.82	4.15
SD Dep. Var.	0.21	1.30	0.21	1.66
Observations	96,153	96,153	96,153	96,153
Num. of Clusters	3,036	3,036	3,036	3,036
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 in the public sector (column 1) versus the private sector (column 3) as well as the corresponding total number of students (reported in inverse hyperbolic sine transformations in columns 2 and 4). Adjusted *p*-values are reported based on Romano-Wolf multiple hypothesis correction across two careers, implemented using rwolf2 command in Stata where the number of boostrap replication is set at 250. The unit of observation is municipality-year-cohort. Regressions are weighted by the total number of students in the baseline year 2010. 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 5: Effect of Audits on Workforce Composition by Ability

	Total Num.	Num. by Quartile of ENEM Grades		
		Lowest 50%	Second Highest 25%	Highest 25%
	(1)	(2)	(3)	(4)
Panel A: Public Sector				
Audit × Post	-0.096	-0.046	-0.013	-0.295***
	(0.117)	(0.186)	(0.214)	(0.083)
R^2	0.84	0.68	0.66	0.77
Mean Dep. Var.	2.60	1.47	1.39	1.66
SD Dep. Var.	1.08	1.01	1.02	1.19
Observations	62,363	62,363	62,363	62,363
Num. of Clusters	2,460	2,460	2,460	2,460
Panel B: Private Sector				
Audit × Post	0.157***	0.064	0.173**	0.173**
	(0.058)	(0.054)	(0.083)	(0.078)
R^2	0.96	0.92	0.92	0.94
Mean Dep. Var.	4.25	3.29	2.92	3.10
SD Dep. Var.	1.55	1.47	1.52	1.67
Observations	77,430	77,430	77,430	77,430
Num. of Clusters	2,615	2,615	2,615	2,615
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 number (IHS-transformed) of students with ENEM grades at different quartiles of the score distribution (controlling for exam year). Panel A reports the sample of students who end up in the public sector and Panel B includes the sample of students who end up in the private sector. The unit of observation is municipality-year-cohort. Regressions are weighted by the total number of students in the baseline year 2010. 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 6: Career Realizations by Job Feature and Student Ability

	Public Sector		Private Sector	
	High Rent-Seeking	Low Rent-Seeking	High Pro-Social	Low Pro-Social
	(1)	(2)	(3)	(4)
Panel A: High-Ability Students (ENEM Highest 25%)				
Audit × Post	-0.281**	-0.319***	0.203**	0.169*
	(0.127)	(0.112)	(0.092)	(0.096)
p-value (H_0: $\beta_1 = \beta_2$)	0.247	0.247	0.926	0.926
R^2	0.69	0.69	0.90	0.91
Mean Dep. Var.	1.29	1.25	2.44	2.66
SD Dep. Var.	1.05	1.04	1.46	1.58
Observations	37,308	44,726	57,059	60,732
Num. of Clusters	1,789	1,973	2,161	2,255
Panel B: Low-Ability Students (ENEM Lowest 50%)				
Audit × Post	-0.110	0.090	0.038	0.066
	(0.231)	(0.144)	(0.063)	(0.064)
p-value (H_0: $\beta_1 = \beta_2$)	0.467	0.467	0.106	0.106
R^2	0.59	0.61	0.90	0.89
Mean Dep. Var.	1.09	1.02	2.77	2.66
SD Dep. Var.	0.88	0.91	1.35	1.36
Observations	37,308	44,726	57,059	60,732
Num. of Clusters	1,789	1,973	2,161	2,255
Muni. × Cohort FE	X	X	X	X
State × Year × Cohort FE	X	X	X	X

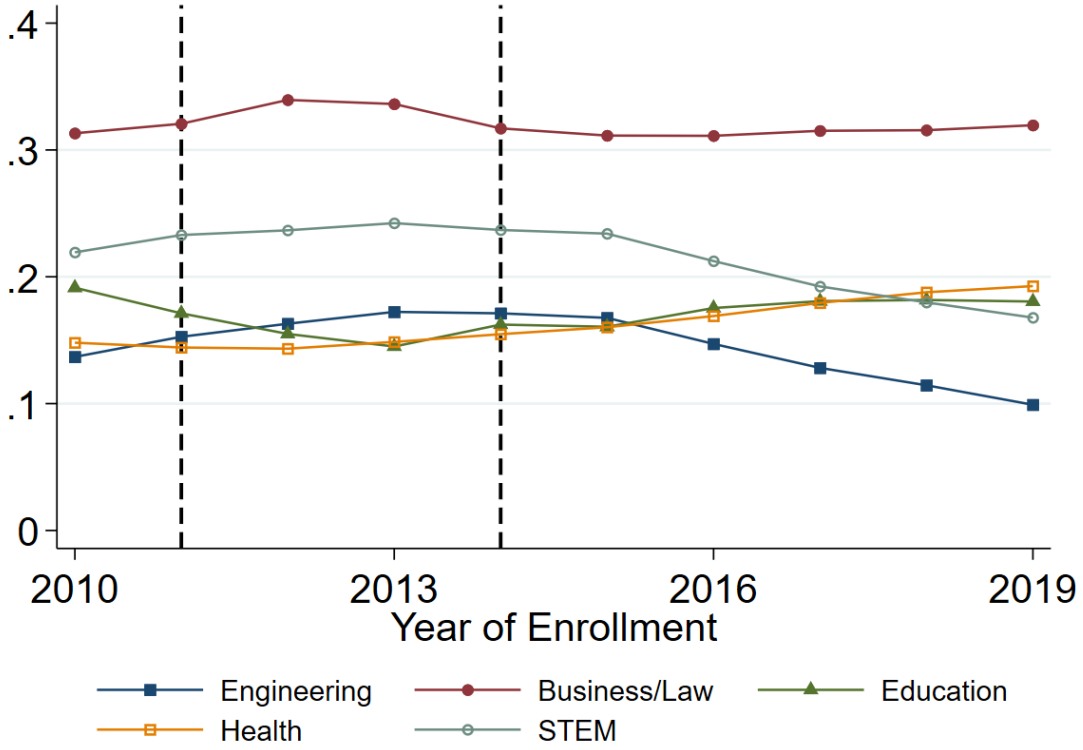
Notes: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the number of high-ability students (IHS-transformed) in corresponding careers in Panel A, and the corresponding number for low-ability students in Panel B. *P*-values are reported for the null hypothesis where coefficients in columns 1-2 or column 3-4 are equal. The unit of observation is municipality-year-cohort. Regressions are weighted by the total number of students in the baseline year 2010. 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Corruption and Talent Allocation

Online Appendix

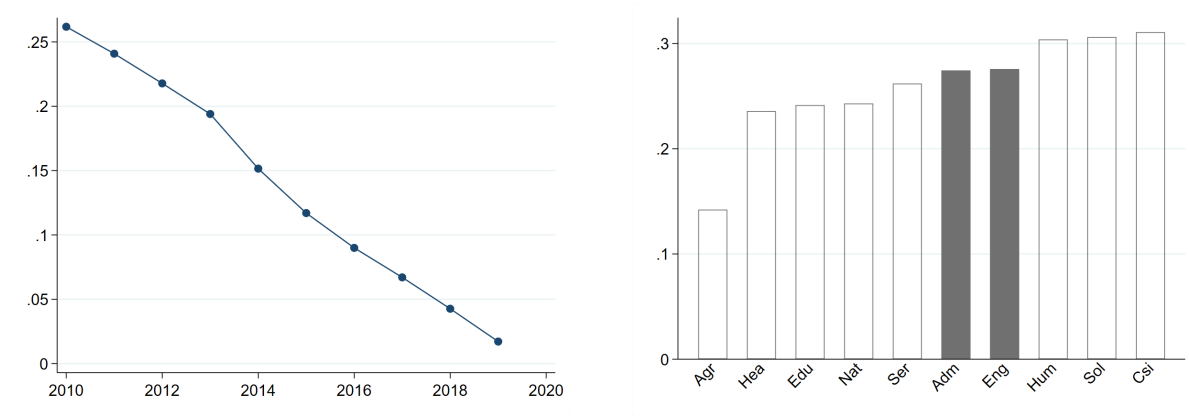
A Additional Figures and Tables

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 IT, as well as engineering.

Figure A2: Share of students traced to RAIS

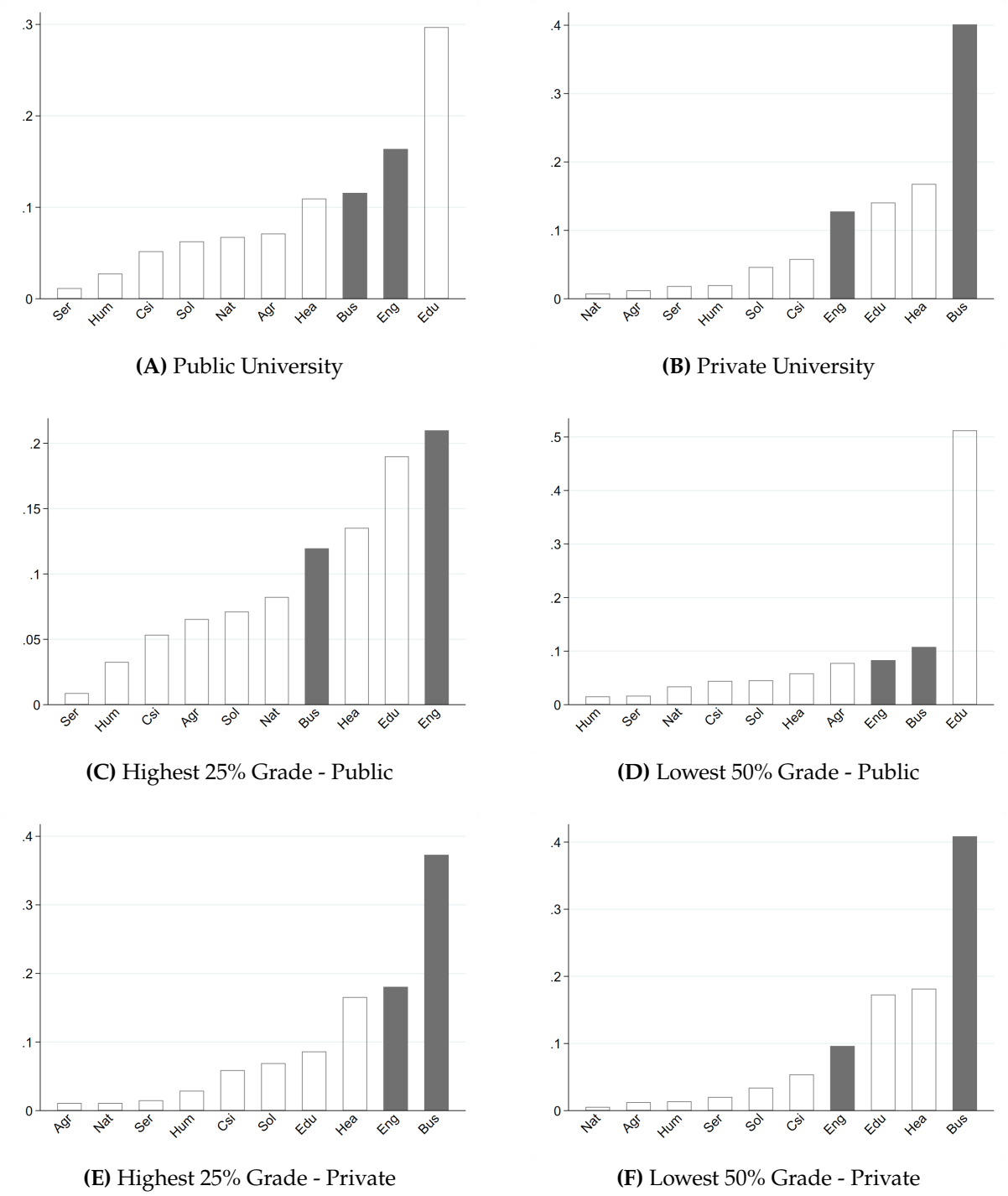


(A) By Year of Enrollment

(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 (2010-2019). 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 patterns of major enrollment



(A) Public University

(B) Private University

(C) Highest 25% Grade - Public

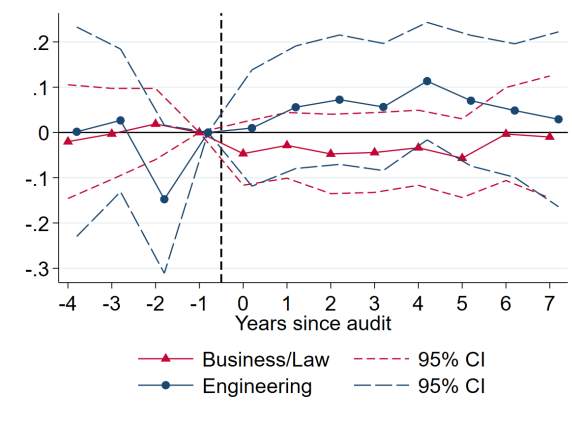
(D) Lowest 50% Grade - Public

(E) Highest 25% Grade - Private

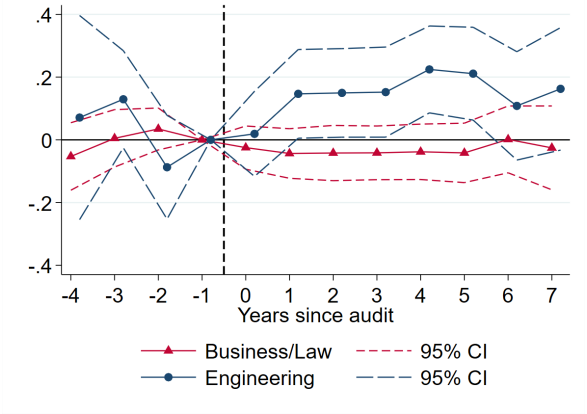
(F) Lowest 50% Grade - Private

Notes: This figure illustrates the shares of major enrollment among all freshmen observed in the Census of Higher Education in 2010. Panel A reports for all public university students and Panel B reports for all private university students. Panel C reports for high-ability students (at the top 25% 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. Panels E and F repeat for private university.

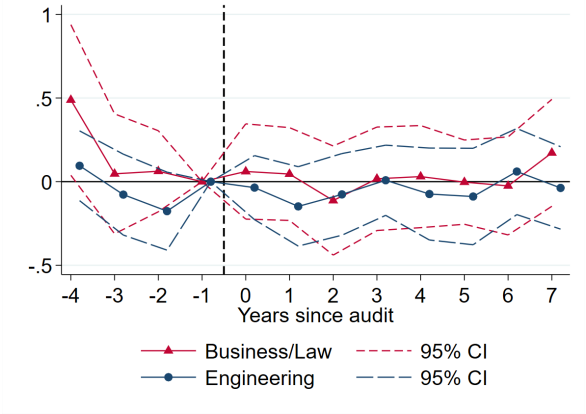
Figure A4: Audits and numbers of major enrollment



(A) All Universities



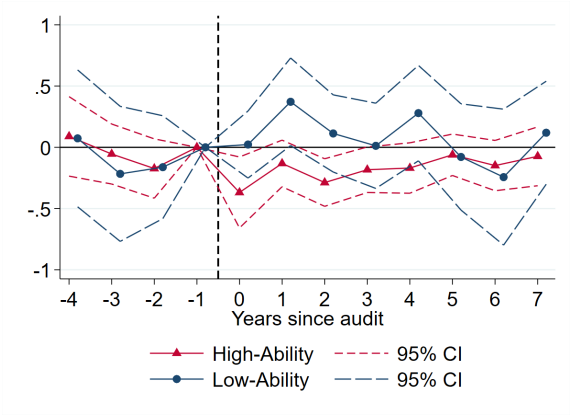
(B) Private Universities



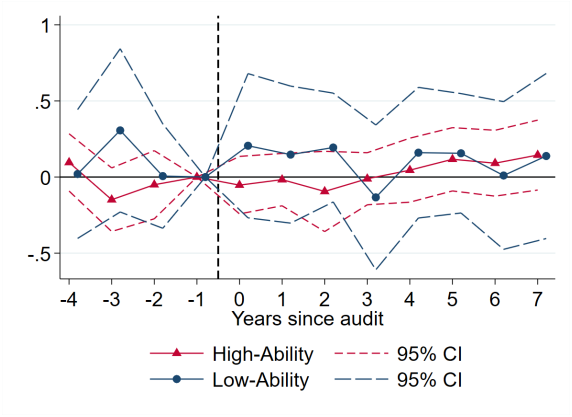
(C) Public Universities

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.

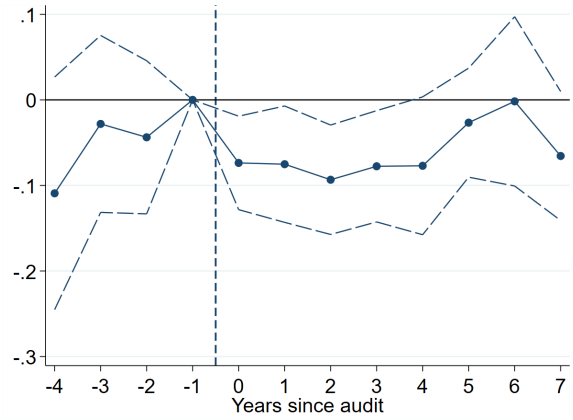
Figure A5: Audits and ability-sorting in public universities



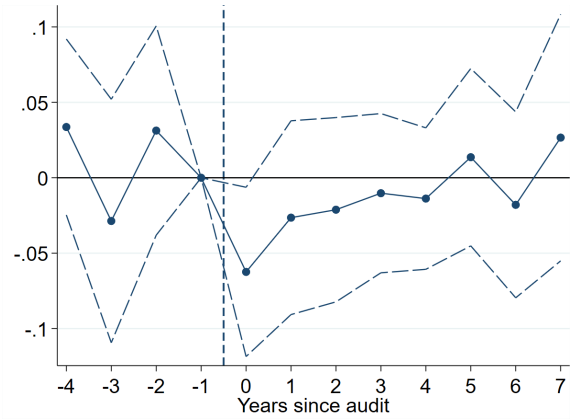
(A) Business/Law - Number



(B) Engineering - Number



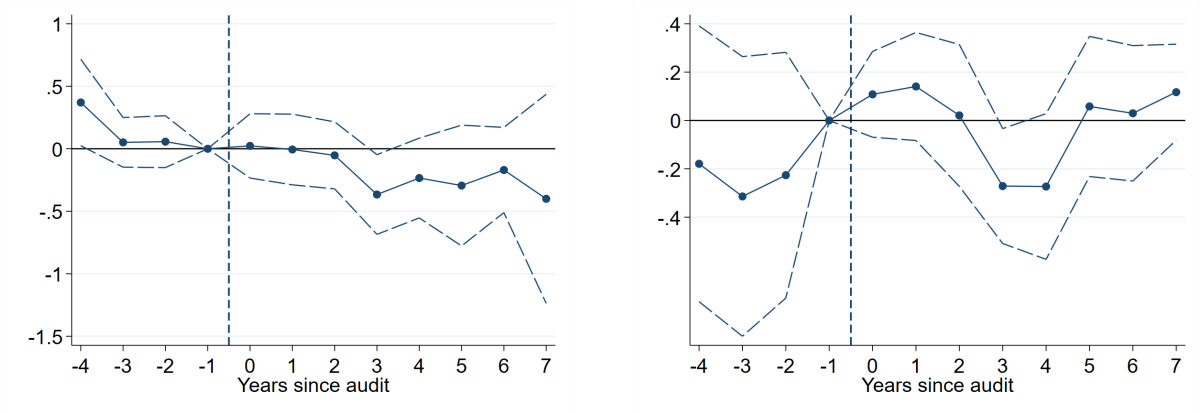
(C) Business/Law - High-Ability Share



(D) Engineering - High-Ability Share

Notes: This figure reports coefficients obtained from the estimation of equation 2. Dependent variable is the number (IHS-transformed) of students from the top quartile of the ENEM grade distribution in Panels A and B, and the corresponding shares in Panels C and D. Panels A and B correspond to table estimates reported in Table 3. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

Figure A6: Audits and realized careers by public sector contract type

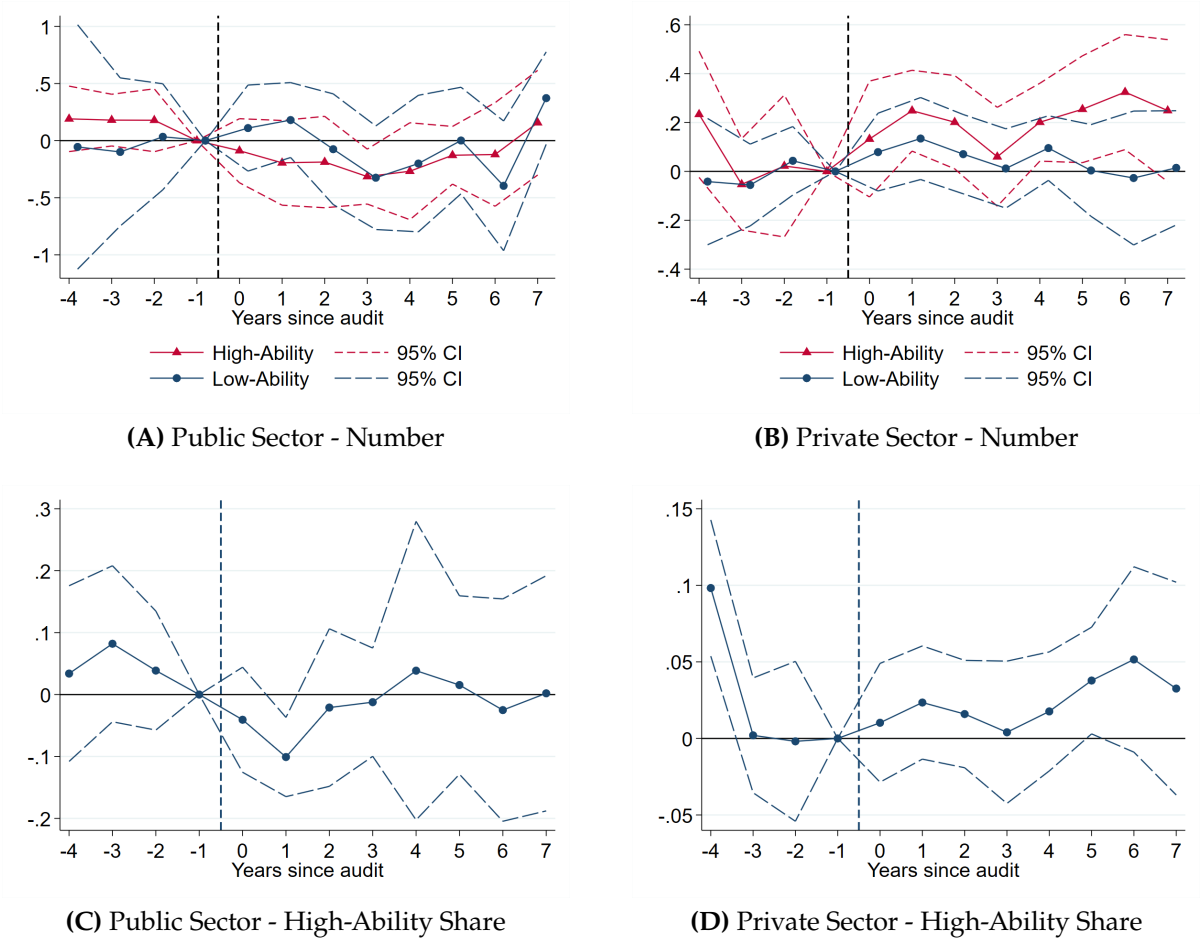


(A) Number (asinh) in Tenure-Track Civil Service

(B) Number (asinh) in Temporary

Notes: This figure reports coefficients obtained from the estimation of equation 2, corresponding to Appendix Table A8. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

Figure A7: Audits and ability-sorting in the workforce



(A) Public Sector - Number

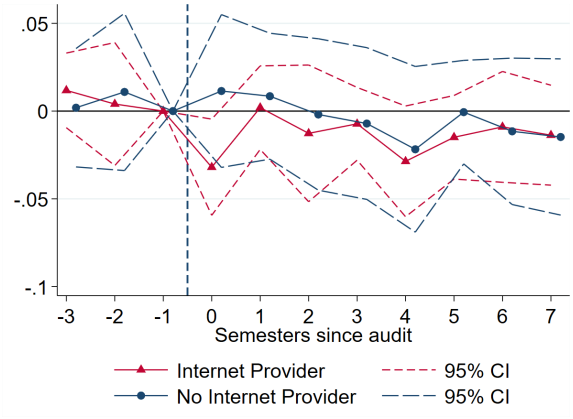
(B) Private Sector - Number

(C) Public Sector - High-Ability Share

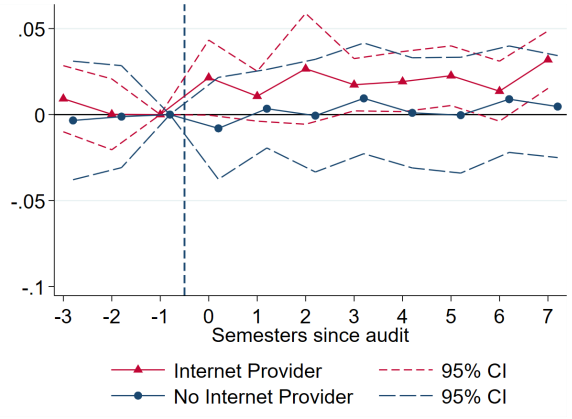
(D) Private Sector - High-Ability Share

Notes: This figure reports coefficients obtained from the estimation of equation 2. Dependent Variable is the number (IHS-transformed) of workers from the top quartile of the ENEM grade distribution in Panels A and B, and the corresponding shares in Panels C and D. Panels A and B correspond to table estimates reported in Table 5. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

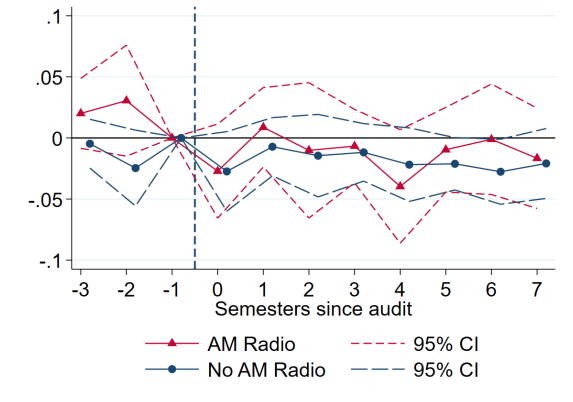
Figure A8: Effect heterogeneity by traditional media



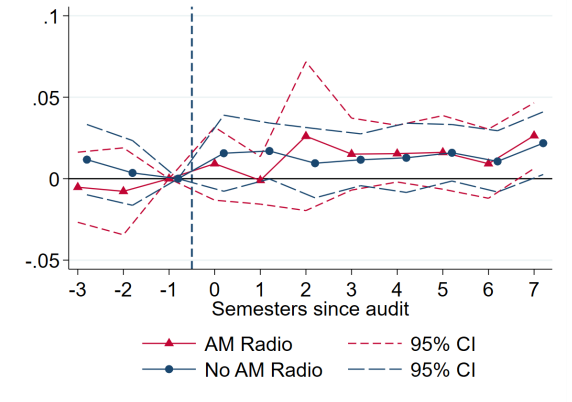
(A) Business/Law - Internet



(B) Engineering - Internet



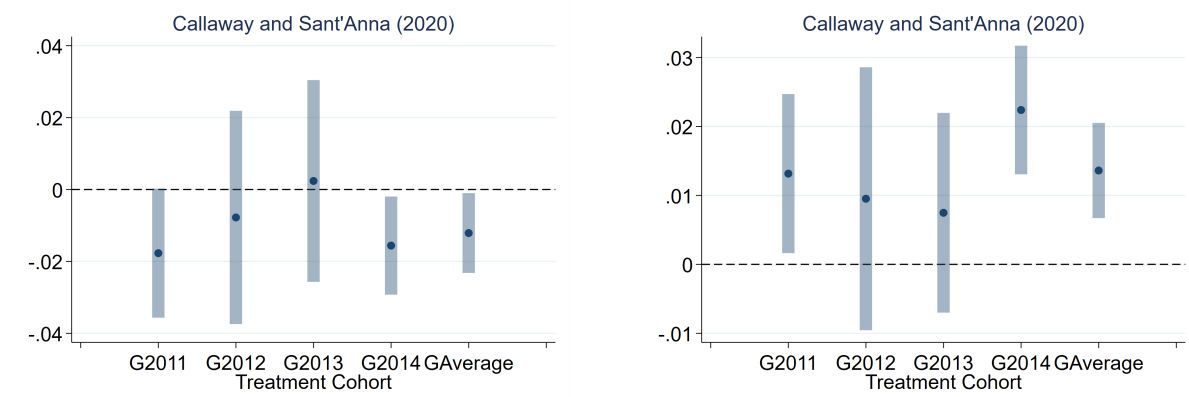
(C) Business/Law - Radio



(D) Engineering - Radio

Notes: This figure reports coefficients obtained from the estimation of equation 2 when outcome is major enrollment shares in business/law versus engineering. Panels A and B (C and D) report separately for municipalities with and without internet providers (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)	Std. Dev. (2)	Mean (3)	Std. Dev. (4)	Mean (5)	Std. Dev. (6)
Num. of workers	349.26	369.23	30.88	32.42	26.05	36.73
Lapse CES-RAIS (Years)	3.65	0.70	4.71	1.28	4.80	1.35
Share female	0.56	0.11	0.62	0.22	0.67	0.24
Age	25.55	2.88	29.38	4.77	29.47	5.80
Share with postgraduate degree	0.01	0.02	0.02	0.05	0.01	0.05
Share with college-educated parent	0.26	0.12	0.22	0.19	0.21	0.20
Share among top family income quartile	0.15	0.08	0.14	0.15	0.11	0.15
Share among top ENEM grade quartile	0.29	0.13	0.37	0.23	0.24	0.23
Avg. ENEM grade	553.03	36.12	563.47	64.63	530.67	72.18
Share enrolled in Business/Law	0.36	0.12	0.20	0.18	0.19	0.20
Share enrolled in Engineering	0.18	0.10	0.08	0.11	0.08	0.13
Share enrolled in Education	0.13	0.10	0.45	0.24	0.45	0.28
Share enrolled in Health	0.11	0.07	0.14	0.17	0.15	0.18
Observations	2,444		1,701		1,645	

Notes: This table shows the means and standard deviations of various characteristics of students who enrolled in higher education in the baseline year of 2010 and were traced in RAIS during 2010-2019. 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.

Table A2: Public Careers by Ability

	Share by Quartile of ENEM Grades		
	Lowest 50% (1)	Second Highest 25% (2)	Highest 25% (3)
Public (all)	0.30	0.26	0.44
Civil servant	0.22	0.26	0.52
Temporary worker	0.40	0.25	0.34
Executive branch	0.32	0.27	0.41
Legislative branch	0.34	0.25	0.41
Judiciary branch	0.13	0.16	0.71
Federal gov.	0.06	0.17	0.77
State gov.	0.27	0.27	0.47
Municipal gov.	0.37	0.27	0.36
Bureaucrats	0.32	0.28	0.40
Frontline providers	0.28	0.24	0.48
High rent-seeking	0.28	0.25	0.47

Notes: This table reports shares of public sector workers by ability groups from the baseline cohort of 2010. Alternative categorizations of public sector careers include: by contract type (civil servants or temporary), by branch of government (executive, legislative, or judiciary), by level of hierarchy (federal, state, or municipal), by occupation (bureaucrats or frontline providers) based on definition in Colonnelli et al. (2020b), as well as high rent-seeking propensity as classified in Appendix 6.

Table A3: Effect of Audits on Other Major Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A	**Education**		**Humanities**		**Social Sci.**		**Natural Sci.**	
	Share	Num.	Share	Num.	Share	Num.	Share	Num.
Audit × Post	0.008	0.034	-0.002*	-0.086	-0.001	-0.063	0.002	0.119
	(0.006)	(0.037)	(0.001)	(0.067)	(0.002)	(0.058)	(0.002)	(0.080)
R^2	0.72	0.95	0.63	0.94	0.52	0.95	0.64	0.92
Romano-Wolf p-value	0.402	0.649	0.175	0.442	0.829	0.550	0.525	0.327
Mean Dep. Var.	0.20	5.02	0.02	2.51	0.05	3.54	0.02	2.43
SD Dep. Var.	0.10	1.51	0.02	1.88	0.03	1.86	0.02	1.76
Observations	169,886	169,886	169,886	169,886	169,886	169,886	169,886	169,886
Num. of Clusters	3,693	3,693	3,693	3,693	3,693	3,693	3,693	3,693
Panel B	**CS and IT**		**Agriculture**		**Health**		**Services**	
	Share	Num.	Share	Num.	Share	Num.	Share	Num.
Audit × Post	-0.001	-0.007	0.000	-0.005	-0.005	-0.024	-0.000	-0.078
	(0.002)	(0.051)	(0.003)	(0.059)	(0.005)	(0.043)	(0.001)	(0.102)
R^2	0.48	0.94	0.65	0.91	0.60	0.96	0.45	0.90
Romano-Wolf p-value	0.861	0.972	0.911	0.972	0.683	0.781	0.861	0.701
Mean Dep. Var.	0.05	3.56	0.04	3.17	0.16	4.82	0.02	2.54
SD Dep. Var.	0.03	1.79	0.04	1.54	0.07	1.62	0.02	1.72
Observations	169,886	169,886	169,886	169,886	169,886	169,886	169,886	169,886
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 the corresponding field of study. Adjusted *p*-values are reported based on Romano-Wolf multiple hypothesis correction across all ten majors, implemented using rwolf2 command in Stata where the number of boostrap replication is set at 250. 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 A4: Effect of Audits on College Enrollment

	Total Num. (1)	Num. in Public Uni. (2)	Share in Public Uni. (3)
Panel A: All Freshman Students			
Audit × Post	0.016	0.014	0.006
	(0.024)	(0.042)	(0.008)
R^2	0.99	0.96	0.85
Mean Dep. Var.	6.06	4.49	0.26
SD Dep. Var.	1.58	1.72	0.17
Observations	169,886	158,228	158,228
Num. of Clusters	3,693	3,692	3,692
Panel B: High-Ability Students (ENEM Highest 25%)			
Audit × Post	0.024	0.025	0.006
	(0.024)	(0.038)	(0.008)
R^2	0.99	0.97	0.79
Mean Dep. Var.	5.00	4.10	0.43
SD Dep. Var.	1.73	1.80	0.18
Observations	160,880	135,126	135,126
Num. of Clusters	3,690	3,593	3,592
Panel C: Low-Ability Students (ENEM Lowest 50%)			
Audit × Post	0.013	0.067	0.004
	(0.027)	(0.089)	(0.008)
R^2	0.98	0.89	0.79
Mean Dep. Var.	5.16	2.83	0.16
SD Dep. Var.	1.55	1.56	0.16
Observations	168,843	130,796	130,796
Num. of Clusters	3,693	3,583	3,583
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 A5: Effect of Audits on Student Composition (Public Uni.) by Ability - Shares

	Share by Quartile of ENEM Grades		
	Lowest 50%	Second Highest 25%	Highest 25%
	(1)	(2)	(3)
Panel A: Business/Law			
Audit × Post	0.026	0.036**	-0.063**
	(0.021)	(0.014)	(0.026)
R^2	0.54	0.30	0.57
Mean Dep. Var.	0.25	0.28	0.47
SD Dep. Var.	0.24	0.20	0.29
Observations	50,448	50,448	50,448
Num. of Clusters	1,486	1,486	1,486
Panel B: Engineering			
Audit × Post	0.025**	0.011	-0.037**
	(0.013)	(0.011)	(0.015)
R^2	0.55	0.36	0.60
Mean Dep. Var.	0.16	0.21	0.64
SD Dep. Var.	0.18	0.16	0.25
Observations	58,210	58,210	58,210
Num. of Clusters	1,648	1,648	1,648
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 the shares of students with ENEM grades at different quartiles of the score distribution (controlling for exam year). 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. Regressions are weighted by the total number of students in the baseline year 2010. 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 Student Composition (Private Uni.) by Ability

	Total Num.	Num. by Quartile of ENEM Grades		
		Lowest 50%	Second Highest 25%	Highest 25%
	(1)	(2)	(3)	(4)
Panel A: Business/Law				
Audit × Post	-0.025	-0.013	-0.038	-0.051*
	(0.031)	(0.040)	(0.035)	(0.027)
R^2	0.98	0.97	0.96	0.95
Mean Dep. Var.	5.50	4.89	4.07	3.82
SD Dep. Var.	1.61	1.61	1.67	1.70
Observations	143,393	143,393	143,393	143,393
Num. of Clusters	3,167	3,167	3,167	3,167
Panel B: Engineering				
Audit × Post	0.185**	0.249***	0.187**	0.111
	(0.073)	(0.088)	(0.085)	(0.069)
R^2	0.96	0.94	0.94	0.95
Mean Dep. Var.	4.87	4.00	3.61	3.56
SD Dep. Var.	1.62	1.60	1.65	1.70
Observations	90,705	90,705	90,705	90,705
Num. of Clusters	2,090	2,090	2,090	2,090

Notes: This table reports coefficients obtained from the estimation of equation 1. Dependent variables are the number (IHS-transformed) of students with ENEM grades at different quartiles of the score distribution (controlling for exam year), 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 A7: Effect of Audits on Detailed Major Enrollment by Ability Group

	Enrollment by Ability Group and Major Fields				
	Business/Law	Engineering	Education	Health	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: High-Ability Students (ENEM Highest 25%)					
Audit × Post	-0.062**	0.062	-0.012	0.030	0.045
	(0.026)	(0.052)	(0.033)	(0.042)	(0.039)
R^2	0.96	0.96	0.94	0.95	0.97
Mean Dep. Var.	4.19	4.00	3.59	3.71	4.14
SD Dep. Var.	1.73	1.83	1.64	1.70	1.85
Observations	160,068	160,068	160,068	160,068	160,068
Num. of Clusters	3,687	3,687	3,687	3,687	3,687
Panel B: Low-Ability Students (ENEM Lowest 50%)					
Audit × Post	-0.029	0.189***	0.052	-0.064	-0.056
	(0.037)	(0.072)	(0.041)	(0.055)	(0.046)
R^2	0.96	0.93	0.92	0.94	0.94
Mean Dep. Var.	4.58	3.44	4.14	3.87	3.77
SD Dep. Var.	1.70	1.77	1.49	1.62	1.69
Observations	168,644	168,644	168,644	168,644	168,644
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 (IHS-transformed) in the corresponding fields of study. 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 A8: Effect of Audits on Early Careers in Public Sector

	Public Sector Contract Type			
	Tenure-Track		Temporary	
	Share (1)	Num. (2)	Share (3)	Num. (4)
Audit × Post	-0.016*	-0.231*	0.018	0.119
	(0.009)	(0.139)	(0.012)	(0.158)
R^2	0.51	0.82	0.65	0.79
Mean Dep. Var.	0.09	1.82	0.09	1.28
SD Dep. Var.	0.13	1.30	0.17	1.23
Observations	96,153	96,153	96,153	96,153
Num. of Clusters	3,036	3,036	3,036	3,036
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, zooming into different types of public sector careers as reported in columns 1-2 of Table 4. Dependent variables are the share of students in the civil service (column 1) versus the temporary public workers (column 3) as well as the corresponding total number of students (reported in inverse hyperbolic sine transformations in 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 audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9: Effect of Audits on Early Careers in Public Sector - Other Categories

	Realizations of First Jobs in Public Sector: Other Categories							
	By Government Branch			By Hierarchical Level			By Occupation Type	
	Executive (1)	Legislative (2)	Judiciary (3)	Federal (4)	State (5)	Municipal (6)	Bureaucrats (7)	Frontline (8)
Audit × Post	-0.051	0.030	-0.140	-0.104	-0.110	-0.050	0.253	-0.265
	(0.120)	(0.086)	(0.096)	(0.149)	(0.084)	(0.185)	-0.179	-0.433
R^2	0.83	0.46	0.57	0.67	0.78	0.79	0.26	0.19
Mean Dep. Var.	2.16	0.26	0.28	0.36	1.51	1.70	0.3	0.58
SD Dep. Var.	1.28	0.51	0.57	0.73	1.20	1.21	1.06	1.47
Observations	96,153	96,153	96,153	96,153	96,153	96,153	96,153	96,153
Num. of Clusters	3,036	3,036	3,036	3,036	3,036	3,036	3,036	3,036
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, zooming into other categories of public sector careers. Dependent variables are the number of students (IHS-transformed). The unit of observation is municipality-year-cohort. Columns 1-3 report results for careers by the branch of government. Column 4-6 report results for careers by the level of hierarchy. Columns 7-8 report results for careers by occupation type, following definitions of bureaucrats and frontline providers as in Colonnelli et al. (2020b). 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 A10: Effect of Audits on Early Careers by Type of Institution

	Public Sector		Private Sector	
	Share (1)	Num. (2)	Share (3)	Num. (4)
Panel A: Private Universities				
Audit × Post	-0.001	-0.157	0.001	0.110**
	(0.017)	(0.140)	(0.017)	(0.051)
R^2	0.62	0.81	0.62	0.93
Mean Dep. Var.	0.22	1.83	0.78	2.72
SD Dep. Var.	0.26	1.26	0.26	1.70
Observations	86,589	86,589	86,589	86,589
Num. of Clusters	2,886	2,886	2,886	2,886
Panel B: Public Universities				
Audit × Post	-0.011	0.036	0.011	0.164*
	(0.021)	(0.092)	(0.021)	(0.095)
R^2	0.57	0.80	0.57	0.88
Mean Dep. Var.	0.23	1.51	0.77	2.04
SD Dep. Var.	0.27	1.26	0.27	1.45
Observations	48,461	48,461	48,461	48,461
Num. of Clusters	2,033	2,033	2,033	2,033
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, as in Table 4, but separately for students enrolled in private (Panel A) versus public (Panel B) 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 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 A11: Effect of Audits on Workforce Composition by Ability - Shares

	Share by Quartile of ENEM Grades		
	Lowest 50%	Second Highest 25%	Highest 25%
	(1)	(2)	(3)
Panel A: Public Sector			
Audit × Post	0.012	0.041	-0.053
	(0.030)	(0.045)	(0.042)
R^2	0.37	0.21	0.38
Mean Dep. Var.	0.33	0.28	0.38
SD Dep. Var.	0.28	0.25	0.28
Observations	62,363	62,363	62,363
Num. of Clusters	2,460	2,460	2,460
Panel B: Private Sector			
Audit × Post	-0.016	0.007	0.009
	(0.013)	(0.021)	(0.018)
R^2	0.46	0.21	0.50
Mean Dep. Var.	0.41	0.26	0.33
SD Dep. Var.	0.20	0.15	0.19
Observations	77,430	77,430	77,430
Num. of Clusters	2,615	2,615	2,615
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 the shares of students with ENEM grades at different quartiles of the score distribution (controlling for exam year). Panel A reports the sample of students who end up in the public sector and Panel B includes the sample of students who end up in the private sector. The unit of observation is municipality-year-cohort. Regressions are weighted by the total number of students in the baseline year 2010. 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 A12: Effect of Audits on Workforce Composition - Other Characteristics

	Demographic and Socioeconomic Characteristics		
	Female	College Educated Parent(s)	High Family Income
	(1)	(2)	(3)
Panel A: Public Sector			
Audit × Post	0.003	0.005	0.024
	(0.042)	(0.039)	(0.044)
R^2	0.29	0.35	0.40
Mean Dep. Var.	0.63	0.25	0.19
SD Dep. Var.	0.30	0.26	0.24
Observations	53,327	53,327	53,327
Num. of Clusters	1,988	1,988	1,988
Panel B: Private Sector			
Audit × Post	-0.005	0.013	-0.011
	(0.017)	(0.019)	(0.010)
R^2	0.23	0.47	0.57
Mean Dep. Var.	0.59	0.33	0.31
SD Dep. Var.	0.18	0.19	0.19
Observations	63,878	63,878	63,878
Num. of Clusters	1,970	1,970	1,970
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 the share of female students (column 1), the share of students with college-educated parent(s) (column 2), and the share with family income at the top quartile of the distribution (column 3). Panel A reports the sample of students who end up in the public sector, 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 is after the period of audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A13: Effect of Audits on Public Sector Worforce Composition - Contract Type

	Total Num.	Num. by Quartile of ENEM Grades		
		Lowest 50%	Second Highest25%	Highest 25%
	(1)	(2)	(3)	(4)
Panel A: Tenure-Track				
Audit × Post	-0.240*	-0.095	-0.127	-0.250**
	(0.133)	(0.190)	(0.272)	(0.103)
R^2	0.80	0.58	0.59	0.73
Mean Dep. Var.	2.33	1.05	1.17	1.53
SD Dep. Var.	1.00	0.90	0.91	1.10
Observations	35,760	35,760	35,760	35,760
Num. of Clusters	1,711	1,711	1,711	1,711
Panel B: Temporary				
Audit × Post	-0.000	0.102	-0.039	-0.259*
	(0.120)	(0.163)	(0.139)	(0.138)
R^2	0.81	0.70	0.67	0.73
Mean Dep. Var.	1.95	1.11	0.86	0.99
SD Dep. Var.	1.01	0.95	0.92	1.02
Observations	41,312	41,312	41,312	41,312
Num. of Clusters	1,838	1,838	1,838	1,838
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 number (IHS-transformed) of students with ENEM grades at different quartiles of the score distribution (controlling for exam year). Panel A reports the sample of students who become tenure-track civil servants and Panel B reports the sample of those who become temporary public sector workers. 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 A14: Effect of Audits on Public Sector Workforce Composition - Other Categories

	High-Ability Students in Public Sector: Other Categories							
	By Government Branch			By Hierarchical Level			By Occupation Type	
	Executive (1)	Legislative (2)	Judiciary (3)	Federal (4)	State (5)	Municipal (6)	Bureaucrats (7)	Frontline (8)
Audit × Post	-0.189**	-0.932***	-0.480	-0.069	-0.253**	-0.382***	-0.084	-0.241**
	(0.091)	(0.191)	(0.338)	(0.229)	(0.105)	(0.103)	(0.229)	(0.096)
R^2	0.74	0.60	0.58	0.75	0.67	0.67	0.68	0.73
Mean Dep. Var.	1.50	0.50	1.01	1.14	1.24	1.21	1.10	1.41
SD Dep. Var.	1.13	0.55	0.64	0.91	0.98	1.01	0.99	1.10
Observations	57,921	4,038	2,198	2,748	30,976	47,946	35,337	45,064
Num. of Clusters	2,350	288	166	200	1,417	2,166	1,646	2,041
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, zooming into other categories of public sector careers. Dependent variables are the number of high-ability students (IHS-transformed) in the corresponding job category. The unit of observation is municipality-year-cohort. Columns 1-3 report results for careers by the branch of government. Column 4-6 report results for careers by the level of hierarchy. Columns 7-8 report results for careers by occupation type, following definitions of bureaucrats and frontline providers as in Colonnelli et al. (2020b). 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 A15: Effect of Audits on Major Enrollment - Spillovers

	Business/Law (1)	Engineering (2)
Panel A: Spillover effects		
Audit × Post	-0.016*	0.009*
	(0.009)	(0.005)
R^2	0.64	0.77
Mean Dep. Var.	0.29	0.16
SD Dep. Var.	0.08	0.07
Observations	22,966	22,966
Num. of Clusters	690	690
Panel B: Excluding spillover effects		
Audit × Post	-0.016**	0.018***
	(0.008)	(0.005)
R^2	0.64	0.77
Mean Dep. Var.	0.29	0.16
SD Dep. Var.	0.07	0.07
Observations	22,546	22,546
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 A16: Effect Heterogeneity by Level of Uncovered Corruption

	Business/Law		Engineering	
	Share	Num.	Share	Num.
	(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)
p-value (H_0: $\beta_1 = \beta_2$)	0.017	0.052	0.025	0.511
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 A17: Effect Heterogeneity by Local Media

	Business/Law		Engineering	
	Share	Num.	Share	Num.
	(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 A18: Effect of Audits on Degree Vacancies

	Num. of Degree Vacancies	
	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 A19: Effect of Audits on Municipal Employment

	Num. of Total First Hires		
	Public Sector		Private Sector
	Tenure-Track	Temporary	
	(1)	(2)	(3)
Panel A: RAIS (2010-2018)			
Audit × Post	0.067	0.010	0.016
	(0.194)	(0.143)	(0.039)
R^2	0.67	0.80	0.97
Mean Dep. Var.	2.83	3.07	6.89
SD Dep. Var.	2.28	2.43	1.68
Observations	157,169	157,169	157,169
Num. of Clusters	3,693	3,693	3,693
Panel B: RAIS (2002-2018)			
Audit × Post	0.308***	0.103	0.008
	(0.094)	(0.097)	-0.023
R^2	0.62	0.73	0.96
Mean Dep. Var.	2.70	2.85	7.04
SD Dep. Var.	2.27	2.33	1.66
Observations	531,786	531,786	531,786
Num. of Clusters	5,348	5,348	5,348
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 (IHS 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 year is after the year of interest. 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:

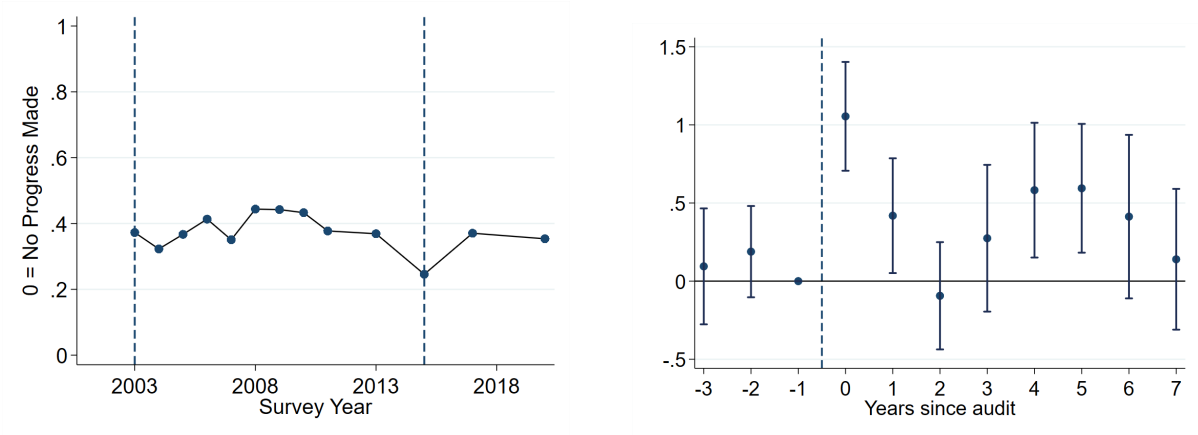
How much progress do you think has been made on reducing corruption in the state institutions during the last 2 years?

I follow the same estimation strategy as outlined in section 4.1, where the outcomes are standardized answers recorded in *Latinobarómetro* spanning from 2001 to 2020. One challenge with using the *Latinobarómetro* survey is that the geolocators provided for Brazil are the names of municipalities as well as the 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 (2003-2015). However, audits do seem to alter 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 reverses back to positive when the corrupt politicians and public officials start facing legal consequences in subsequent years. 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 likely 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 help corroborate 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*

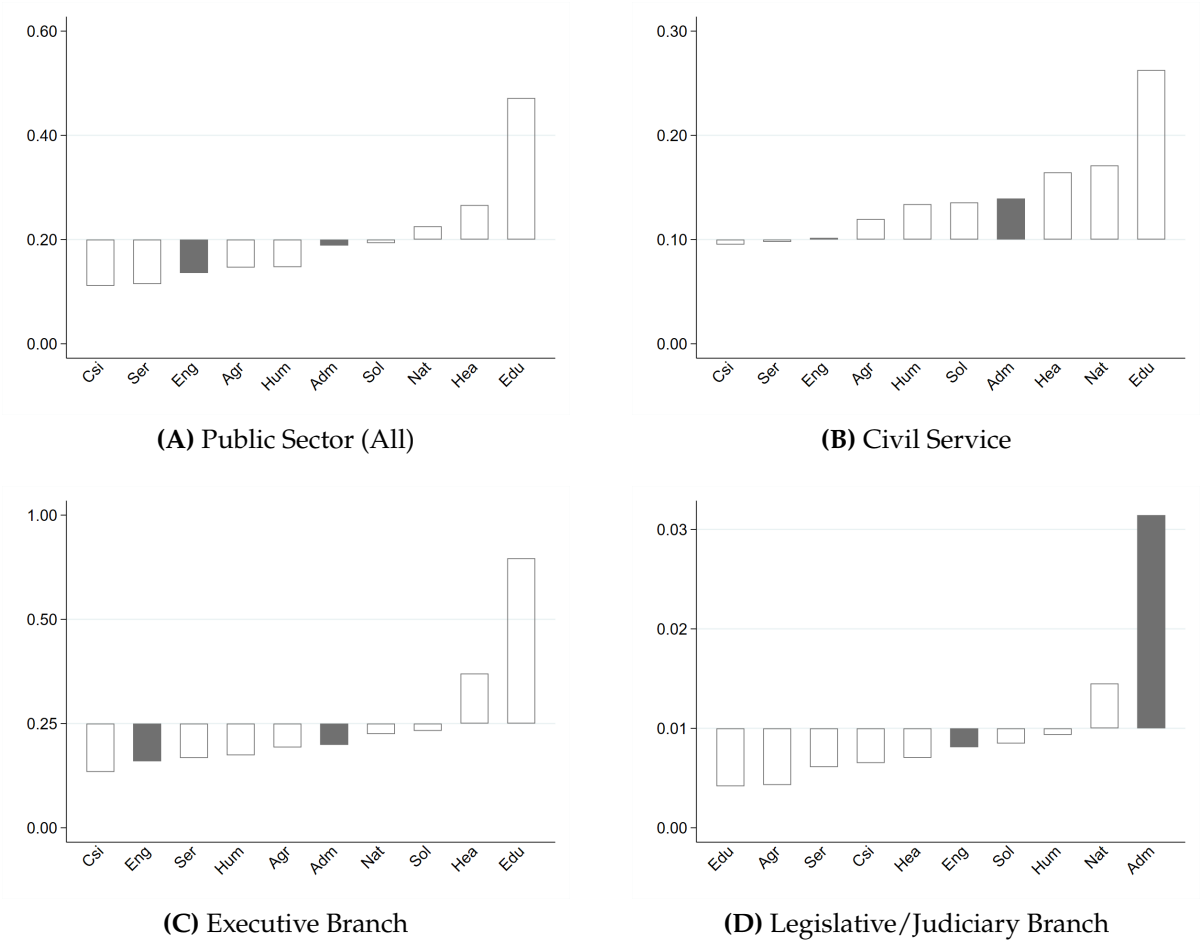
	Perception of Corruption		Trust in Institutions		
	(1) Problem	(2) Progress	(3) Congress	(4) Fed. Gov.	(5) Judiciary
Audit × Post	-0.072	0.167	-0.058	-0.075	-0.008
	(0.097)	(0.127)	(0.113)	(0.125)	(0.095)
R^2	0.72	0.70	0.71	0.63	0.61
Mean Dep. Var.	0.11	0.40	0.38	0.42	0.48
SD Dep. Var.	0.14	0.15	0.15	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), 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 2010. Note that due to data availability of RAIS and data attrition as explained in footnote 35, I eventually trace about 70,000 students of the 2010 enrollment cohort to their first jobs in the formal labor market.

Figure C1: Mapping of majors to early careers



(A) Public Sector (All)

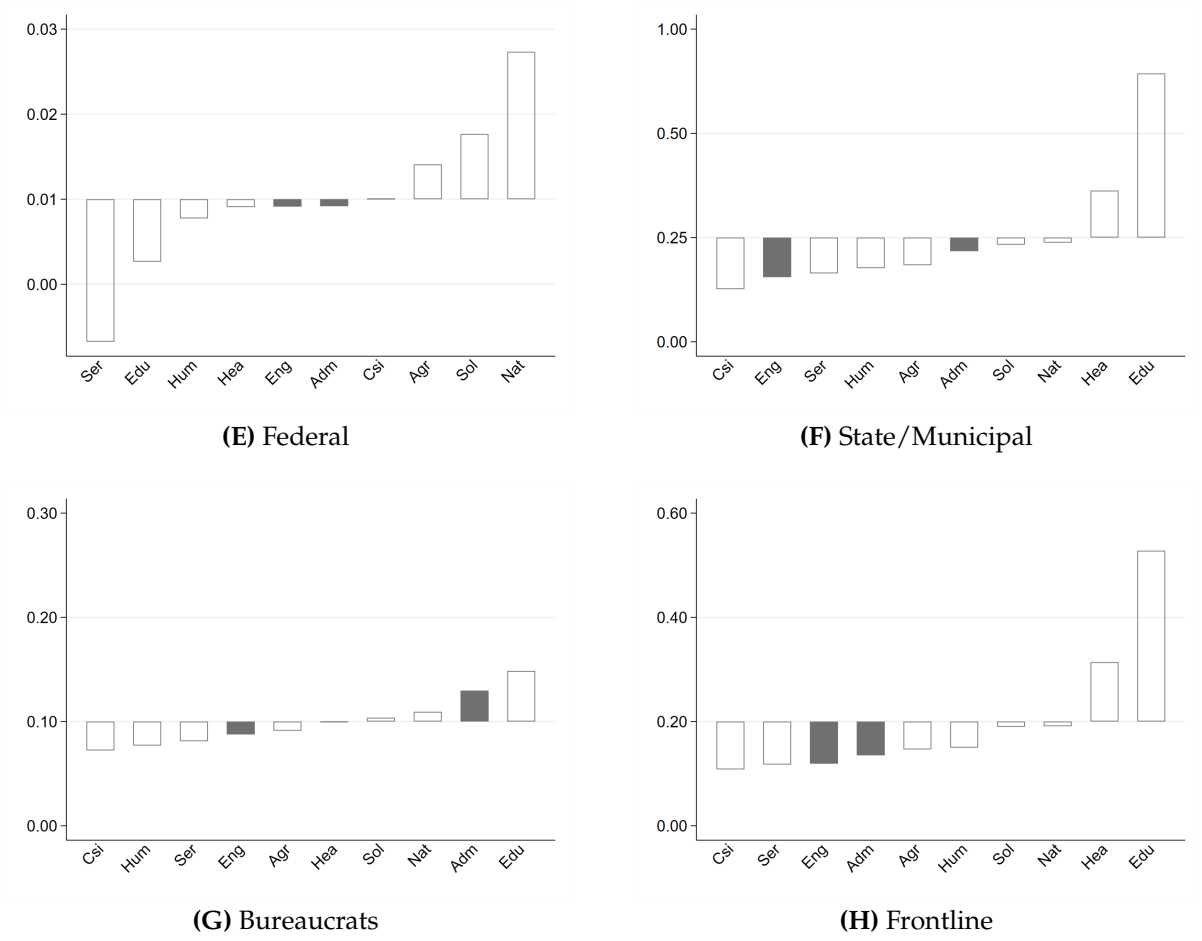
(B) Civil Service

(C) Executive Branch

(D) Legislative/Judiciary Branch

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, C, F, and H) while business/law degrees stand out particularly for the legislative/judiciary branches of the government (Panel D) as well as for bureaucratic positions (Panel G). As a comparison,

Figure C1: Mapping of majors to early careers (continued)



(E) Federal

(F) State/Municipal

(G) Bureaucrats

(H) Frontline

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 the shares for tenure-track civil servants. Panels C and D display results for executive branch and non-executive (legislative and judiciary) branch. Panels E and F display results for federal versus local (state and municipal) government positions. Panels G and H display results by occupation type, following definitions of bureaucrats and frontline providers as in Colonnelli et al. (2020b).

engineering degrees are under-represented across different public sector positions overall.

It is worth noting that education as a field of study stands out as an exception. Results from Appendix Table A3 show that audits have a slight positive effect on enrollment in education, although the coefficient is imprecisely estimated. Several reasons could explain the "outlier" behavior of the education major. First, 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. Bureaucratic corruption

involving misappropriation or embezzlement of fiscal transfers might be less relevant for frontline providers such as public school teachers and healthcare providers, 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, education degrees 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 in hiring practices. 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). These reasons highlight that education should be treated as a special case as opposed to other fields of study.

Table C1: Audits and Major Enrollment

	High Public Employability		Low Public Employability	
	Share	Num.	Share	Num.
	(1)	(2)	(4)	(5)
Audit × Post	-0.018***	-0.012	0.018***	0.067*
	(0.007)	(0.026)	(0.007)	(0.037)
R^2	0.70	0.98	0.69	0.98
Mean Dep. Var.	0.72	6.42	0.27	5.41
SD Dep. Var.	0.09	1.57	0.09	1.71
Observations	169,886	169,886	169,886	169,886
Num. of Clusters	3,693	3,693	3,693	3,693
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. The dependent variables are the shares of freshmen enrolled (columns 1 and 3), as well as the corresponding numbers in inverse hyperbolic sine transformation (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 year is after the year of interest. Panel A reports estimates for all students, pooling public and private universities. Panels B and C report estimates separately for students attending private and public universities, respectively. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

To help further alleviate concerns regarding the arbitrariness of focusing only on business/law and engineering degrees in the main analysis, I also construct an alternative "public sector employability" index, grouping majors by their baseline propensity for civil service careers. Based on evidence in Figures 2 and C1, I categorize education, health, business/law, humanities, and social sciences into high "public sector employable" majors, while low "public sector employable" majors include STEM degrees (including engineering) and the remaining smaller fields such as agriculture and services. The results

shown in Table C1 are very similar to those in Panel A of Table 2. These results, combined with effects decomposing each individual field of study in Table A3, illustrate that audits drive students away from public-sector-oriented majors, as manifested by business administration and law.

D Audits and Out-Migration

In this appendix, I discuss anti-corruption audits and students' decisions to migrate. 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 is after the period of audit. 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	0.135**	-0.059**	0.045*
	(0.060)	(0.027)	(0.024)
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	57348	57348	57348
Num. of Clusters	2299	2299	2299
Panel B: Work Muni. Different Than Residence (Movers)			
Audit × Post	0.105	-0.058***	0.046***
	(0.090)	(0.017)	(0.016)
R^2	0.93	0.27	0.28
Mean Dep. Var.	2.59	0.27	0.17
SD Dep. Var.	1.52	0.21	0.17
Observations	66125	66125	66125
Num. of Clusters	2565	2565	2565
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 is after the period of audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

E Categorization of Occupations in Brazil

In this appendix, I discuss how I classify occupations in Brazil by their level of rent-seeking and prosociality.

I begin by conducting a keyword search through the job description texts of the classification of Brazilian occupations (*Classificação Brasileira de Ocupações*, or CBO), available for download on the official website of the Ministry of Labor and Employment, and assign a "rent-seeking" score and "prosocial" score for each 4-digit CBO code based on numbers of keywords matched. I rely on GPT-4 for the initial classification, but also manually check the plausibility of the final categorization (Korinek, 2023).

Below are the lists of keywords (in Portuguese) for "rent-seeking" and "prosocial", respectively

- *Rent-seeking*: análise, aquisição, aquisições, arrecadação, auditam, auditar, auditor, auditores, auditoria, autorização, autuação, benefício, compras, concessão, concessões, contrato, contratos, contratual, controle, correção, decisão, fiscais, fiscal, fiscalizar, fiscalização, gestão de recursos, imposto, impostos, incentivo, interdição, investigação, legislação, lei, leis, licença, licitação, licitações, multa, multar, multas, norma, orçamento, orçamentos, orçamentária, orçamentário, pagamento, parecer, penalidade, permissão, poder, recolhimento, reguladora, regulamenta, regulamentação, regulamento, regulatório, regulação, repasse, sanção, transferência, tributação, tributo, tributos, tributária, tributário, vigilância, agente fiscal, atos normativos, autarquia, autoridade, benefícios, burocracia, cartório, certificação, compliance, concorrência, consultoria jurídica, decisório, deliberação, faturamento, formalização, gestor público, indenização, instrução normativa, inquérito, julgamento, jurídico, licenciamentos, minuta, normativo, parecerista, prestação de contas, processo administrativo, regime jurídico, remuneração, rendimentos, repartição, subsídio, taxação, vinculação, vinculado, aquisição, aquisições, compras, licitante, licitantes, licitação, licitações, contrato, contratos, contratual, contratação, contratante, fornecedor, fornecedores, pregão, dispensa, edital, orçamento, orçamentos, planejamento de compras, requisição, cotação, proposta, propostas, adjudicação, homologação, recurso administrativo

- *Prosocial*: acolher, acolhimento, apoio, assistencial, assistência, atendimento, capacitação, clínica, clínicas, comunidade, conselheiro, conselho tutelar, criança, cuidado, cuidador, cuidados, doente, doentes, educação, emergência, emergências, enfermagem, enfermeira, enfermeiras, enfermeiro, enfermeiros, ensino, escuta, família,

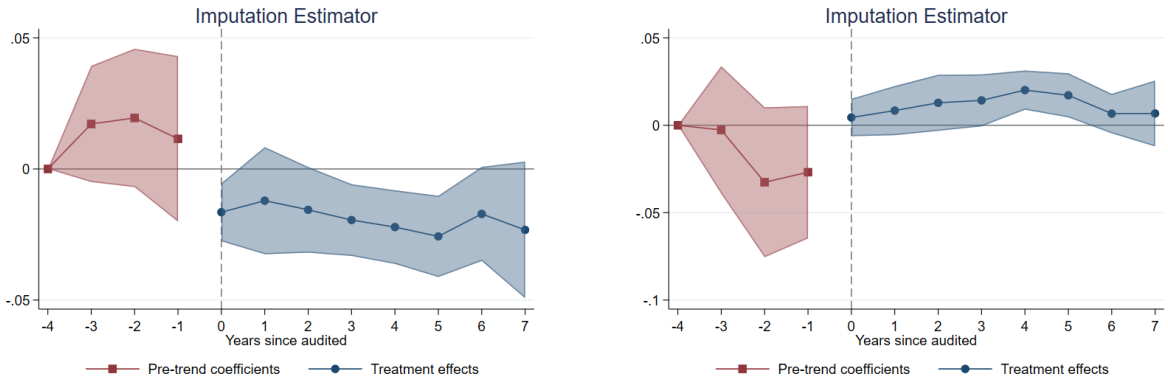
formação, hospitais, hospital, hospitalar, idoso, idosos, inclusão, instrutor, mediação, monitoria, orientação, paciente, pacientes, pedagógico, proteção, psicológico, psicossocial, reabilitação, resgatam, resgatar, resgate, saúde, social, socorro, solidariedade, solidário, terapia, tratamento, voluntária, voluntárias, voluntário, voluntários, acompanhamento, acolhedor, ajuda, ajudante, aluno, alunos, aprendizagem, assistente social, atenção, bem-estar, cuidadores, docente, educador, educadores, enfermar, formador, intervenção, orientador, pedagogo, prevenção, psicologia, refugiado, reintegração, relação de ajuda, sensibilização, serviço social, solidarismo, suporte, tutoria, vulnerável, vulneráveis

The overall correlation between the prosocial score and rent-seeking score across all occupations is -0.05, while the median number of rent-seeking and prosocial counts are 1 and 4, respectively. I then categorize occupations into high or low rent-seeking based on whether the occupation scores above- or below-median "rent-seeking" score from the keyword search, and similarly for high and low prosocial occupations.

Example occupations (along with their 4-digit CBO codes) that score the highest in terms of "rent-seeking" are state and municipal tax inspectors (2544), social security tax auditors (2542), federal revenue auditors and technicians (2541), metrological and quality inspection agents (3523), road transport technicians (3423), administrative supervisors (4101); while occupations at the top of the "prosocial" list are nursing technicians and assistants (3222), community health agents and related occupations (5151), caregivers of children, youth, adults and the elderly (5162), dental hygienists and technicians (3224), psychologists and psychoanalysts (2515), occupational therapists and related occupations (2239).

F Robustness Checks

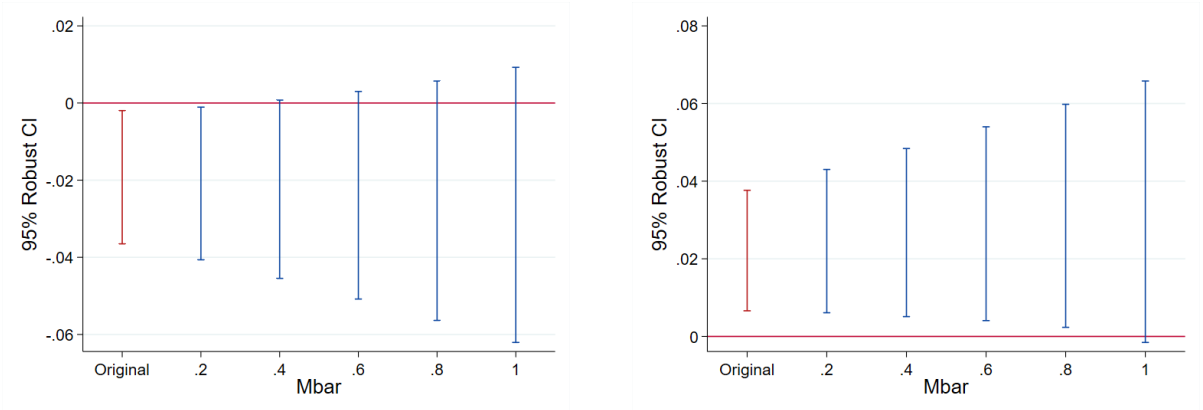
Figure F1: Audits and major enrollment - alternative estimator



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

Notes: This figure presents event study estimators for the effects of audits on shares of freshmen major enrollment (pooling public and private universities), using the *did_imputation* approach (Borusyak et al., 2024).

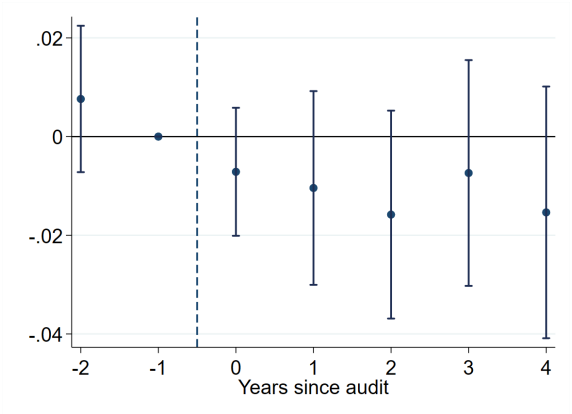
Figure F2: Parallel trends sensitivity analysis via Rambachan and Roth (2023)



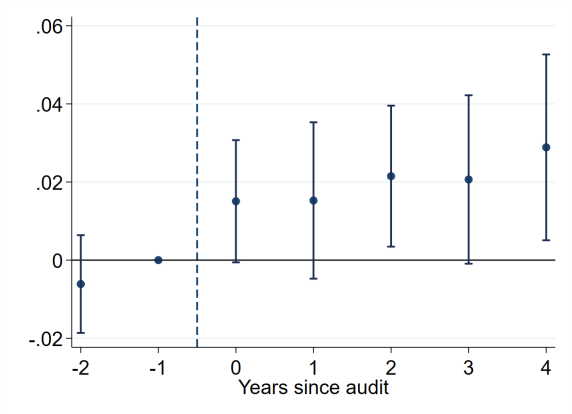
(A) Share in Business/Law **(B)** Share in Engineering

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 stacked DiD estimator in equation 1. The sample includes all students pooling public and private universities. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

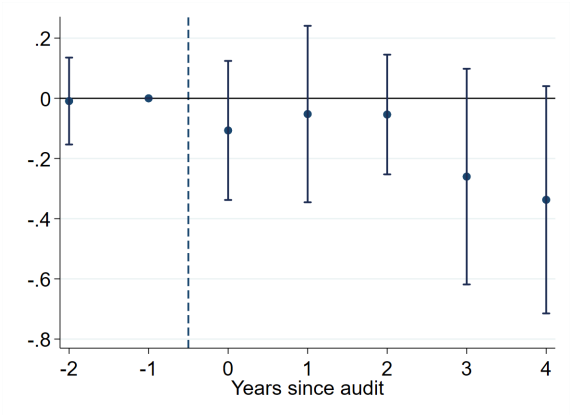
Figure F3: Audits and majors & careers - balanced panel



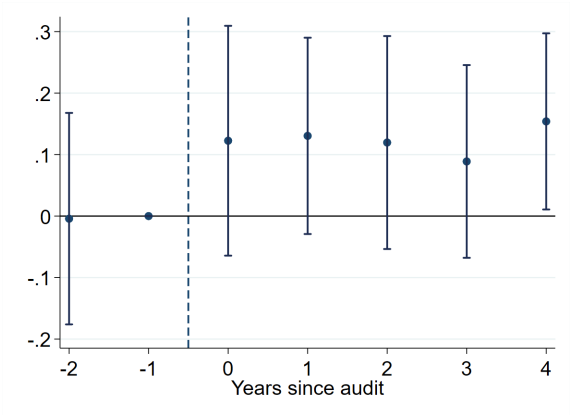
(A) Share in Business/Law



(B) Share in Engineering



(C) Number in Public Sector



(D) Number in Private Sector

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 shortened to [-2,4]. Reporting 95% confidence intervals. Standard errors are clustered at the municipality level.

Table F1: Alternative Estimation Methods to IHS Transformation

	(1)	(2)	(3)	(4)
Panel A: Major Enrollment	Poisson		Log Transformation	
	Business/Law	Engineering	Business/Law	Engineering
Audit × Post	-0.021	0.345***	-0.042	0.099*
	(0.034)	(0.074)	(0.029)	(0.057)
Implied Prop. Effect	-0.021	0.412***		
	(0.033)	(0.104)		
R^2			0.98	0.97
Mean Dep. Var.	323.91	175.34	4.75	4.07
SD Dep. Var.	439.80	246.22	1.66	1.73
Observations	169,835	169,477	164,179	145,926
Num. of Clusters	3,693	3,686	3,692	3,659
Panel B: Career Realization	Poisson		Log Transformation	
	Public Sector	Private Sector	Public Sector	Private Sector
Audit × Post	-0.024	0.161***	-0.094	0.155***
	(0.069)	(0.054)	(0.118)	(0.059)
Implied Prop. Effect	-0.023	0.175***		
	(0.068)	(0.064)		
R^2			0.83	0.96
Mean Dep. Var.	22.63	283.98	1.88	3.54
SD Dep. Var.	31.03	402.45	1.12	1.57
Observations	110,584	114,062	62,363	77,430
Num. of Clusters	3,094	3,252	2,460	2,615
Muni. × Cohort FE	X	X	X	X
State × Year × Cohort FE	X	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, this table reports results when dependent variables are the raw numbers and the coefficients are estimated using Poisson quasi-maximum likelihood estimation (QMLE) in columns 1-2, and when using simple log transformation in columns 3-4. 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, 2024). 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 F2: Effect of Audits on Shares of Major Enrollment - Robustness

	Balanced Panel		Time is Semester		Hybrid-Included	
	Bus./Law	Eng.	Bus./Law	Eng.	Bus./Law	Eng.
	(1)	(2)	(3)	(4)	(5)	(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 F3: Audits and Civil Servants - Alternative Sample Restrictions

	full sample	$n > 0$	$n > 1$	$n > 2$
	(1)	(2)	(3)	(4)
Panel A: Num. of Public Sector Workers				
Audit × Post	-0.095	-0.096	-0.112	-0.139
	(0.115)	(0.117)	(0.125)	(0.134)
R^2	0.84	0.84	0.83	0.83
Mean Dep. Var.	2.33	2.60	2.84	3.03
SD Dep. Var.	1.30	1.08	0.93	0.83
Observations	96,153	62,363	34,225	21,416
Num. of Clusters	3,036	2,460	1,499	989
Panel B: Num. of High-Ability Students Among Public Sector Workers				
Audit × Post		-0.295***	-0.307***	-0.319***
		(0.083)	(0.083)	(0.081)
R^2		0.77	0.75	0.75
Mean Dep. Var.		1.66	1.85	2.03
SD Dep. Var.		1.19	1.14	1.08
Observations		62,363	34,225	21,416
Num. of Clusters		2,460	1,499	989
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 public sector workers 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 worker 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 workers. The dependent variable in Panel A is the total number (IHS-transformed) of students entering public sector and in Panel B it is the number ((IHS-transformed)) of public sector workers 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 is after the period of audit. Standard errors are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.