Anti-Corruption and Selection into State Jobs: Evidence from China

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Abstract

This paper studies the impact of corruption crackdown on the quality of state-sector candidates, leveraging China's staggered anti-corruption inspections that have dampened perceived corruption returns. Using unique applicant data from state organizations, I find that the anti-corruption induces positive selection for integrity and prosociality into the state sector, without significantly affecting overall ability. These compositional shifts are associated with improved post-recruitment performance. Further evidence suggests talent reallocation as a prominent mechanism: following the shock, individuals with higher integrity and prosociality show higher preferences for state jobs — even when conditioned on ability and other psychometric traits. I further document dynamic effects wherein households increase investment in the human capital and integrity of the next generation, which may reinforce the allocational effects in the longer run. Collectively, these findings shed light on how reward structures shape the state sector's human capacity.

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1 Introduction

Human capital is a key resource of the state sector, shaping the allocation of public resources and the quality of governance (Dal Bó, Finan and Rossi, 2013; Fenizia, 2022; Best, Hjort and Szakonyi, 2023; Muñoz and Otero, 2025). Attracting appropriate talent — those who are not only capable but also possess the integrity to serve the public — is therefore important for state organizations (Francois, 2000; Caselli and Morelli, 2004). Yet, in a society in which corruption is widespread, the relative rewards of rent-seeking vs. productive activities (the reward structure) may distort talent allocation (Baumol, 1990; Murphy, Shleifer and Vishny, 1991, 1993; Acemoglu, 1995; Ehrlich and Lui, 1999), affecting the quality of political/bureaucratic candidates and thus the functioning of the state. Despite the conceptual significance, real-world empirical evidence of this remains scarce.

In this paper, I ask: how does a nationwide anti-corruption crackdown shape the quality of candidates to the state sector? Exploiting the staggered anti-corruption inspections in China and unique applicant data, I find that state-sector applicants from cities exposed to anti-corruption shocks exhibit significantly higher integrity and prosociality, without compromising the overall ability of the candidate pool. These shifts likely translate into greater workplace performance, and I document suggestive evidence that talent reallocation is one salient mechanism.

In 2013, China initiated an unprecedented top-down anti-corruption campaign. To realize anti-corruption efforts down to local societies in such a large polity, the country has implemented staggered anti-corruption visits by dispatching independent inspection teams to scrutinize local governments. These visits may help promote a heightened awareness of potential corruption risks at the local level, thereby generating relevant variation across cities and time (Marquis and Yang, 2014; Chen, 2023). The setting thus allows me to empirically examine how corruption opportunities affect candidate types in a real-world context: in a staggered difference-in-differences (DiD) framework, I compare the quality of applicants from regions that experienced an inspection to those from regions that did not, holding the target organization constant. Following the literature on public personnel selection (e.g., Caselli and Morelli, 2004; Gregg et al., 2011; Hanna and Wang, 2017; Barfort et al., 2019; Ashraf et al., 2020), I focus on integrity/prosociality and ability — the two key dimensions that determine the quality of governance.[1]

[1] As laid out by Caselli and Morelli (2004), ability and integrity constitute the two central dimensions of the quality of public officials: ability (competence) is the skill to identify the appropriate policy objectives and achieve them effectively, and integrity is the character that leads an official to perform their duties without stealing and harassing citizens. Notably, while integrity and prosociality (or more broadly, public service motivation) are conceptually not identical, they overlap to a discernable extent: prior empirical studies — as well as this paper — consistently find that these traits are positively correlated (Hanna and Wang, 2017; Barfort et al., 2019). Therefore, I do not distinguish between integrity and prosociality further in the conceptual discussion, but I show in the

In theory, corruption produces additional rent-seeking returns for state jobs (and thus lowers the relative values of private jobs for high-ability individuals), but it also imposes disutility on more honest and prosocial individuals. Additionally, it may influence the broader population's accumulation of integrity and prosociality traits, though such effects are more likely to unfold in the longer run. Accordingly, the net impact of corruption returns on the incoming state workforce's quality will depend on the relative utility gains by individual type, which necessitates empirical investigation. This is also of significant policy relevance, as policymakers and media outlets often frame anti-corruption efforts in terms of an inevitable integrity–ability trade-off.[2]

I start with a sanity check that city-level anti-corruption visits are associated with lower perceived returns to (or greater punishment for) corruption. To enhance the validity of the DiD design, I demonstrate that a range of local socioeconomic characteristics — including both their levels and growth rates — cannot consistently predict the timing of anti-corruption visits. I also discuss the relevance of city-level treatment: as the city is the basic unit of socioeconomic activity for most citizens, local anti-corruption shocks can plausibly raise the crackdown's salience (Colonnelli and Prem, 2022).[3] Analyzing data from a representative survey, I show that local residents perceive a greater punishment for corruption after their city has been inspected, with non-differential trends prior to the treatment.

The main analysis explores how anti-corruption affects the quality of candidates for state jobs. I leverage unique entry-level applicant data from a leading human resource platform, covering 90 sampled state organizations in two provinces between 2011 and 2017.[4] Crucially, the data provide a psychometric integrity measure, as well as scores for cognitive ability and other personality traits of applicants by their cities each year. These measures are scored by a comprehensive psychometric assessment system and have been shown to be useful predictors of workplace performance (Perry and Hondeghem, 2008; Heckman, 2011; Klinger, Khwaja and Del Carpio, 2013; Callen et al., 2022); in an incentivized cheating task, I further corroborate that such psychometric measures of integrity and prosociality are strongly correlated with elicited dishonesty (Fischbacher and Föllmi-Heusi, 2013). For the identification strategy, to mitigate the confounding role of recruitment-side changes, I exploit the variation generated by anti-corruption visits to applicants' cities of college attendance — where these individuals primarily reside and experience social changes before entering the workforce. Specifically, I

empirical analysis that the effects of anti-corruption are consistent on both measures.

[2] See for example: https://www.economist.com/china/2015/06/06/who-wants-to-be-a-mandarin.

[3] A city, the second-level administrative division in China, typically has around 3 million residents. As of 2013, there are officially 333 city-level divisions in the country.

[4] Each province has an extensive geographical span, comparable to the size of England, which makes the local inspection treatment plausibly relevant.

estimate a DiD by comparing the traits of individuals applying *before* their college cities have been inspected to those of individuals applying *after* their college cities have been inspected, holding the position constant (i.e., only comparing those who apply to the same organization in the same year).

My main analysis finds that state job applicants from city-year cells that experienced an anti-corruption visit score significantly higher on integrity and public service motivation, while showing no significant difference in cognitive ability on average. The effect is more pronounced for applicants exposed to more intense corruption crackdowns, as measured by the number of senior officials investigated. The results remain robust when using alternative DiD estimators and inference criteria. Given the immediate jump in post-treatment estimates from the event study, the observed change in candidate quality appears consistent with a fast-moving talent reallocation mechanism (Gregg et al., 2011) — whereby individuals with greater integrity are more attracted to state jobs after the crackdown. To complement the organization-city-year level aggregates, I further access unique individual-level candidate data from five of the sampled organizations. The empirical patterns remain consistent.

Furthermore, I document that these shifts in candidate quality likely translate into workplace performance. I digitize evaluation reports for all candidates recruited by these same five sampled organizations, which provide comparable performance assessments for the fiscal year 2019. The estimates indicate higher performance ratings for treated recruits (i.e., those exposed to college-city anti-corruption shocks prior to application). Notably, such performance changes could not only reflect changed candidate quality but also be amplified by screening processes (Ashraf et al., 2020). Linking candidate–employee data, I find that state organizations place strong weight on candidate ability, with integrity also positively associated with admission likelihood. Importantly, however, tests of coefficient equality suggest that recruiters do not apply different selection criteria to candidates from inspected *versus* non-inspected cities. Meanwhile, employees' performance is positively related to their psychometric measures at the application stage. Given that admission standards remained largely uniform across groups, these combined results suggest that part of the observed workplace performance changes may stem from candidate pool changes.

For the mechanism analysis, I present evidence suggesting talent reallocation (sorting by type) as one important channel in the short run, while also testing alternative explanations. I begin by examining two channels closely linked to decreased rewards of corruption: (i) the *allocational margin* — individuals with greater integrity and prosociality sort into state jobs more; and (ii) the *incentive margin* — individuals endogenously become more honest and prosocial.

First, I use college student surveys conducted in one sampled province (2015 – 2017), which contain both psychometric assessments and job-seeking preferences of prospective graduates. The DiD estimates show no significant differences in integrity or other psychometric scores between cohorts exposed to anti-corruption visits and those that were not. This implies that changes in traits play a relatively modest role (in the short run). Then, to explore the allocational margin, I compare changes in state job preferences across students with different traits. I find substantial differences as a function of student integrity and prosociality: students featuring higher integrity and prosociality scores reveal a stronger preference for state jobs following anti-corruption shocks, compared to those with lower scores. Further controlling the differential effects by cognitive ability and other personality traits does not alter this pattern.

Second, to corroborate the allocational channel in a broader population, I draw on a nationally representative household panel of middle-school student-parent pairs since 2013. This dataset includes pre-constructed measures of *ex-ante* student traits and tracks within-household changes in desirable future jobs. Again, I find that households with more *ex-ante* prosocial students view state jobs as more desirable following the corruption crackdown. The effect remains unchanged to controls for cognitive ability and socioeconomic background. These findings — together with the heightened anti-corruption perception — suggest talent reallocation driven by the decreased rewards of corruption as one logical mechanism.

I then examine alternative mechanisms less related to corruption returns. One, I present qualitative and quantitative evidence that the observed sorting pattern is unlikely to be driven by career uncertainty or security concerns. The sorting effect on integrity and prosociality remains significant even after controlling for individual risk appetite. Two, I consider potential downstream changes in private sector growth and economic conditions, which could also affect the relative economic values of state *versus* private jobs. My core results remain robust after accounting for city-year socioeconomic characteristics — including state-sector size and public-private wage gaps — as well as heterogeneity in state job preferences by household socioeconomic status, suggesting these factors may not confound the main effects. Three, beyond expected job utility, I also study perceptions of job-seeking competitiveness. In the college student survey, individuals with higher integrity scores do not perceive themselves as significantly more competitive in the job market following the crackdown. This suggests that perceived likelihood of job attainment plays a limited role in this context. Overall, while factors unrelated to corruption returns may be present, they appear less prominent in explaining the differential selection by integrity and prosociality into the state sector.

The final part of the paper discusses broader implications. First, I explore the role of spillover effects — to what extent citizens read inspection signals beyond their immediate localities. Following the spirit of Colonnelli and Prem (2022), I re-estimate the effects by using a "cleaner" control group — cities not adjacent to an inspected one — to address geographic spillovers. The candidate sample reveals moderate yet discernible spillover effects (approximately 18% of the baseline magnitude), implying that the true nationwide effects may exceed estimates based solely on direct exposure. Second, although short-term shifts in personality traits appear limited, they can prove more malleable over the longer run (Tirole, 1996; Ehrlich and Lui, 1999). Using the middle-schooler panel, I find that households focus more attention on the conduct and wrongdoing of the next generation. While more speculative, these findings suggest a potential dynamic effect on the future talent pool: although immediate changes in the stock of traits may be small, such shifts may materialize in the longer run, augmenting the allocational margin and reinforcing path dependence.

Taken together, this article relates to three main strands of literature.

First, it links canonical theories of talent allocation to the growing literature on the personnel economics of the state (Finan, Olken and Pande, 2017; Besley et al., 2021). While cross-sectional studies suggest that high-corruption contexts attract dishonest candidates into state jobs (whereas low-corruption ones attract honest types),[5] causal evidence remains scarce due to the observational equivalence problem and the challenge of measuring candidate quality. The rich real-world data and within-society variation allow me to integrate existing cross-sectional findings and make progress in understanding how candidate quality changes (Jiang, Shao and Zhang, 2020; Lai and Li, 2024; Xun, 2024; Ang et al., 2025).[6] My findings suggest that anti-corruption efforts can substantially reshape the state's talent pool, without necessarily trading off integrity and public service motivation for overall competence. This echoes the long-standing debate on political selection and intrinsic motivation in public employment (Bénabou and Tirole, 2003; Besley and Ghatak, 2018). By investigating endogenous changes in candidate pools, the paper also adds to the limited work on the supply of public workers (e.g., Dal Bó, Finan and Rossi, 2013; Hanna and Wang, 2017; Deserranno, 2019; Ashraf et al., 2020).

Second, this study contributes to the consequences of corruption for human capital. A well-

[5] Banerjee, Baul and Rosenblat (2015), Hanna and Wang (2017), Gans-Morse (2021), and Cruces, Rossi and Schargrodsky (2023) find self-selection of dishonest types into state jobs in Indian, Argentinian, and Ukrainian contexts respectively. In contrast, Barfort et al. (2019) document that honest students in Denmark, a low-corruption country, prefer state jobs more. There is no strong correlation between ability and state job preferences in any of these studies. All of these studies hold the reward structure to corruption constant. One related exception is Brassiolo et al. (2021), where the authors experimentally manipulate corruption opportunities among college students in a lab, and they observe a sorting of dishonesty into high-graft jobs.

[6] In the context of anti-corruption programs, Ang et al. (2025) examine impacts on occupational mobility based on student surveys, Jiang, Shao and Zhang (2020) study changes in the GPA and background composition of Masters of Public Administration attendees, Lai and Li (2024) examine the number of applicants to inspected departments, and Xun (2024) studies Brazilian students' major choices. This paper takes a step back by examining the quality of real applicants, including psychometric traits beyond cognitive ability and their post-recruitment performance, and by using survey data to directly observe job preferences.

documented empirical channel is the indirect course — distorted incentives of politicians and bureaucrats impair the delivery of education and health services.[7] In contrast, I concentrate on a more direct channel via which corruption alters incentives for individuals to allocate their talent. This speaks to the broader question of whether corruption is a self-reinforcing equilibrium. Additionally, my analysis also delves into an under-studied scenario in which productivity has not yet been realized, by examining anti-corruption's role in shaping the occupational preferences of youngsters (Reinikka and Svensson, 2005; Ajzenman, 2021).

Finally, my findings echo the recent work in labor economics linking perceived returns to actual economic decisions and outcomes (e.g., Arcidiacono et al., 2020; Gong, Stinebrickner and Stinebrickner, 2020; Kuka, Shenhav and Shih, 2020; Wiswall and Zafar, 2021). These studies emphasize the role of personal perceptions about education, occupations, or family life in shaping individual behavior and realized outcomes. In a similar spirit — though more suggestive — my combined results imply that citizens' perceived returns to corruption can translate into behavioral changes, reshaping the immediate career aspirations of the youth.

2 Context

China represents an appealing setting to examine my research question. It features a middling state-capacity environment with a substantial potential for corruption (Cuneo, Leder-Luis and Vannutelli, 2023; Fang, 2023), making the sharp, top-down corruption crackdown relevant and noticeably large in magnitude to identify.

2.1 State Jobs in China

There are two key features of state jobs in the Chinese setting. First, the state sector plays a central role in China's economy,[8] and the decisions of public officials have a substantial bearing on the whole society (e.g., Chow, 2015; Haveman et al., 2019; Acemoglu, Yang and Zhou, 2022; Beraja, Yang and Yuchtman, 2022). Also, there is a corresponding state-wage premium relative to the private sector, ranging from 1.55 to 3.09 across provinces in 2010 (Bai et al., 2025). Second, compared with private sector jobs, state sector positions in China — especially before Xi's anti-corruption campaign — were generally associated with plentiful room for rent-seeking, with a narrow possibility of punishment (Cai, Fang and Xu, 2011; Chen and Kung, 2019; Chu,

[7] See for example Mauro (1998), Gupta, Davoodi and Tiongson (2001), Gatti, Gray-Molina and Klugman (2003), Tanzi (2004), Reinikka and Svensson (2005), Olken (2006), Ferraz, Finan and Moreira (2012), Lichand, Lopes and Medeiros (2016), and Zamboni and Litschig (2018)

[8] In the 2010s, state-owned enterprises still accounted for over 60% of China's market capitalization (calculated based on the Chinese Statistic Yearbook). A wide range of important industries are heavily influenced by the state (e.g., banks and the financial sector, energy, land transactions, education, health provision, etc.)

Kuang and Zhao, 2019; Fang, Gu and Zhou, 2019).

Competition for entry into the state sector is strikingly fierce: in the 2010s, applicants registering for national-level civil service exams had only a 1-in-68 chance of being recruited on average. The recruitment of the state sector in China puts a strong emphasis on screening ability — as in most other countries — and appears particularly successful (Bell, 2016; Yao, 2018). However, the sorting of rent-seekers and pervasive corruption remain challenges for the Chinese Communist Party (Liu, 1983; Manion, 2004; Jiang, Shao and Zhang, 2020).[9] This observation speaks to the theory of reward structures: in an institutional setting with abundant opportunities to profit from corrupt behavior, the state sector may likely attract dishonest individuals, who have a greater willingness to steal and also a comparative advantage in rent extraction (Caselli and Morelli, 2004; Hanna and Wang, 2017).

2.2 Anti-corruption Inspection Visits

China's Anti-Corruption Crackdown. The anti-corruption campaign was initiated at the end of 2012, when Xi Jinping took power. In his inaugural speech, President Xi made special mention of corruption, emphasizing it as a great threat to the Party's survival. On December 6th of that year, the deputy leader of Sichuan Province was abruptly investigated, marking the beginning of Xi's anti-corruption campaign, the largest corruption crackdown in recent history.

Despite Xi's initiatives and policy signals, in its early stage, the media and the public viewed this campaign skeptically, wondering whether it was just a short-term show aimed at a power struggle (Marquis and Yang, 2014; Lorentzen and Lu, 2018). Moreover, in a large-scale polity where top leadership is far distant from local societies, curbing grassroots corruption and shifting the perception of ordinary citizens can hardly be achieved in one stroke. These challenges motivate the rise of the inspection visit institution — an apparatus that sends anti-corruption efforts down to local societies in a staggered manner (Xu, 2014).

Ex-post, the severity of the crackdown has shown to be both unprecedented and unanticipated. The campaign pledged to target both top-ranked officials (known as "tigers") and grassroots officials (known as "flies"). While some argue the former may combine with political motives, the latter are generally believed to feature more about combating local corruption (Zhu, Huang and Zhang, 2017; Xi, Yao and Zhang, 2021; Chen, 2023). By the end of 2020, 394 provincial and higher ranking officials had been investigated, contrasting with the 151 investigated in 1993–2012. At the local level, approximately 20,000 local state employees face internal discipline or public investigation each year. Notably, over 60% of local cases are brought

[9]China was ranked 80th in 2013 in terms of the Corruption Perception Index (CPI), according to *Transparency International*. A vast literature has also documented the role of rent-seeking in the Chinese political economy (e.g., Fisman et al., 2018; Kung and Ma, 2018).

to light by the inspection visit institution, according to official accounts.[10]

The Inspection Visit Institution. The inspection visit (or "anti-corruption visit") institution contributes prominently to the top-down anti-corruption campaign. A powerful tool in Xi's corruption crackdown, this independent apparatus dispatches inspection teams to local governments and state organizations, conducting intensive audits and gathering clues from citizens to derive insights regarding local corruption at different time points. Relevant to my empirical strategy, the inspection visit institution contributes to the staggered variation in heightened anti-corruption salience for local citizens. This paper considers the first anti-corruption inspection visits on the city level, exploiting inspections conducted by the *Provincial Commission for Discipline Inspection* (PCDI). A city ranks below provinces as the second-level administrative division in the country, which is the basic unit of residence and socioeconomic activity for ordinary citizens. As of 2013, China is officially divided into 333 city-level divisions. Below, I detail the organizational features of these inspection visits, and then discuss the rationale behind exploiting this layer of variation.

The PCDI, a province-level institution, supervises the state-sector members within its jurisdiction and is in theory independent of other governance operations (Xu, 2014).[11] Xi's anti-corruption program dictates that the PCDI dispatches inspection teams to city-level local governments. According to the *Rules for Inspection Work*, in each round of the visits, the list of selected regions is confidential before the PCDI is notified, with selected cities informed 10 days before the inspection. Members of inspection teams are appointed temporarily, avoiding potential conflicts of interest. Upon arrival in a city, inspection teams publicize their contact via local media (newspapers and television) and posters. Inspection teams are responsible for extracting information and potential evidence regarding local corruption; the cases will then be handled by the PCDI, which can generally result in investigations and arrests once confirmed. Specifically, inspection teams "audit records and documents from local Communist Party committees; participate in disciplinary meetings; gather and manage information provided by whistle-blowers and local residents; organize forums to collect public opinions on corruption issues; individually interview with state workers; [and] spot-check to obtain a better understanding of local corruption issues".[12] To date, the inspection visit institution remains the most primary instrument for anti-corruption in China and has even been adopted by other countries (e.g., Vietnam and Cambodia).

[10]Source: http://politics.people.com.cn/n1/2016/1020/c1001-28795090.html and https://m.ccdi.gov.cn/ content/98/f8/20269.html.

[11]The PCDI is led directly by the *Committee of Five*, which comprises a province's most powerful political leaders: Party Secretary, Chief Governor, Deputy Party Secretary, Head of the Organization Department, and Secretary of Discipline Commission.

[12]See https://www.wsj.com/articles/BL-CJB-28767 for details.

Local Anti-Corruption Visits as a Relevant shock. There are two major rationales for city-level visits serving as a relevant shock. First, the city is the basic unit of residence, socioeconomic activity, and social interaction for local citizens (Song, 2014; Dingel, Miscio and Davis, 2021; Chen, Gu and Zou, 2022). Second, in practice as a result of imperfect information and behavioral constraints, local inspection visits may raise awareness of the crackdown and further shift related perceptions. That is, even after the program's announcement in 2013, the earnestness and magnitude of the crackdown may not have been fully believed or appreciated by the public. In this framework, individuals' perceived corruption risks can become more salient after their cities experience an inspection visit (Bobonis, Cámara Fuertes and Schwabe, 2016; Avis, Ferraz and Finan, 2018; Zamboni and Litschig, 2018; Colonnelli and Prem, 2022). There are both conceptual and context-related arguments to buttress this perspective.

Firstly, given that the 2013 crackdown marked the first anti-corruption program of its kind since 1978, and it was initiated under new leadership, citizens might not have accurately known the costs and consequences of an inspection visit *ex-ante* — as shown by qualitative evidence presented later, even some local officials would underestimate them. Most anti-corruption initiatives that were launched prior to Xi's era have been proven to be temporary or superficial (Fang, 2017).

Secondly, given the considerable distance between high-stakes politics and the public in China, a top-down campaign takes time to be transmitted to local communities (Liu, 2020).[13] A popular anecdote in 2013 captures the public's lack of confidence that the crackdown would be substantive: "Big tigers are too distant from us; it is those annoying surrounding flies that bother every day; so let's wait and see."[14] This view aligns with other work in which agents put disproportionate weight on their own experiences in forming beliefs (Kleven et al., 2011; Malmendier and Nagel, 2011; Gallagher, 2014). Accordingly, individuals and households in my study, when inferring occupational returns and making career decisions, may rely more on their direct experiences and locally circulated information.[15]

Certainly, what I estimate is likely to be a lower bound of the total effects: cross-sectional shocks such as the dismissal of top-ranking officials and policy signals from the central gov-

[13]Different from most studies that concentrate on senior officials, I focus on entry-level state job applicants — typically fresh college graduates. According to the *National Academy of Governance*, the likelihood of climbing to managerial positions in the state sector is smaller than 4%, and over 90% of state workers will remain at the local level throughout their careers.

[14]Source: http://theory.people.com.cn/n/2014/0109/c40531-24068295.html.

[15]While investigations involving city and higher-ranked leaders may attract broader media coverage, a substantial portion of local inspection activities is conducted internally. For cases of front-line "flies", many investigations are disclosed within the corresponding state organizations, and related information is often disseminated informally through local social networks. Such cases constitute about 70% of punishment at the local level. See for example http://www.news.cn/legal/2023-01/13/c_1129280007.htm.

ernment — absorbed by time fixed effects — can all contribute to the public's perception; meanwhile, neighborhood shocks can likely generate spillover effects.

A large body of media accounts demonstrates that anti-corruption visits help curtail local corruption and change citizen perception.[16] For example, reports released by the Discipline Commissions and think tanks highlight both the unexpected harshness of anti-corruption visits and the decline of local corruption:

> "Many cadres viewed the inspection visit team as a scarecrow before the team arrived. However, the thorough inspection and rectification indeed deterred corruption." (*Central Commission for Discipline Inspection*, 2014)
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> "I was aware that some of my former colleagues had been detected, but perhaps they were just not careful enough. I thought I might be able to slip through the crackdown, yet I was wrong." (Zhenchao Yang, Former deputy governor of Anhui Province, in the documentary *Sharp Sword of Inspection*, 2017)
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> "A survey conducted by the Canton Public Opinion Research Center shows that more than 80% citizens perceive a lower need to engage in bribery and favor-exchange after the anti-corruption." (China Youth Daily, 2014)
>
> "I knew there was a high-profile crackdown going on, but frankly speaking, I still did not have a very clear idea in its early stage. However, after the father of a friend — someone who is extremely corrupt in our city — was investigated, I began to believe that there would indeed be major changes in our state sectors." (Interview of college students, Xiamen Daily, 2015)

The evidence is consistent with the findings of Lorentzen and Lu (2018) and Francois, Trebbi and Xiao (2023) that, apart from the intention of consolidating power, the anti-corruption crackdown, at least to some extent, features an attempt to reduce corruption. I later provide quantitative evidence that inspection visits affect citizens' anti-corruption perceptions. Finally, it is also important to note that the corruption crackdown may lead to changes in factors unrelated to corruption — such as broader economic conditions or career uncertainty — which I also discuss when exploring mechanisms.

[16]See also for example Qian and Wen (2015), Chen and Kung (2019), Chu, Kuang and Zhao (2019), Agarwal et al. (2020), Ding et al. (2020), Hao et al. (2020), Xu et al. (2021), and Fang (2023) for related quantitative accounts.

3 Data and Empirical Design

3.1 Data

The empirical analysis combines digitized anti-corruption visit records with multiple administrative and survey data. Here, I briefly describe the data to be used, with variables of interest detailed as they become relevant.

Staggered Anti-Corruption Visits. I conduct a large-scale data digitization to construct a city-level anti-corruption visit dataset for the period 2012 – 2017. The unit is the *city-year-month* spell. The timing of each province-to-city inspection visit is collected manually from local newspapers and authorized websites. I also elicit the number of investigated local senior officials from the website of each PCDI for sanity checks. **Figure 1** visualizes the timing of the first visit to each city, and the timing does not appear to be concentrated in cities with geographic proximity.

Applicant and Employee Data from State Organizations. To examine candidate quality, I utilize applicant data from 90 sampled state organizations in two provinces (2011 – 2017). One province is coastal and in Southern China, while the other is inland and in Northern China. This unique database is derived from a large human resource service platform, which provides screening and personality testing services for about 70% state enterprises and public institutions in the two provinces.[17] The 90 sampled organizations were randomly drawn by the data provider. In the Chinese setting, the majority of these state-sector applicants are fresh college graduates. Once recruited, they will start at entry-level positions on the ladder. All data used in my analysis are restricted to first-time, college graduate applicants to ensure comparability (97% of the total applicants).

The data offer two major advantages. First, while privacy regulations preclude access to full individual-level records, the system allows for aggregation at an ideal level for identification: for each organization, I can observe the mean applicant traits for each *college city* $\times$ *year* cell. The sample includes a total of 63 college cities. Second, applicant traits are derived from psychometric test results. Such psychometric measures, increasingly used by economists when examining non-cognitive skills, have been shown to be effective predictors of real outcomes such as public service and entrepreneurship (Perry and Hondeghem, 2008; Heckman, 2011; Klinger, Khwaja and Del Carpio, 2013; Deserranno, 2019; Ashraf et al., 2020; Callen et al., 2022).

[17] In the period under study, most state enterprises and about half of the public institutions in the provinces use psychometric testing systems in some form, which enables our data collection. The recruitment of civil servants for certain bureaucracies and other state institutions — approximately 28% of the total state workers — is conducted separately by national and local agencies (usually with more demanding processes) and is thus not covered in the sample.

Crucially, apart from cognitive and personality traits, the data contain an algorithmic integrity score. This score is part of an internal reliability analysis not included in the standard personality report. Rather than relying on direct self-reports of ethical behavior, the score is calculated by psychometric algorithms designed to assess an applicant's tendency to cheat throughout the entire test (Hart et al., 2023). Although the underlying algorithms are proprietary, the test provider offers demonstrations of the measure's nature and validity. The psychometric test is fast-paced and intense, typically requiring respondents to answer 100 – 240 randomly drawn multiple-choice questions (from frequently updated test banks) within 10 to 15 minutes, prompting instinctive responses. There are three key components: (1) lie-scale items are interspersed throughout the test, creating a web of cross-referencing questions; inconsistencies in responses are flagged, and raw scores are benchmarked against a representative norm group, with deviations serving as one component of integrity; (2) vignettes or "honesty game" style questions are incorporated when feasible; and (3) confounding factors (e.g., inattention and cognitive skills) are adjusted to ensure the score is comparable across individuals. A set of past sampled questions are presented in **Appendix C.2**. I also show supplementary evidence to better validate the psychometric integrity measure below.

In addition to this indirect measure, the test also uses a standard psychometric module to elicit public service motivation (Perry, 1996). The module comprises a series of statements and hypothetical situations in which respondents choose their actions. Studies have found that these scores are positively associated with integrity and ethical behavior (Perry and Hondeghem, 2008; Ashraf et al., 2020; Callen et al., 2022). I use this measure for robustness checks.

To corroborate the aggregate-level analysis and evaluate post-recruitment performance, I further obtain individual-level candidate data from five of the sampled organizations. For performance outcomes, I digitize evaluation reports of all 1,205 employees recruited between 2011 and 2017 by these five state organizations. Each report contains assessments of behavioral probity and overall effectiveness for the 2019 fiscal year, scored on a 4-point integer scale. These assessments are based on aggregated feedback from managers, colleagues, and performance appraisal systems. To obtain a more objective performance measure, I additionally draw on administrative records indicating whether an employee was issued a warning for corruption-related misconduct.

To the best of my knowledge, these rare non-experimental data represent one of the first instances enabling the evaluation of candidate quality in the Chinese context.

Validation of Psychometric Measures. To further validate the integrity-related psychometric measures, **Appendix C.3** presents a validation test in collaboration with the platform in their internal studies in 2025. Following the experimental economics literature (Fischbacher and Föllmi-Heusi, 2013), test-takers participate in a dice-rolling task online. Participants are instructed to roll a dice twelve times in private, with their monetary payoff determined solely by the number of sixes they reported. Since the actual rolls were unobserved, this setup ensures that while individual lying is undetectable, the incentive to cheat is salient. Consequently, an aggregate frequency of reported sixes exceeding the statistical probability provides a revealed-preference measure of dishonesty. I find that this behavioral dishonesty measure correlates strongly with the psychometric integrity and public service motivation scores, but is uncorrelated with cognitive ability, supporting the discriminant validity of the psychometric measures.

Survey Data on Occupational Preferences. To facilitate mechanism analysis, I use the College Student Job Outlook (CSJO) surveys conducted by one studied province. Since 2015, these surveys have been administered each spring by the provincial human resource center in collaboration with 34 local colleges in 21 cities. They target randomly selected third-year college undergraduates — one year before graduation in the Chinese education system — to understand job preferences and related perceptions. The survey includes an online psychometric test and a short questionnaire on job outlooks. With university cooperation, response rates are high, ranging from 93% to 97% across survey waves. Given that all cities were inspected by the end of 2017, I use the three survey waves conducted from 2015 to 2017. The final sample includes 7,823 students, with data on their personality traits, job preferences, and related perceptions. **Table A7** reports the correlations among major personality traits. Consistent with existing literature, integrity is positively correlated with public service motivation, while neither trait exhibits a strong correlation with cognitive ability (Hanna and Wang, 2017; Barfort et al., 2019; Gans-Morse, 2021).

To corroborate findings in a broader population, I supplement with data from the China Education Panel Survey (CEPS). The CEPS is a nationally representative, middle-school-based survey based on a stratified sampling design. It includes approximately 10,000 student households from 112 schools across 28 cities in mainland China. Each household is surveyed annually (during either the fall or spring semester), and I have access to four waves of data collected between 2013 and 2015. Crucially, the CEPS elicits occupational preferences in each wave: students report their ideal future occupation, while parents report the occupation they hope their child will pursue. To measure household preferences for state jobs, I construct a binary variable equal to one if both the student and their parents indicate a preference for state jobs. The first wave also includes pre-coded information on students' cognitive ability, personality traits, and demographic characteristics.

Additional Data Sources. The Chinese Social Survey (CSS) is also used in my analysis.

The CSS is a nationwide biennial cross-sectional survey project initiated by the Chinese Academy of Social Sciences. Each wave of the survey includes about 7,000 – 10,000 individuals across 125 sampled cities. I pool its 2011, 2013, 2015, and 2017 waves, which elicit perceived anti-corruption perceptions from respondents. In balance and robustness checks, I also incorporate variables on cities' socioeconomic characteristics, drawn directly from city statistical yearbooks and the 2010 Population Census.

3.2 Empirical Strategy

My empirical analysis uses variants of the following DiD specification:

$$y_{hst} = \beta * Inspected_{st} + \lambda_s + \tau_t + \varepsilon_{hst} \tag{1}$$

where y_{hst} is the outcome of interest; h indexes the unit of observation — individual, household, or city-year cell, depending on the data; s denotes the city of residence/origin; and t is time. $Inspected_{st}$ is the DiD term, which is 1 if the city has been visited by the anti-corruption inspection team; and β is the coefficient of interest that captures the average effect of anti-corruption visits. The time fixed effects, τ_t, absorb all common temporal shocks. The city fixed effects, λ_s, absorb time-invariant and slow-moving differences in city characteristics. Finally, I allow idiosyncratic error terms, ϵ_{hst}, to be correlated at the city level, corresponding to the level of treatment.

With staggered treatment timing, traditional two-way fixed effects regressions yield consistent estimates only under strong assumptions of homogeneous treatment effects (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Baker, Larcker and Wang, 2022; Borusyak, Jaravel and Spiess, 2024). To address this concern, I use the weighted stacked-by-event estimator by Wing, Freedman and Hollingsworth (2024), which is heterogeneity-robust and allows for higher-order interactions and flexible data structures (Cengiz et al., 2019; Deshpande and Li, 2019; Vannutelli, 2021).[18,19] I also show that my results remain similar when using other robust estimators.

The key identification assumption is the absence of differential trends between units

[18]Conceptually, the robust DiD estimator (1) uses only not-yet-treated units as clean controls to separately estimate impact for each round of treatment, and (2) aggregates these local DiD effects into an unbiased overall estimate. Wing, Freedman and Hollingsworth (2024) implement this approach by vertically stacking all treatment cohorts and their corresponding control units into one dataset, and running weighted regressions with fully saturated fixed effects. Corrected analytical weights are applied to address repeated use of control units.

[19]For transparency, I follow the default of Wing, Freedman and Hollingsworth (2024) for selecting control groups, where each treated unit is paired with all not-yet-treated observations when estimating local DiD effects. Other recent DiD estimators adopt different criteria for control selection, and my results are robust to these alternatives.

experiencing anti-corruption visits at different points in time. While the counterfactual parallel trend cannot be observed, I provide both qualitative and quantitative evidence to help validate my empirical design.

Timing of Treatment and City Characteristics. Table 1 examines whether cities treated at different times exhibit significant differences in a set of city characteristics in the pre-inspection period. Columns (1) – (4) display the average levels and growth rates of outcomes by treatment timing. To statistically test the balance, Columns (5) – (6) show the P-values of the unconditional and conditional F-tests for the difference in means across the five treatment cohorts, respectively. I do not find that any observable city characteristic consistently predicts the timing of anti-corruption visits, consistent with the finding of other studies utilizing the staggered inspection variation (e.g., Chen, 2023). Notably, the absence of a significant difference in pre-treatment characteristics is actually a stronger test, since the DiD only requires no counterfactual differential trends in the outcomes.

The balance corroborates the qualitative notion that the upper-level government seeks to make inspection visits unexpected, which can likely lead to an insignificant relationship between arrival timings and pre-treatment city characteristics.[20] Accordingly, local households and individuals are unlikely to fully anticipate or alter the exact timing of anti-corruption visits.

Timing of Treatment and Survey Time. As I also use student and household survey data, an additional issue to address is the orthogonality of the timing of the survey and the anti-corruption visit. Qualitatively, the exact timing of visits is not available to the public *ex-ante*, and the timing of each survey roll-out is determined several months earlier by the research team.

Quantitatively, because my analysis employs pooled cross-sectional data (CSS and CSJO) and treats the CEPS as unbalanced panels with finer points in time, I can explicitly test the compositional changes in individual or household characteristics. The exercise is essentially a placebo test, assuaging the possibility that my estimated effects are purely driven by changes in demographic features of respondents before and after a city's anti-corruption shock. Table 2 provides descriptive statistics for household/individual characteristics and their conditional differences. Holding constant the city and timing of the survey, I do not observe respondents interviewed after the anti-corruption visit to differ systematically from those interviewed before the visit.

Confounding Policy Shocks. A special concern about exploiting China's anti-corruption as a natural experiment is that the shock is accompanied by broader institutional changes in

[20] See for example: Xu (2014) and https://china.huanqiu.com/article/9CaKrnJJVoC.

Xi's era. An advantage of exploiting the variation generated by province-to-city inspection visits is that treatment occurs in a highly staggered and local way (as shown in **Figure 1**), and any other broader shock is absorbed by time fixed effects. Moreover, the inspection visit institution is highly independent of other policies and local governmental affairs (Xu, 2014). In the following section, I conduct sanity checks to empirically examine whether inspection visits truly alter perceived corruption returns.

3.3 Sanity Check: Perceived Corruption Returns

I begin by quantitatively checking the relevance of local anti-corruption visits.

First, given the primary goal of inspection visits is to detect and dampen corruption, I compare the number of local officials arrested before and after the shock. Because many lower-ranking officials are internally punished (thus not fully disclosed), I focus on city-level ranking officials, the powerful local seniors who were seldom investigated before Xi's campaign. **Figure A1** demonstrates a sharp jump in the number of officials investigated after the province-to-city anti-corruption visit, associating the treatment with greater corruption crackdown. **Figure A2** further presents the corresponding event study plot. The absence of pre-trends assuages the possibility that the anti-corruption visit is triggered by a trend in corruption scandals driven by unobservable factors in the first place.

However, as discussed, citizens might not necessarily perceive the inspection as an earnest effort to deter corruption if the crackdown is politically oriented and that inspection visits may focus attention on the broader message of reforms. In concept, while high-stakes crackdowns (e.g., center-to-province visits and investigations on sub-national leaders) may represent an intention to consolidate power (Lorentzen and Lu, 2018; Ru, 2021), province-to-city inspection visits are less prone to such concerns.

Empirically, I utilize the CSS pooled data, which feature a consistent perception measure in the "anti-corruption harshness" outcome variable, where 1 is the lowest and 4 the highest.[21] Columns (1) – (2) of **Table 3** present the DiD estimates, which associate inspection visits with heightened anti-corruption perception (around 17% standard deviation). The estimated coefficient remains virtually unchanged when adding a host of respondent demographic controls. Next, Columns (3) – (4) show the results of placebo tests, using visits from central government leaders since the beginning of Xi's term as the treatment. In the Chinese polity, it is common for new leaders to visit local municipalities to signal their reform agenda. None of my previous results are associated with these short-term visits, suggesting that the observed changes

[21]The corresponding survey question is "How do you rate the current anti-corruption (its strength on curbing corruption)?"

are unlikely to be driven by messages concerning broader institutional shifts or falsification (citizens simply say good about any political event).

Figure 2 visualizes the estimated coefficients using the event study specification. There is no evidence of differential perception pre-trends. In Table A1, I present more suggestive evidence about the relevance of local anti-corruption visits: individuals who consume more local media or are socially proximate to the state sector (with possibly more exposure and information) exhibit greater changes in anti-corruption perception.

4 Results

4.1 Candidate Quality for State Organizations

To study changes in the quality of state job candidates, I compare the traits of individuals applying to the same organization in the same year; and the DiD variation comes from anti-corruption visits in their college cities. Formally, I estimate the following augmented DiD specification:

$$y_{ost} = \beta * Inspected_{st} + \lambda_{os} + \tau_{ot} + \varepsilon_{ost} \tag{2}$$

where y_{ost} is the trait of applicants from city s, who applied to organization o in year t. λ_{os} holds the *city-of-college-attendance* $\times$ *organization* constant, capturing time-invariant differences in candidate quality from each city-of-origin to each state organization. Importantly, τ_{ot} holds the *recruitment year* $\times$ *organization* constant, capturing organization-specific temporal shocks (e.g, fluctuations in job openings). $Inspected_{st}$ is the DiD term that is 1 if the city of college attendance has been inspected, and β is the coefficient of interest that captures the average impact on applicant quality.

Impacts on Candidate Traits. Table 4 presents the results of organization-city-year level candidate analysis. Each outcome variable is the average trait score of applicants from a city-year cell for a given organization. Column (1) finds that individuals applying for state positions after their college cities have been inspected score, on average, 0.758 standard deviations higher for their integrity, as assessed by psychometric tests.[22] Column (2) assesses the robustness by adding city-specific trends, allowing applicants from each city to have their own linear cohort trend. Following the approach in Kahn-Lang and Lang (2020), I estimate these city-specific trends using only pre-treatment observations, detrend the dependent variable

[22]All effective treatment cohorts in my sample entered college prior to 2013 (graduated prior to 2016) before the anti-corruption campaign began. Therefore, endogenous selection into college cities is not a threat.

using the estimated linear trends, and then re-estimate the main specification. The estimate remains virtually unchanged, suggesting that the result is unlikely to be driven by pre-existing differential trends.

Turning to cognitive ability, Columns (3) – (4) find that anti-corruption shocks do not yield statistically significant changes in the average cognitive competence of these state-job applicants, and the magnitudes are small compared to those on integrity changes. These combined results suggest that there is no necessary trade-off between integrity and competence in the changing composition of the candidate pool.

The results remain robust when using alternative estimators and inference criteria. **Table A2** reports results adopting robust DiD estimators from De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), Callaway and Sant'Anna (2021), and Borusyak, Jaravel and Spiess (2024). **Table A3** reports standard errors clustered at the organization level and at the two-way level (organization and city), as well as P-values computed using wild-bootstrap methods. To bolster the pre-treatment comparability, **Figure 3** further presents event study plots. Notably, the sharp jump in applicant integrity immediately following the treatment suggests that, while other gradual mechanisms may be at play, talent reallocation (i.e., sorting by type) is a likely driver in the short run — a point I return to later.

Table A4 examines heterogeneity by anti-corruption intensity, serving as an additional check on the relevance of the treatment. While the DiD estimates capture the average effect, students from regions facing stronger enforcement may exhibit a more pronounced treatment effect. To proxy anti-corruption intensity, a city is classified as "high-intensity" if the number of its investigated senior officials (city-ranked or above) per post-crackdown year exceeds the mean 0.506. Consistent with the hypothesis, the treatment effect for applicants from cities classified as having higher intensity is, on average, twice as large.

The data also provide a set of additional psychometric traits, which are major dimensions used by most employers. As noted, the psychometric test constructs a direct measure related to one's integrity and prosociality — labeled as "public service motivation" entry — based on the widely-used Perry's public service psychometric module (Perry, 1996). This serves as a valuable validation metric. **Figure A3** displays the estimated effects on all major dimensions of candidate traits. There is a consistent increase in public service motivation scores among candidates from treated cities. Coefficients on other dimensions — such as emotional maturity and risk attitudes — remain relatively small, suggesting no substantial overall shifts in those candidate traits.

To better complement the organization-city-year cell analysis, I obtain individual-level applicant data from five of the sampled organizations. Due to strict confidentiality constraints, very few organizations were able to provide access to individual candidate records. Nevertheless, the scope of these data remains unique in the studied context. Such data enable me to control for individual characteristics and, crucially, to link psychometric traits to subsequent workplace performance. Columns (1) – (2) of **Table 5** replicate the baseline specification at the individual level, controlling for age, gender, and ethnicity. The empirical patterns remain consistent with the aggregate findings.

Further Results on Post-Recruitment Performance. To further study the relevance of candidate pool changes, I turn to subsequent post-recruitment performance. To this end, I re-estimate the empirical model using digitized data on candidates recruited between 2011 and 2017 by the five state organizations examined above. The outcome variables are individual-level performance measures for the fiscal year 2019. Comparing annual performance in the same year within each organization ensures a uniform standard of evaluation that does not depend on past selection margins.

Columns (3) – (5) of **Table 5** presents the results. An advantage of this setting is that, in addition to standard overall ratings (1, poor – 4, excellent), Chinese state enterprises explicitly evaluate candidates on "probity and discipline", reflecting the Communist Party's stated emphasis on behavioral ethics. To complement with a more objective measure, I also use administrative records to identify employees who were warned for corruption-related misconduct. Column (3) shows a significant improvement in the probity rating for employees exposed to the anti-corruption shock before their application. In Column (4), I turn to the standard rating "overall effectiveness", which likely captures the combined output of ability and integrity. The estimate for this outcome is also positive. Finally, Column (5) supplements a behavioral outcome, where the dependent variable is an indicator for whether the individual was ever warned for misconduct by 2019. While the estimate is less precise, the coefficient's direction is consistent with the change found in the "probity and discipline" rating.

It is worth noting that the improved workplace performance may not be solely attributed to shifts in applicant quality, but may also be compounded by context-specific screening processes of the state sector (e.g., Ashraf et al., 2020). Since organization-year–level temporary shocks (such as changes in an organization's overall recruitment criteria) are absorbed in my identification, there are two other remaining layers that could also contribute to post-recruitment performance. First, one may wonder if applicants from inspected cities are screened differently. Second, there could also be an incentive effect — even holding worker type constant, recruits from treated cities might exhibit different behavior once hired.

Although the aforementioned factors may be present, **Table A5** provides suggestive evidence that the candidate quality channel (i.e., changes in candidate quality translating into performance outcomes) is likely at play. First, I examine the extent to which integrity and ability matter for selection, and whether candidates from inspected and non-inspected cities are selected differently. Columns (1) – (2) of **Table A5** find that the state sector places strong weight on candidates' ability. This emphasis on ability is in line with long-standing literature on the Chinese bureaucracy (Bell, 2016; Yao, 2018). Moreover, tests of coefficient equality do not reject the null hypothesis that these patterns are the same for treated and non-treated applicants. This suggests that recruiters do not apply noticeably different criteria based on candidates' anti-corruption exposure.[23]

Second, among recruited candidates, I examine whether psychometric trait measures predict subsequent workplace performance. Columns (3) – (4) of **Table A5** show that, for workplace "probity and discipline", pre-entry integrity scores exhibit significant predictive power, whereas cognitive ability appears less relevant. Turning to "overall effectiveness" in Columns (5) – (6), individuals with higher pre-entry cognitive and integrity scores demonstrate stronger performance ratings, with both traits showing significant predictive power. Comparing results across these columns, I further document that the trait-performance association does not significantly differ between recruits from inspected cities *versus* non-inspected cities. Taken together, these patterns imply that the quality of the candidate pool matters for organizational performance; furthermore, they reinforce the predictive validity of the psychometric measures to meaningfully capture individual traits.

Collectively, these findings provide logical support for the relevance of the candidate pool, which can alter the composition of recruits, thereby contributing to state-sector performance.

4.2 Mechanisms

As established in the sanity check, anti-corruption visits have significantly altered citizen perceptions thereof (**Table 3**). Therefore, I begin by exploring two major channels related to corruption returns.

First, the sharp short-term changes in candidate quality could be consistent with a Roy-style model of talent allocation, where individuals self-select into sectors based on their traits in response to the decreased rewards of corruption. In theory, corruption opportunities provide additional scope for pecuniary and illicit returns from state jobs, which may be particularly significant for high-ability individuals; however, such opportunities also impose a disutility on honest and prosocial individuals. **Appendix B** presents related theoretical discussions. Intuitively, while the net effect of an anti-corruption shock on applicant composition is a *priori*

[23] Qualitatively, there are also no administrative rules differentiating applicants based on the inspection status of their city of origin. While residency restrictions exist for certain state positions (often for security or administrative reasons), these do not apply to the fresh college graduates in my sample.

ambiguous, under regular conditions the shock is likely to improve average candidate integrity. The effect on average ability, by contrast, remains largely indeterminate.

Second, an alternative mechanism — conceptually more relevant in the longer run — involves a dynamic shift in population traits. If the post-crackdown population becomes intrinsically more honest and prosocial, the integrity of state job candidates would rise mechanically.

Trait Changes. To empirically explore these mechanisms, I use the CSJO pooled cross-sectional data to examine changes in college students' psychometric traits and occupational preferences. I begin with a DiD specification to assess whether there are notable short-run shifts in population traits:

$$y_i = \beta * Inspected_{st} + \lambda_s + \tau_t + \varepsilon_i \tag{3}$$

where i indexes the student, s the city of college attendance, and t the survey year. $Inspected_{st}$ is the DiD term defined as before.

As shown in **Table 6**, during the sample period (2015 – 2017), college students' integrity and public service motivation scores do not exhibit significant changes in response to anti-corruption shocks. This null result is particularly informative: under the reasonable logic that the crackdown would unlikely encourage lower integrity, any strategic adaptation or norm adoption (especially among those aspiring to state jobs) should mechanically yield a positive estimate for β. The absence of such a positive shift suggests that short-run changes in population traits are unlikely to be the main driver. Other major personality traits, such as risk attitudes and emotional maturity, also remain stable across cohorts.[24]

Talent Reallocation (Sorting by Type). To study the allocational mechanism, I next interact the DiD term with the measure of student traits (which have been shown to remain relatively stable above). This leads to the following triple-difference design:

$$\begin{aligned} y_i = \beta_0 * Inspected_{st} &+ \beta_{\mathbf{1}} * Inspected_{st} * Trait_i \\ &+ \sum_s \lambda_s * Trait_i + \sum_t \tau_t * Trait_i + \lambda_s + \tau_t + \varepsilon_i \end{aligned} \tag{4}$$

where β_1 is the triple-difference coefficient of interest, capturing heterogeneous changes in state job preferences by student trait.[25] $Trait_i$ represents a proxy for the student trait (e.g.,

[24] Both the psychology and economics literature documents that adult cognitive ability is highly persistent, and that non-cognitive traits evolve only slowly after adulthood (Roberts and DelVecchio, 2000; Almlund et al., 2011). This evidence may help explain why ability and integrity traits in the near-term population remain relatively stable.

[25] A student is coded as preferring state jobs if their first-choice ideal employer is a government agency, public institution, or state-owned enterprise (public service). See Q4 of **Appendix C.4** for the survey question.

integrity). The full-saturated lower interaction terms, $\sum_s \lambda_s * Trait_i$ and $\sum_t \tau_t * Trait_i$, remove trait-specific city differences and time trends unrelated to anti-corruption. The remaining variables are defined as before.

Table 7 reports the allocational-margin results. Column (1) shows that the estimated coefficient is statistically indistinguishable from zero, suggesting no significant change in the average preference for state jobs. Column (2) allows the impact to vary according to student integrity z-scores. Experiencing anti-corruption visits is associated with a significant increase in the likelihood that individuals with higher integrity scores prefer state-sector employment. This suggests a pattern of differential sorting, where curbing corruption increases the relative appeal of state jobs for high-integrity applicants compared to those with lower integrity scores.[26]

Column (3) of **Table 7** further controls for selection based on other individual traits. To better clarify the nature of these controls, **Table A7** reports the correlations among these personality traits. As highlighted, the integrity metric is strongly and positively correlated with public service motivation, yet neither trait shows a significant correlation with cognitive ability. In Column (3), the coefficient on integrity remains statistically significant, though its magnitude is partially absorbed by public service motivation — an expected result given their positive covariance. Notably, controlling for cognitive ability does not materially alter the main effect. This speaks to the argument in Hanna and Wang (2017) that screening based on ability alone is insufficient to prevent rent-seekers from entering public service. Column (4) further add controls for student-level characteristics, and the results remain robust.

Finally, to bolster the allocational mechanism in a broader sample, **Table 8** uses the CEPS student-parent panel, which provides a nearly nationally representative sample of middle schoolers' households. This panel allows me to hold constant students' *ex-ante* traits and compare their households' occupational preferences before and after their city experienced an anti-corruption visit.[27] Relying on *ex-ante* trait measures further mitigates the concern that individuals desiring state jobs might strategically misreport or endogenously alter their traits. Consistent with the college student survey findings, **Table 8** shows that households with more prosocial children exhibit a stronger preference for state jobs following an anti-corruption visit, and this pattern remains robust to controlling for heterogeneity in cognitive ability.

Together, these two sets of results — combined with the rise in citizens' anti-corruption

[26]**Table A6** corroborates this point further by using an alternative approach: dividing students into (relatively) high- and low-integrity groups, with the mean integrity score as the cutoff. The results show an overall increase in state job preferences among the "high-integrity" group, and a negative coefficient for the "low-integrity" group. However, as personality traits are more meaningful in relative terms, I do not further interpret the magnitude of these estimates.

[27]The dataset includes pre-constructed measures of students' cognitive ability, integrity and prosociality, as well as family socioeconomic status. For data details, see **Appendix C.5**.

perception — point to talent reallocation due to changes in corruption rewards as one logical mechanism. Below, I discuss other explanations unrelated to corruption returns.

Career Risks. The top-down corruption crackdown may likely yield greater career uncertainty for state jobs on two fronts: (1) some informal practices common in state jobs that are hard to be clearly justified may associate with the uncertainty of punishment, along with a fear of purges (Wang, 2021); (2) meanwhile, the removal of investigated officials can also aggravate the uncertainty of a locality or department. This raises the question of whether the main effects instead capture selection based on applicants' risk-taking.

Theoretically, new entrants are less exposed to the purge risks brought about by the anti-corruption campaign than senior officials, who are not the focus of this study. Empirically, while career stability matter, there is no necessary strong correlation between risk-taking and integrity/prosociality or ability (as shown in **Table A7**). **Table 7** presents a more direct test: the magnitude of sorting on integrity and public service motivation remains largely unchanged after accounting for selection based on risk appetite. These findings suggest that career risks may not predominantly explain the main effects.

Private-sector and Socioeconomic Conditions. A number of studies have documented mixed down-stream impacts of anti-corruption programs on firm-level and private-sector outcomes (e.g., Xu and Yano, 2017; Chen and Kung, 2019; Ding et al., 2020; Kong and Qin, 2021; Colonnelli and Prem, 2022; Fang et al., 2022). One may thus wonder whether these socioeconomic conditions — which can influence the perceived pecuniary payoffs of state versus private jobs — confound the observed selection pattern.

I conduct two related sets of empirical exercises. First, **Table A9** uses a horse-race approach and find that my core results remain robust to a host of time-variant city controls, including local GDP, population, fiscal revenue, fiscal expenditure, and the relative average wages in the public and private sectors, as well as the interaction terms between baseline characteristics (only available in 2010) — the distance to the coast, average years of schooling, the share of public workers, and the share of urban residents — with the full vector of time fixed effects (Martinez-Bravo et al., 2022; Ang et al., 2025). Second, I use the CEPS household panel, which allows me to directly control for heterogeneous reactions by household socioeconomic status. If economic considerations overwhelm other channels, we would expect that accounting for this heterogeneity would substantially attenuate the estimated effects by integrity and ability. However, as shown in **Table 8** (Columns 3 and 6), the magnitudes of sorting by integrity and ability remain largely unchanged when including an interaction between anti-corruption visits and household socioeconomic background ($Inspected \times Socioeconomic\ index$).

It is worth noting that these results do not contradict the potential role of career risks and economic payoffs in job selection; rather, they only suggest that such factors may not significantly confound the observed differential effects by trait.

Chances of Attaining Jobs. The above analyses have focused on factors affecting expected job utility. For sophisticated applicants, a nuanced consideration beyond is the perceived likelihood of being hired. On one hand, anti-corruption signals may raise honest individuals' perceived chances of securing state jobs. On the other hand, the highly competitive nature of job seeking in China may dampen this effect. **Table A8** explores this channel by examining changes in students' perceived likelihood of attaining their preferred jobs, based on a survey question from the CSJO data.[28] The interaction term of interest allows the anti-corruption impact to vary with individual integrity scores. The estimated coefficient is not statistically different from zero, suggesting that changes in perceived job attainment likelihood do not constitute a dominant channel in this context.

4.3 Further Discussion

Lastly, this section presents two sets of additional results for further implications. The first set explores more about spillover effects by accounting for neighborhood shocks. The second discusses intergenerational changes in household talent investment that may shape human capacity across sectors in the long run.

Neighboring Spillover Effects. While not a threat to identifying the sign of causality, understanding the spillover impact of local anti-corruption shocks is relevant, as it contributes to the self-reinforcing nature of corruption and related policy debates. To better position my results into the existing literature, I consider spillovers arising from geographical proximity. To quantitatively assess the role of neighboring spillover effects, I follow the design of Colonnelli and Prem (2022) by excluding control units that are potentially subject to treatment spillovers. Specifically, I re-estimate the DiD after removing all control cities that ever neighbored an inspected one. The new estimates will therefore be relative to a group of cities that are plausibly neither directly nor indirectly affected, and a large difference in the magnitudes of estimated impacts would suggest a salient presence of neighboring spillovers.

The even columns of **Table A10** present the new estimates for the main candidate sample. As expected, the effects of anti-corruption visits when excluding spillover controls tend to be stronger. The corresponding spillover effects are about 18% of the baseline magnitudes.[29]

[28] See Q5 of **Appendix C.4** for the survey question.

[29] For reference, this is roughly one-third the size of those estimated by Colonnelli and Prem (2022) (about 50%), who study firm outcomes in response to local anti-corruption audits in Brazil.

This suggests a moderate yet discernible role of neighboring anti-corruption shocks, consistent with the contagious nature of corruption (Gulino and Masera, 2022). Given that I focus on psychometric traits rather than monetary outcomes, I refrain from over-interpreting these magnitudes. Nevertheless, these indirect compounding effects can be relevant when evaluating the aggregate benefits and costs of anti-corruption programs.

Dynamic Effects in the Longer Run. The previous analyses suggest population traits are relatively static in the short run. Yet, in the longer run, the allocational effect may be compounded by dynamic shifts in citizen traits: corrupt societies may discourage investment in productive human capital and integrity (e.g, Murphy, Shleifer and Vishny, 1993; Tirole, 1996; Ehrlich and Lui, 1999), resulting in an overall undesirable talent pool, which can, in turn, aggravate the negative selection into the state sector further.[30] This section presents speculative evidence on the dynamic impact by exploring household intergenerational talent investment. I use the CEPS student-parent panel, which contains a set of questions to elicit parental attention allocated to their children across four broad dimensions. Each dependent variable is a binary that is 1 if the parent reports they pay particular attention to the corresponding domain.

Figure A4 visualizes the results. The anti-corruption shock is associated with a greater probability of allocating attention to children's academic performance (Row 1) and behavior/wrongdoing (Row 2). These empirical patterns are consistent with greater inputs in the quality of human capital, plausibly featuring a greater intention of accumulating productivity and integrity in response to a lower reward of corruption. While not statistically significant, there is a modest decline in parental emphasis on children's peers and networks (Row 3). This result should be interpreted with caution: it may reflect a diminished value of social networks, a key nexus for rent-seeking in China (Ang et al., 2025); alternatively, it may just indicate a reallocation of parental focus due to limited attentional resources. Row 4 shows that changes in household attention to children's appearance and dress — items less related to anti-corruption — are close to zero. Finally, the figure also presents estimated effects based on students' perceptions of parental attention, elicited separately. The results are consistent with those based on parents' own reports.

It is worth noting that such human capital investment (especially when concerning personality traits) often requires sustained inputs and can take several years before materially affecting behavior or labor market performance (Cunha and Heckman, 2007; Almlund et al.,

[30] Households can have less incentive to invest in productive talent traits when corruption is rampant (Pecorino, 1992; Murphy, Shleifer and Vishny, 1993; Gulino and Masera, 2022). Similarly, when a generation is known to be corrupt and thus "being corrupt" is a useful trait, this can change the desire of future generations to bear integrity (Tirole, 1996). Such traits developed during formative years can significantly shape long-run behavior (Cruces, Rossi and Schargrodsky, 2023).

2011). I therefore regard the evidence here as more heuristic.

5 Concluding Remarks

This paper provides evidence that anti-corruption shocks significantly reshape the quality of candidate pools for the state sector. The Chinese context offers an ideal setting for empirical analysis, as rent-seeking remains prevalent in its state sector, and staggered anti-corruption visits generate relevant local variation in heightened corruption risks. Leveraging unique applicant data from state organizations, I document positive selection on integrity and prosociality into state jobs following the crackdown, without compromising the overall ability of the candidate pool. Moreover, such changes in candidate quality likely translate into improved workplace outcomes. While other mechanisms may be at play, evidence from multiple survey datasets identifies talent reallocation in response to decreased corruption rewards as one important driver.

A key takeaway is that the impact of corruption on the human capacity and effectiveness of the state can, in part, be attributed to the endogenous supply of talent. Of particular relevance, cross-sectional studies observe that high-corruption countries attract rent-seekers into their state sector, while low-corruption countries attract more honest and prosocial individuals. By leveraging within-society anti-corruption variation and unique data from multiple sources, my results bridge these prior findings on self-selection (Hanna and Wang, 2017; Barfort et al., 2019; Gans-Morse, 2021; Cruces, Rossi and Schargrodsky, 2023) and provide empirical evidence echoing the more general theories on reward structures and talent allocation.

There are also several avenues for future research. First, this paper focuses on identifying candidate-side responses to the corruption crackdown, with a focus on causal direction more than magnitudes. However, the relative importance of candidate-side shifts *versus* incumbent-side shifts (e.g, Bobonis, Cámara Fuertes and Schwabe, 2016; Avis, Ferraz and Finan, 2018; Zamboni and Litschig, 2018; Weaver, 2021) for overall state-sector performance remains to be understood, which bears important policy implications. Second, there is also relatively little work on how corruption affects the flow of talent within the state sector. For instance, what happens when more honest and prosocial new entrants ascend their career ladder and are exposed to existing (and perhaps more corrupt) officials and politics? Last but not least, field experiments (e.g., informational treatments) connecting corruption and household behavior have the potential to identify more specific mechanisms through which corruption shapes talent formation.

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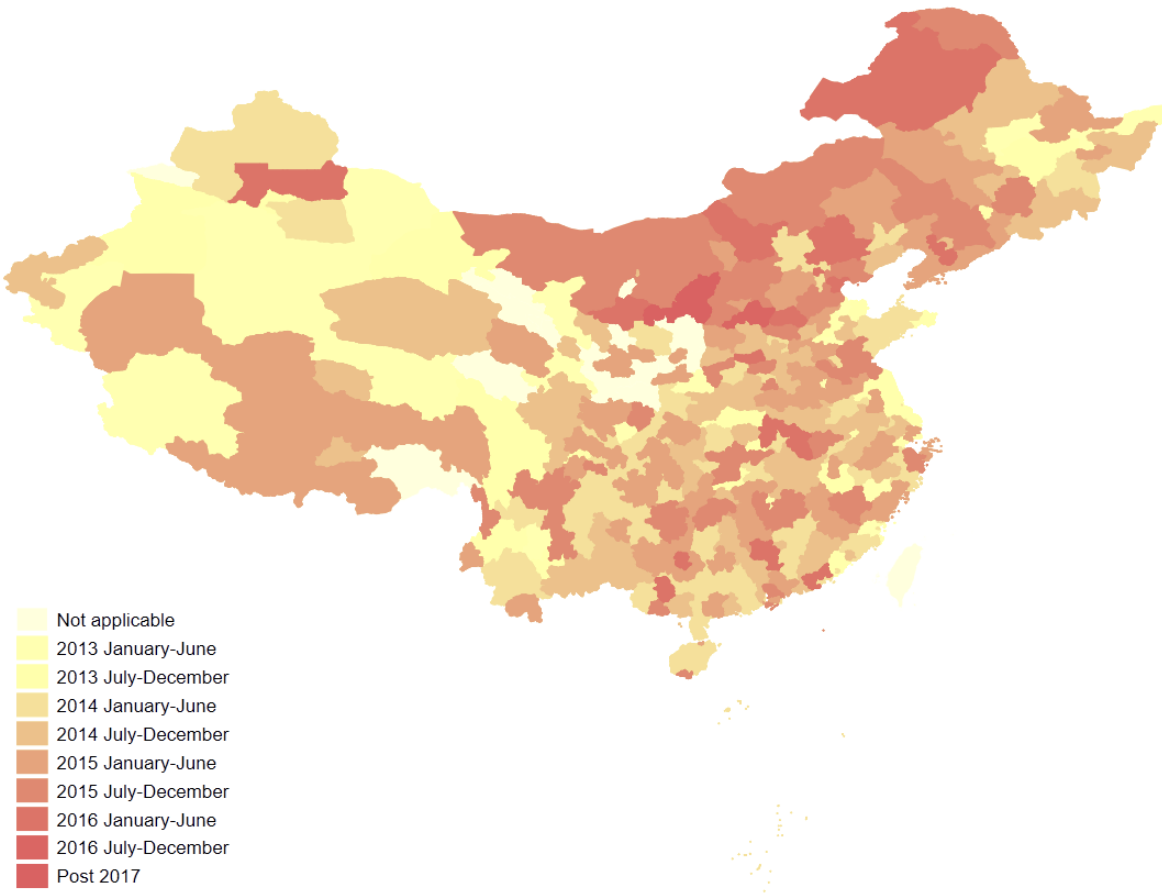
Figures

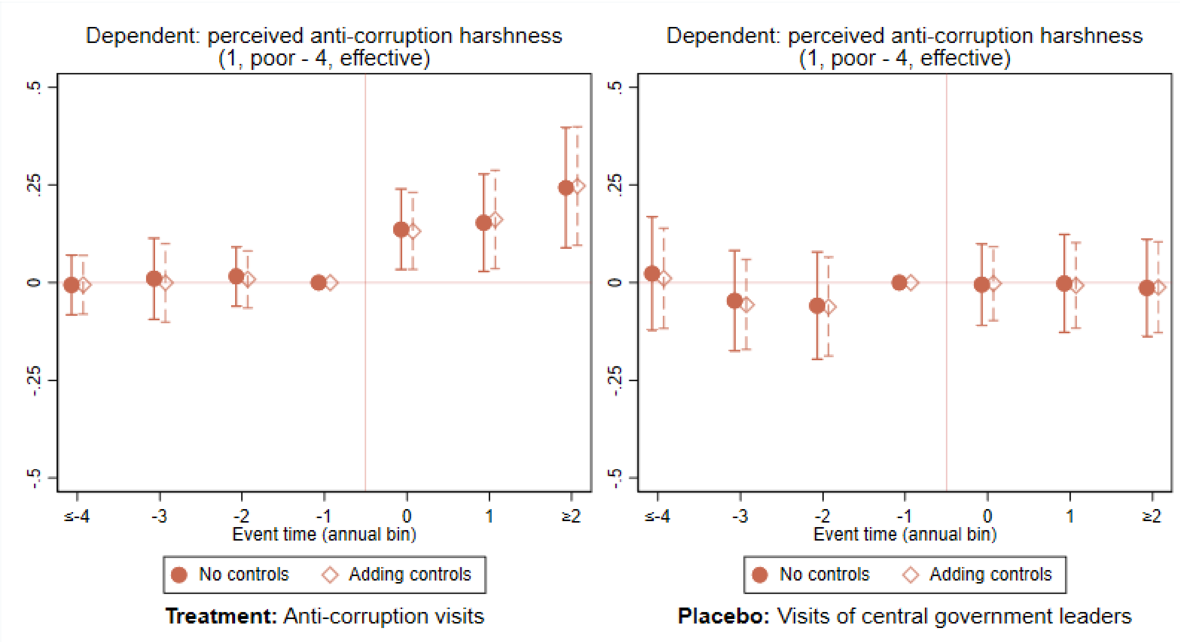
Figure 1: Timing of the first anti-corruption visit (2013 – 2017)



Notes: The map shows the timing of the first province-to-city inspection visit in China's anti-corruption crackdown since 2013. All cities have been inspected by the first half of 2017.

[Back to Page 11]

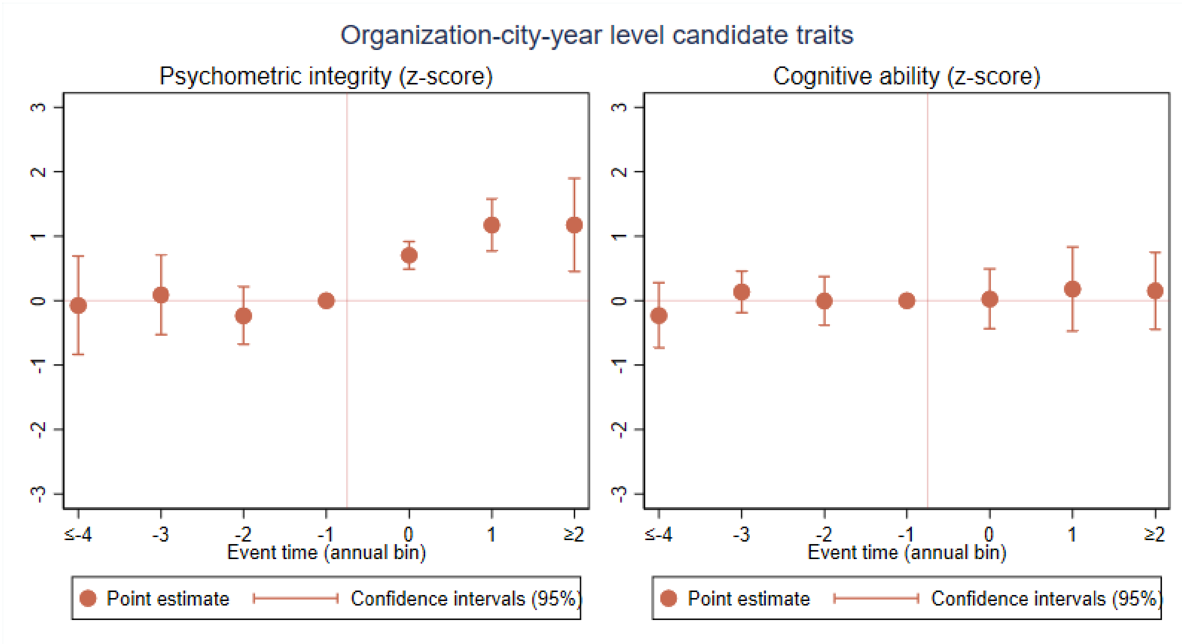
Figure 2: Anti-corruption visits and citizen perception



Notes: The left panel shows the dynamic effects of anti-corruption visits, and the right panel shows the placebo test using short-term visits from central government leaders. The specification corresponds to that of **Table 3** but allows the effect to vary by each event-time period. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). The 95% confidence intervals are reported using standard errors clustered at the city level. The unit of observation is the individual. Data source: Chinese Social Survey (pooled cross-section, 2011 - 2017).

[Back to Page 17]

Figure 3: Changes in candidate traits for state jobs



Notes: The figure shows the dynamic effects of anti-corruption on candidate traits for the 90 sampled state organizations. The unit of observation is the organization-city-year. Sample period: 2011 – 2017. The specification corresponds to that of **Table 4** but allows the estimated effect to vary by each event-time period. For example, "+1" refers to applicants from cities that experienced anti-corruption visits one year ago, compared to applicants from cities one year prior to anti-corruption visits ("-1" as the baseline period), holding the *recruitment year* × *organization* and the *city of origin* × *organization* constant. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). The 95% confidence intervals are reported using standard errors clustered at the city level.

[Back to Page 18]

Tables

Table 1: Summary statistics and balance check – city characteristics

	Treatment timing (cohort)				Uncond. F-test (P-value)	Cond. F-test (P-value)
	2013	2014	2015	2016		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Level (2012)						
Log. Distance to the coast (km)	5.259	6.015	5.701	5.963	0.357	0.814
Log. Local GDP	16.231	15.999	16.208	15.881	0.247	0.725
Log. Population (10 thousand)	5.716	5.753	5.758	5.425	0.176	0.474
Log. Fiscal expenditure	13.511	13.546	13.495	13.451	0.972	0.569
Log. Fiscal revenue	12.953	12.903	12.874	12.853	0.600	0.439
Log. Public education spending	9.109	9.128	9.106	8.939	0.972	0.370
Share of urban residents (2010)	0.496	0.442	0.478	0.465	0.562	0.444
Share of public workers (2010)	0.115	0.111	0.121	0.111	0.338	0.627
Average years of schooling (2010)	8.825	8.591	8.587	8.628	0.698	0.319
Panel B: Growth rate (2012)						
Local GDP	0.112	0.119	0.119	0.124	0.325	0.235
Population	0.051	0.056	0.039	0.042	0.193	0.843
Fiscal expenditure	0.212	0.216	0.181	0.224	0.430	0.539
Fiscal revenue	0.260	0.261	0.202	0.226	0.212	0.245
Public education spending	0.313	0.364	0.377	0.405	0.723	0.629

Notes: The table shows summary statistics of characteristics of cities for which the first anti-corruption inspection visit starts, respectively, at different time points. The sample comprises all cities in Mainland China. All monetary-related terms are measured in 10,000 CNY. Column (5) displays the P-value of unconditional F-Tests for equality of means across cohorts. For each characteristic, the F-test is obtained by an OLS regression on a set of indicators for the different cohorts and then testing the equality of estimated coefficients. Column (6) displays the P-value of conditional F-tests, by further controlling the province fixed effects. The statistics about average years of schooling, shares of urban residents, and shares of public workers are only available from the 2010 Population Census. Data source: the 2010 Population Census and the National Bureau of Statistics of China.

[Back to Page 15]

Table 2: Summary statistics and balance checks – survey respondents

CEPS (2013 - 2015)			CSJO (2015 - 2017)			CSS (2011 - 2017)		
	(1)	(2)		(3)	(4)		(5)	(6)
Variable	Mean	Diff-in-diff	Variable	Mean	Diff-in-diff	Variable	Mean	Diff-in-diff
Female	0.473	0.001	Female	0.461	0.002	Female	0.454	0.009
	(0.499)	(0.006)		(0.486)	(0.008)		(0.498)	(0.015)
Rural household	0.527	-0.029	Rural resident	0.245	0.003	Rural resident	0.450	-0.018
	(0.499)	(0.022)		(0.407)	(0.008)		(0.497)	(0.023)
Only child	0.616	0.012	Minority	0.068	0.009	Family size	4.276	-0.124
	(0.486)	(0.013)		(0.215)	(0.030)		(1.897)	(0.097)
Household SES (1 – 5)	2.807	-0.010	Age	21.312	0.153	Age	46.702	0.455
	(0.607)	(0.011)		(1.015)	(0.366)		(14.009)	(0.492)
Father: CCP member	0.124	0.011	CCP member	0.208	-0.005	CCP member	0.103	0.009
	(0.330)	(0.008)		(0.353)	(0.011)		(0.303)	(0.008)
Father: $\geq$ high school	0.398	0.013	Father: $\geq$ high school	0.516	0.027	$\geq$ High school	0.304	0.014
	(0.489)	(0.013)		(0.499)	(0.054)		(0.460)	(0.016)
Mother: CCP member	0.057	-0.003	Mother: $\geq$ high school	0.463	0.051			
	(0.231)	(0.004)		(0.474)	(0.060)			
Mother: $\geq$ high school	0.360	0.012						
	(0.480)	(0.013)						
Unit of observation	Schooler-Year		Unit of observation	Student		Unit of observation	Individual	
Observations	20,558		Observations	7,823		Observations	27,485	

Notes: The table shows summary statistics (mean and standard deviation) of respondents' demographic characteristics in each survey. "CEPS " refers to the China Education Panel Survey; "CSJO" refers to the College Student Job Outlook Survey; "CSS " refers to the Chinese Social Survey . "*Diff-in-diff* " is the estimated coefficient of "*Inspected*" conditional on the city and time fixed effects, in which "*Inspected*" is a dummy that is 1 if the city of residence has been inspected by an anti-corruption visit team. In Columns (2), (4) and (6), standard errors clustered at the city level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 15]

Table 3: Anti-corruption visits and citizen perception

	Perceived anti-corruption harshness (1, poor – 4, effective)			
	Sanity check		Placebo test	
	(1)	(2)	(3)	(4)
Mean of dep. var	2.875	2.877	2.875	2.877
Inspected	0.153**	0.158**		
	(0.066)	(0.065)		
Placebo: Visited by center government leaders			0.029	0.033
			(0.038)	(0.039)
Survey year FEs	Y	Y	Y	Y
City FEs	Y	Y	Y	Y
Individual controls	-	Y	-	Y
Observations	17,870	17,870	17,870	17,870
Data source	Chinese Social Survey (2011 – 2017, pooled)			

Notes: Unit of observation is the individual. "*Inspected*" is a dummy that is 1 if the city of residence has been inspected by an anti-corruption visit team. "*Visited by center government leaders*" is a dummy that is 1 if the city of residence has been visited by central government leaders since Xi's term. Individual controls include fixed effects for gender, age, family size, educational attainment, rural residence, and Party membership. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 17] [Back to Page 20]

Table 4: Anti-corruption and candidate traits for state jobs

Org-city-year level analysis (90 sampled orgs., 2011 – 2017)	Psychometric integrity z-score		Cognitive ability z-score	
	(1)	(2)	(3)	(4)
Applying after college city inspected	0.758**	0.703**	0.037	0.014
	(0.291)	(0.259)	(0.181)	(0.169)
Recruitment year × Organization FEs	Y	Y	Y	Y
College city × Organization FEs	Y	Y	Y	Y
College city specific trends	-	Y	-	Y
Observations	8,379	8,379	8,379	8,379

Notes: Unit of observation is the organization-city-year. Each outcome variable represents the average trait score of applicants from a given city-year cell for a particular state organization, based on their psychometric test results. "*Applying after college city inspected*" is a dummy equal to 1 for individuals who applied to state jobs after their college city underwent an anti-corruption visit. The empirical strategy compares the traits of applicants for the same organization in the same year, and the DiD variation comes from anti-corruption visits in their college cities. Columns (2) and (4) add city-specific trends to allow for a differential linear cohort trend in each college city, following the spirit of Kahn-Lang and Lang (2020): only using pre-treatment observations to estimate city-specific trends, and then de-trending the dependent variable around the estimated linear trend to obtain the "detrended" outcome and repeating the regression as before. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). The sample covers 90 large-scale state organizations in two provinces. Standard errors are clustered at the city level (63 clusters). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 17]

Table 5: Complementary results using individual-level data

Individual level analysis (5 sampled orgs., 2011 – 2017)	**Candidate traits**		**Workplace performance**		
	Psychometric integrity z-score	Cognitive ability z-score	Rating: "Probity & discipline" (1, poor – 4, excellent)	Rating: "Overall effectiveness" (1, poor – 4, excellent)	Warned for misconduct (binary)
	(1)	(2)	(3)	(4)	(5)
Applying after college city inspected	0.887***	0.055	0.603***	0.483***	-0.006*
	(0.148)	(0.100)	(0.220)	(0.150)	(0.003)
Sample	All applying candidates		Recruited candidates		
Individual controls	Y	Y	Y	Y	Y
Recruitment year × Organization FEs	Y	Y	Y	Y	Y
College city × Organization FEs	Y	Y	Y	Y	Y
Observations	37,251	37,251	1,293	1,293	1,293

Notes: Unit of observation is the individual. The sample includes five state organizations for which individual-level data are available. In Columns (1) – (2), each outcome variable is the trait score of an individual applicant, derived from their psychometric test results. In Columns (3) – (4), each outcome variable is constructed based on annual evaluation reports for the recruited candidates in the fiscal year 2019. In Column (5), where the outcome variable is an indicator for whether the employee was ever warned for corruption-related misconduct by 2019. "*Applying after college city inspected*" is a dummy equal to 1 for individuals who applied to state jobs after their college city underwent an anti-corruption visit. Individual controls include fixed effects for gender, age, and ethnicity. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 18]

Table 6: Anti-corruption and trait changes in the short run

	Student psychometric z-scores					
	Integrity	Public service motivation	Risk attitudes	Interpersonal intelligence	Emotional maturity	Cognitive ability
	(1)	(2)	(3)	(4)	(5)	(6)
Inspected	0.031	0.052	-0.010	-0.007	0.064	-0.003
	(0.060)	(0.061)	(0.015)	(0.016)	(0.070)	(0.008)
Survey year FEs	Y	Y	Y	Y	Y	Y
City FEs	Y	Y	Y	Y	Y	Y
Individual controls	Y	Y	Y	Y	Y	Y
Observations	7,823	7,823	7,823	7,823	7,823	7,823
Data source	College Student Job Outlook Survey (2015 – 2017)					

Notes: Unit of observation is the student. "*Inspected*" is a dummy that is 1 if the city of college attendance has been inspected by an anti-corruption visit team. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Individual controls include fixed effects for age, gender, ethnicity, rural residence, and Party membership. Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 21]

Table 7: Anti-corruption and student preferences for state jobs

	Preferring state sector jobs (binary)			
	(1)	(2)	(3)	(4)
Mean of dep. var	0.385	0.385	0.385	0.385
Inspected	-0.008	-0.008	-0.008	-0.008
	(0.010)	(0.009)	(0.009)	(0.008)
× Integrity		0.055**	0.045**	0.042**
		(0.026)	(0.015)	(0.015)
× Public service motivation			0.013**	0.014**
			(0.006)	(0.007)
× Risk attitudes			-0.015	-0.017
			(0.010)	(0.012)
× Interpersonal intelligence			-0.004	0.002
			(0.008)	(0.008)
× Emotional maturity			0.010	0.009
			(0.007)	(0.006)
× Cognitive ability			0.009	0.008
			(0.019)	(0.017)
Lower-order terms	Y	Y	Y	Y
Survey year FEs	Y	Y	Y	Y
City FEs	Y	Y	Y	Y
Individual controls				Y
Observations	7,823	7,823	7,823	7,823
Data source	College Student Job Outlook (2015 – 2017)			

Notes: Unit of observation is the student. Each trait measure is normalized into a standardized z-score. "*Inspected*" is a dummy that is 1 if the city of college attendance has been inspected by an anti-corruption visit team. Individual controls include fixed effects for age, gender, ethnicity, rural residence, and Party membership. Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 22]

Table 8: Supplementary evidence on household preferences for state jobs

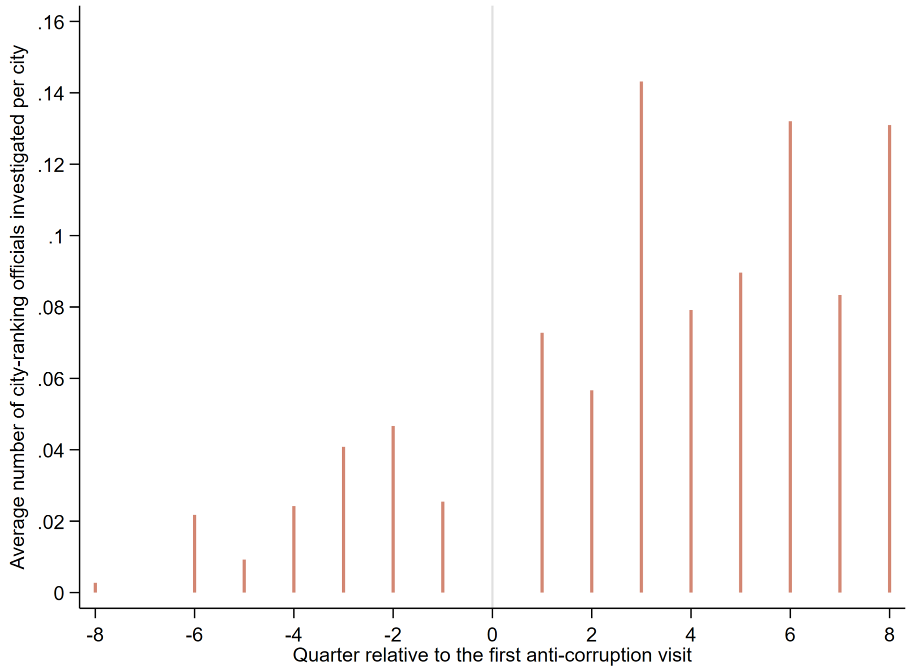
	Occupational preferences: state sector jobs			Occupational preferences: government officials		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of dep. var	0.352	0.355	0.355	0.061	0.062	0.062
Inspected	0.013	0.011	0.008	-0.003	-0.003	-0.003
	(0.024)	(0.024)	(0.024)	(0.011)	(0.011)	(0.011)
× Prosociality & integrity z-score	0.035***	0.035***	0.034***	0.014**	0.012*	0.012*
	(0.012)	(0.011)	(0.011)	(0.007)	(0.006)	(0.006)
× Cognitive ability z-score		0.006	0.003		0.004	0.003
		(0.012)	(0.011)		(0.008)	(0.009)
× Socioeconomic z-score			-0.015			-0.009
			(0.013)			(0.006)
Household FEs	Y	Y	Y	Y	Y	Y
Time (semester) FEs	Y	Y	Y	Y	Y	Y
Observations	17,455	17,455	17,455	17,455	17,455	17,455
Data source	China Education Panel Survey (2013 – 2015)					

Notes: Unit of observation is the household-year. All student trait z-scores are measured *ex-ante*. The dependent variable is a dummy that is 1 if both the child and their parent prefer the corresponding occupation; "*Government official*" positions are a subset of broader "*state sector jobs*". "*Inspected*" is a dummy that is 1 if the city of residence has been inspected by an anti-corruption visit team. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 22]

Appendix A Additional Figures and Tables

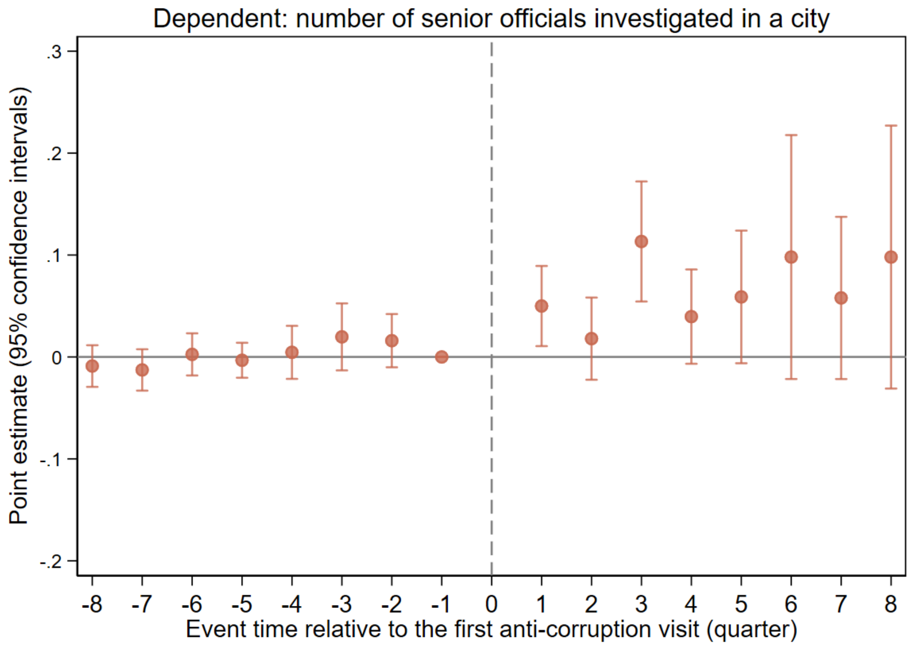
Figure A1: Anti-corruption visits and # officials investigated per city



Notes: The figure shows the average number of city-ranking (senior) local officials investigated per city by quarter bins. Each city usually has 9-14 incumbent city-ranking positions.

[Back to Page 16]

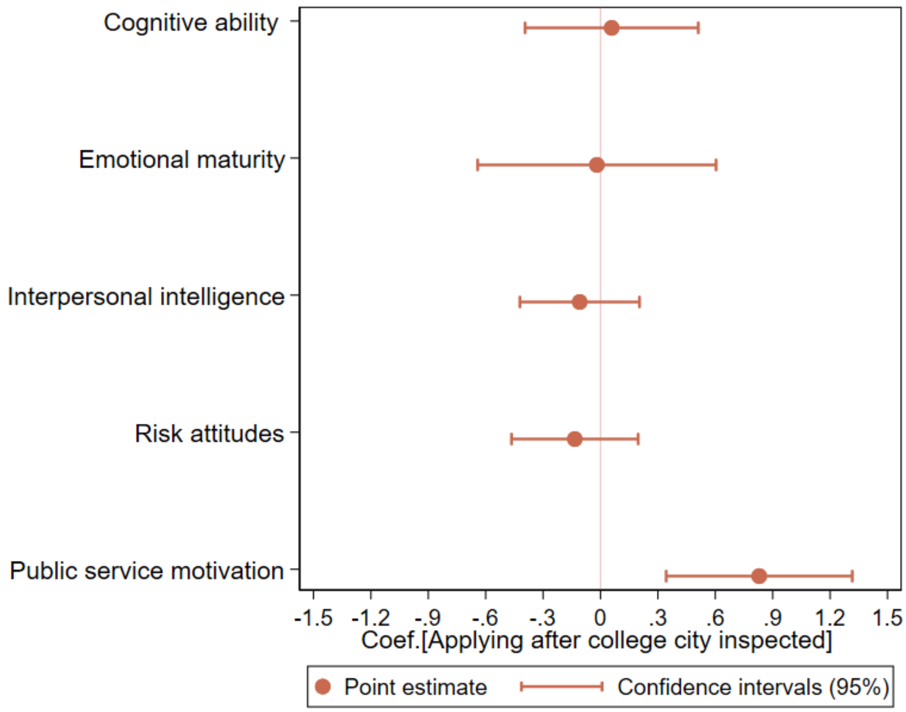
Figure A2: Anti-corruption visits and number of officials investigated (event study plot)



Notes: The figure presents the dynamic estimated effects of province-to-city anti-corruption visits on the number of senior local officials investigated using the stacked DiD estimator (Wing, Freedman and Hollingsworth, 2024). The unit of observation is the city-quarter.

[Back to Page 16]

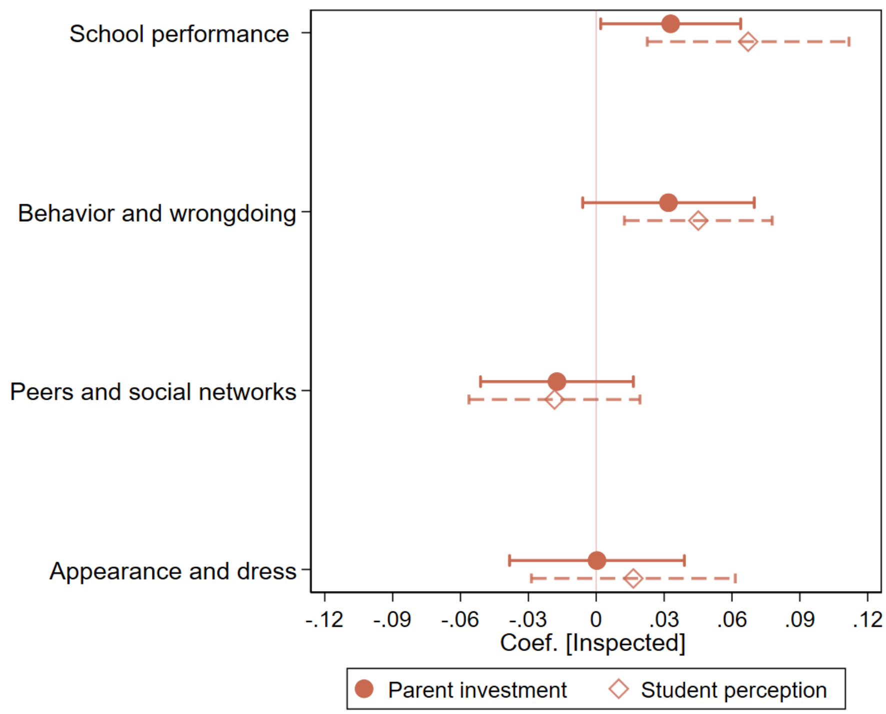
Figure A3: Anti-corruption and candidate traits for state organizations



Notes: The figure shows the estimated effects of anti-corruption visits on applicants' psychometric traits for 90 sampled state organizations. Each outcome variable is a z-score. The specification corresponds to that of **Table 4**. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). The 95% confidence intervals are reported using standard errors clustered at the city level.

[Back to Page 18]

Figure A4: Household attention to talent investment in the next generation



Notes: The figure presents the estimated effects of anti-corruption visits on how parents allocate attention to different dimensions of investment in their children's human capital. The dots visualize the results based on parent responses, and the hollow diamonds visualize the results using child responses. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). The 95% confidence intervals are reported using standard errors clustered at the city level. Data source: China Education Panel Survey (2013–2015).

[Back to Page 25]

Table A1: Anti-corruption visits and citizen perception – heterogeneity

	Perceived anti-corruption harshness (1, poor – 4, effective)		
	(1)	(2)	(3)
Mean of dep. var	2.877	2.847	2.847
Inspected	0.158***	0.135*	0.144**
	(0.065)	(0.077)	(0.065)
× Local media consumption		0.053*	
		(0.029)	
× CCP member			0.076*
			(0.043)
Year FEs	Y	Y	Y
City FEs	Y	Y	Y
Individual controls	Y	Y	Y
Observations	17,870	17,870	17,870
Data source	Chinese Social Survey (2011 – 2017)		

Notes: Unit of observation is the individual. "*Inspected*" is a dummy that is 1 if the city of residence has been inspected by an anti-corruption visit team. "*Local media consumption*" is a dummy that is 1 if the individual indicates they frequently consume local newspapers and radio. ''*CCP member*" is a dummy for Chinese Communist Party membership, capturing one's informational proximity to local state sectors. Individual controls include fixed effects for gender, age, family size, educational attainment, rural residence, and Party membership. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 17]

Table A2: Robustness to alternative DiD estimators

Org-city-year level analysis (90 sampled orgs., 2011 – 2017)	Psychometric integrity z-score	Cognitive ability z-score
	(1)	(2)
Two-way fixed effects	0.733***	0.052
	(0.174)	(0.183)
Two-way fixed effects (weighted by applicant #)	0.683**	-0.036
	(0.250)	(0.276)
De Chaisemartin and d'Haultfoeuille (2020)	0.741***	0.147
	(0.110)	(0.196)
Sun and Abraham (2021)	0.804***	0.125
	(0.189)	(0.208)
Callaway and Sant'Anna (2021)	0.812***	-0.016
	(0.104)	(0.157)
Borusyak, Jaravel and Spiess (2024)	0.705***	0.019
	(0.143)	(0.175)
Wing, Freedman and Hollingsworth (2024)	0.758**	0.037
	(0.291)	(0.181)

Notes: Unit of observation is the organization-city-year. Each cell reports the estimated coefficient of "*Applying after college city inspected*" from a separate regression, using the corresponding robust DiD estimator. Each outcome variable represents the average trait score of applicants from a given city-year cell for a particular state organization, based on their psychometric test results. This paper employs as its baseline the weighted stacked DiD estimator by Wing, Freedman and Hollingsworth (2024). Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 17]

Table A3: Robustness to alternative clustering criteria

Org-city-year level analysis (90 sampled orgs., 2011 – 2017)	Psychometric integrity z-score	Cognitive ability z-score
	(1)	(2)
Applying after college city inspected	0.758	0.037
Cluster: city	(0.291)**	(0.181)
Cluster: organization	(0.111)***	(0.162)
Cluster (Two-way): city & organization	(0.265)**	(0.209)
Cluster (Wild-bootstrap P-value): city	[0.016]***	[0.588]
Recruitment year × Organization FEs	Y	Y
College city × Organization FEs	Y	Y
Observations	8,379	8,379

Notes: Unit of observation is the organization-city-year. Each outcome variable represents the average trait score of applicants from a given city-year cell for a particular state organization, based on their psychometric test results. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Parentheses show standard errors based on different clusters. Brackets report wild-bootstrap P-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 17]

Table A4: Heterogeneous effects by anti-corruption intensity

Org-city-year level analysis (90 sampled orgs., 2011 – 2017)	Psychometric integrity z-score		Cognitive ability z-score	
	(1)	(2)	(3)	(4)
Applying after college city inspected	0.905**	0.521	-0.104	0.069
	(0.428)	(0.333)	(0.380)	(0.210)
Sample	High intensity	Low intensity	High intensity	Low intensity
Recruitment year $\times$ Organization FEs	Y	Y	Y	Y
College city $\times$ Organization FEs	Y	Y	Y	Y
Observations	3,429	4,950	3,429	4,950

Notes: Unit of observation is the organization-city-year. "*Applying after college city inspected*" is a dummy equal to 1 for individuals who applied to state jobs after their college city underwent an anti-corruption visit. A city is classified as "high-intensity" if the number of its investigated senior officials (city-ranked or above) per post-crackdown year exceeds the mean 0.506. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 18]

Table A5: Psychometric traits, recruitment, and post-recruitment performance

	Recruited (binary)		Performance: "Probity & discipline" (1, poor - 4, outstanding)		Performance: "Overall effectiveness" (1, poor - 4, outstanding)	
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of dep. var	0.033	0.036	2.740	2.922	2.817	2.938
Integrity (z-score)	0.004*	0.003*	0.372***	0.401***	0.343***	0.399***
	(0.002)	(0.002)	(0.129)	(0.134)	(0.071)	(0.055)
Cognitive ability (z-score)	0.009***	0.009***	0.085	0.100	0.439***	0.507***
	(0.003)	(0.002)	(0.116)	(0.096)	(0.099)	(0.073)
Subsample	Untreated candidates	Treated candidates	Untreated employees	Treated employees	Untreated employees	Treated employees
Statistical difference: Integrity	P-value = 0.453		P-value = 0.717		P-value =0.628	
Statistical difference: Cognitive ability	P-value = 0.821		P-value = 0.925		P-value =0.373	
Recruitment year × Organization FEs	Y	Y	Y	Y	Y	Y
College city × Organization FEs	Y	Y	Y	Y	Y	Y
Individual controls	Y	Y	Y	Y	Y	Y
Observations	14,665	22,586	476	817	476	817

Notes: Unit of observation is the individual. Columns (1) – (2) use candidate data from the five state organizations between 2011 and 2017, while Columns (3) – (6) use data on recruited candidates from the same period and organizations. The subsample is divided according to whether individuals' college city had been inspected. "*Integrity*" and "*Cognitive ability*" refer to application-stage psychometric measures. Columns (1) – (2) assess how these traits affect the likelihood of recruitment, and Columns (3) – (6) test whether they are positively associated with post-recruitment performance. Individual controls include fixed effects for gender, age, and ethnicity. Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 19]

Table A6: Student preferences for state jobs by integrity group

	Preferring state sector jobs (binary)	
	(1)	(2)
Mean of dep. var	0.389	0.382
Inspected	0.096**	-0.043*
	(0.045)	(0.026)
Sample	High integrity	Low integrity
Survey year FEs	Y	Y
City FEs	Y	Y
Individual controls	Y	Y
Observations	3,563	4,260
Data source	College Student Job Outlook Survey (2015 – 2017)	

Notes: Unit of observation is the student. A student is classified as "*high integrity*" if their psychometric integrity score exceeds the sample mean score; otherwise, they are classified as "*low integrity*". "*Inspected*" is a dummy that is 1 if the city of college attendance has been inspected by an anti-corruption visit team. Individual controls include fixed effects for age, gender, ethnicity, rural residence, and Party membership. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 22]

Table A7: Correlation between individual traits

	Integrity	Public service motivation	Risk attitudes	Interpersonal intelligence	Emotional maturity	Cognitive ability
Integrity	-					
Public service motivation	0.423	-				
Risk attitudes	-0.084	-0.051	-			
Interpersonal intelligence	-0.064	-0.003	0.042	-		
Emotional maturity	0.070	0.101	-0.005	0.033	-	
Cognitive ability	0.017	0.008	-0.035	0.059	0.026	-
Data source	College Student Job Outlook Survey (2015 – 2017)					

Notes: The table reports pairwise correlations between key individual psychometric traits (in z-scores), based on 7,823 students from the CSJO data (2015 – 2017).

[Back to Page 13][Back to Page 22]

Table A8: Anti-corruption and perceived chances of attaining jobs

	Perceived chances of attaining a desirable job (1, low – 10, high)		
	(1)	(2)	(3)
Mean of dep. var	4.467	4.467	4.467
Inspected	0.126	0.103	0.101
	(0.152)	(0.166)	(0.159)
× Integrity (z-score)		0.051	0.059
		(0.082)	(0.078)
Lower-order terms	Y	Y	Y
Survey year FEs	Y	Y	Y
City FEs	Y	Y	Y
Individual controls			Y
Observations	7,823	7,823	7,823
Data source	College Student Job Outlook Survey (2015 – 2017)		

Notes: Unit of observation is the student. "*Inspected*" is a dummy that is 1 if the city of college attendance has been inspected by an anti-corruption visit team. Individual controls include fixed effects for age, gender, ethnicity, rural residence, and Party membership. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 24]

Table A9: Accounting for changes in city socioeconomic conditions

Org-city-year level analysis (90 sampled orgs., 2011 – 2017)	Psychometric integrity z-score		Cognitive ability z-score	
	(1)	(2)	(3)	(4)
Applying after college city inspected	0.758**	0.811***	0.037	0.100
	(0.291)	(0.240)	(0.181)	(0.180)
Recruitment year × Organization FEs	Y	Y	Y	Y
College city × Organization FEs	Y	Y	Y	Y
City-year time-variant controls		Y		Y
City characteristics (2010) × Event time FEs		Y		Y
Observations	8,379	8,379	8,379	8,379

Notes: Unit of observation is the organization-city-year. "*Applying after college city inspected*" is a dummy equal to 1 for individuals who applied to state jobs after their college city underwent an anti-corruption visit. City controls refer to the socioeconomic characteristics of college cities. Time-variant characteristics include (log) GDP, population, fiscal expenditure, fiscal revenue, and the relative average wages in the public and private sectors. Time-invariant baseline characteristics include the distance to the coast, average years of schooling, the share of urban residents, and the share of public workers, which are only available from the 2010 Population Census. The relative wage statistics are obtained with the assistance of the provincial human resource centers; all other city-level statistics are publicly available from city statistical yearbooks and population censuses. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 23]

Table A10: Accounting for neighboring city spillovers

Org-city-year level analysis (90 sampled orgs., 2011 – 2017)	Psychometric integrity z-score		Cognitive ability z-score	
	(1)	(2)	(3)	(4)
Applying after college city inspected	0.758**	0.891**	0.037	0.134
	(0.291)	(0.344)	(0.181)	(0.225)
Recruitment year × Organization FEs	Y	Y	Y	Y
College city × Organization FEs	Y	Y	Y	Y
Excluding spillover-affected controls		Y		Y
Observations	8,379	5,220	8,379	5,220

Notes: Unit of observation is the organization-city-year. "*Applying after college city inspected*" is a dummy equal to 1 for individuals who applied to state jobs after their college city underwent an anti-corruption visit. For "*excluding spillover-affected controls*", I follow the design of Colonnelli and Prem (2022) by removing all control cities that ever neighbored an inspected one. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Back to Page 23]

Appendix B Conceptual Discussion

This section presents a parsimonious theoretical framework, focusing on how changes in corruption affect the sorting of two key traits — ability and integrity — into the state sector. As integrity is positively correlated with public service motivation/prosociality in the data and enters job utility in a mathematically similar manner, I do not further model them separately here. Corruption yields extra room for state-job rents and thus lowers the relative values of private jobs for high-ability individuals, but it also generates disutility to honest and prosocial individuals. To fix ideas, I present a stylized model built upon the frameworks of Hanna and Wang (2017) and Ashraf et al. (2020). I also discuss extensions that consider perceived recruitment probabilities and endogenous changes in traits.

Each individual i is characterized by (A_i, π_i): ability A_i and integrity π_i, which are independently distributed.[31] One can either enter the state sector or the private sector. The private-sector return is solely determined by ability, $m(A_i)$, with $m'(A_i) > 0$. The nature of the framework remains unchanged as long as the corrupt utility gains are greater in the state sector than in the private sector.

In contrast, state employees receive $w + g(c, A_i) + h(c, \pi_i)$, a combination of a constant wage w, additional rents $g(c, A_i)$, and public-service-related utility $h(c, \pi_i)$. $c > 0$ refers to the societal level of corruption, which is taken as given for an individual when choosing their job to reflect the equilibrium nature of corruption (Acemoglu, 1995; Fisman and Golden, 2017). The rent term $g(c, A_i)$ is increasing in both corruption and ones' ability ($\frac{\partial g}{\partial c} > 0$ and $\frac{\partial g}{\partial A_i} > 0$). As is standard, I assume $\frac{\partial g}{\partial A_i} < \frac{\partial m}{\partial A_i}$: on the margin, ability is more valuable in the private sector than it is in the state sector (Ashraf et al., 2020). This is particularly plausible for entry-level officials in my study, who lack access to large-scale corrupt rents and whose promotions depend primarily on seniority. It also guarantees that the implicit function remains well-behaved — an issue I return to later. The public service gain $h(c, \pi_i)$ increases with one's integrity and decreases with the perceived corruption level ($\frac{\partial h}{\partial c} < 0$, $\frac{\partial h}{\partial \pi_i} > 0$). Moreover and relevantly, the disutility from corruption is more severe for individuals with higher integrity ($\frac{\partial^2 h}{\partial c \partial \pi_i} < 0$).

Together, an individual will prefer state jobs if and only if:

$$\begin{gathered} w + g(c, A_i) + h(c, \pi_i) > m(A_i), \\ \text{where } g_c > 0, g_A > 0, h_c < 0, h_\pi > 0, h_{c\pi} < 0, m_A > 0 \end{gathered} \tag{5}$$

[31] The independence assumption aligns with the data pattern that ability and integrity are only weakly correlated (consistent with most previous studies), while also enhancing the model's tractability.

This parsimonious model is not meant to provide a comprehensive theoretical exploration; instead, it intends to help articulate the margins through which sorting can occur in response to changes in corruption. To see this, let $\tilde{A}$ denote the marginal type that is indifferent from state jobs and private jobs. For a fixed integrity type π_i, we have:

$$\frac{\partial \tilde{A}}{\partial c} = \frac{g_c(c,\tilde{A}) + h_c(c,\pi_i)}{m'(\tilde{A}) - g_A(c,\tilde{A})} \tag{6}$$

As noted, $m'(\tilde{A}) - g_A(c,\tilde{A}) > 0$ since $g_A < m_A$. This ensures the existence of implicit function, and implies that for any given π, individuals with $A < \tilde{A}$ always prefer state jobs. Because $g_c > 0$ and $h_{c\pi} < 0$, the numerator is strictly decreasing in π. This indicates that there exists at most one threshold integrity level $\pi^*(c)$ such that $g_c + h_c(c,\pi^*) = 0$ (i.e., $\frac{\partial \tilde{A}(c,\pi^*)}{\partial c}$=0). Hence, the sign of $\frac{\partial \tilde{A}}{\partial c}$ can change at most once as integrity rises. We therefore have the following comparative statics:

Proposition 1 (Sign of $\frac{\partial \tilde{A}}{\partial c}$)**.** *With $m'(A_i) - g_A(c,A_i) > 0$ and $h_{c\pi} < 0$, there exists at most one pivotal $\pi^*(c)$ such that $g_c + h_c(c,\pi^*) = 0$,*

$$\tilde{A}_c(c,\pi_i) = \frac{\partial \tilde{A}}{\partial c} = \frac{g_c(c,\tilde{A}) + h_c(c,\pi_i)}{m'(\tilde{A}) - g_A(c,\tilde{A})} \begin{cases} > 0 & \textit{if} \quad \pi_i < \pi^*(c) \\ = 0 & \textit{if} \quad \pi_i = \pi^*(c) \\ < 0 & \textit{if} \quad \pi_i > \pi^*(c) \end{cases}$$

*That is, $\frac{\partial \tilde{A}}{\partial c}$ exhibits a **single-crossing property** in π.*

Conceptually, reducing corruption ($c \downarrow$) may lower the ability of the marginal type among the low-integrity group ($\pi_i < \pi^*(c)$, $-\frac{\partial \tilde{A}}{\partial c} < 0$), as now it curtails the illegal returns and that the low-integrity type does not gain much from performing prosocial duties. At the same time, however, it can improve the sorting of competent individuals among the high-integrity group ($\pi_i > \pi^*(c)$, $-\frac{\partial \tilde{A}}{\partial c} > 0$), as there is a relatively large increase in prosocial returns for them following the anti-corruption, so the marginal type needs to have a higher outside option (i.e., greater ability) to compensate. Similarly, we can show that the sign of $\frac{\partial \tilde{\pi}}{\partial c}$ is also not strictly positive or negative (though it may fail to satisfy the single-crossing property without additional assumptions on g_{cA}).

Below, I formalize the comparative statics on how average ability and integrity of state-sector applicants respond to changed corruption levels. Suppose A_i and π_i are independent with densities $f_A(\cdot)$ and $f_\pi(\cdot)$, and cumulative distribution functions $F_A(\cdot)$ and $F_\pi(\cdot)$, respectively.

Let S denote the share of population preferring state jobs, $\bar{A}$ the average applicant ability, and $\bar{\pi}$ the average applicant integrity. Then, by definition:

$$\begin{aligned} S &= \int_{\pi} F_A(\tilde{A}(c,\pi))\; f_{\pi}(\pi)\, d\pi, \\ \bar{\pi} &= \frac{1}{S}\int \pi f_A(\tilde{A}(c,\pi))\; f_{\pi}(\pi)\, d\pi, \\ \bar{A} &= \frac{1}{S}\int_{\pi} [\int_{\min A}^{\tilde{A}(c,\pi)} a f_A(a) da]\; f_{\pi}(\pi)\, d\pi. \end{aligned} \tag{7}$$

Differentiating with respect to c gives:

$$\begin{aligned} \frac{dS}{dc} &= \int f_A(\tilde{A})\, \tilde{A}_c(c,\pi)\, f_{\pi}(\pi)\, d\pi, \\ \frac{d\bar{A}}{dc} &= \frac{1}{S}\int f_A(\tilde{A})\, \tilde{A}_c(c,\pi)\, (\tilde{A}(c,\pi) - \bar{A})\, f_{\pi}(\pi)\, d\pi. \\ \frac{d\bar{\pi}}{dc} &= \frac{1}{S}\int f_A(\tilde{A})\, \tilde{A}_c(c,\pi)\, (\pi - \bar{\pi})\, f_{\pi}(\pi)\, d\pi, \end{aligned} \tag{8}$$

As shown in **Proposition 1**, the sign of $\tilde{A}_c(c,\pi)$ may not always be strictly positive or negative: it is positive when $\pi < \pi^*$ and negative when $\pi < \pi^*$ (s.t. $\tilde{A}_c(c,\pi^*) = 0$). Moreover, its sign does not necessarily change monotonically as $(\tilde{A}(c,\pi) - \bar{A})$ crosses zero. Consequently, the sign of $\frac{d\bar{A}}{dc}$ (changes in average ability) is highly ambiguous, depending on the specific functional forms and the underlying trait distributions.

For $\frac{d\bar{\pi}}{dc}$ (changes in average integrity), provided that the distribution of types is sufficiently regular — meaning π^* and $\bar{\pi}$ are not too divergent due to heavy skewness — we obtain $\frac{d\bar{\pi}}{dc}$<0. Intuitively, as corruption becomes more pervasive, the state sector can likely attract individuals with a greater propensity for dishonesty.

Proposition 2 (Changes in average applicant quality)**.** *Write $S(c)$ for the state-sector applicant population, $\bar{\pi}(c)$ and $\bar{A}(c)$ for applicant quality means. Let $A \perp \pi$ with densities f_A, f_{π}. Assume the indifferent ability cutoff $\tilde{A}(c,\pi)$ under recruitment probability exists and is differentiable with*

$$\tilde{A}_c(c,\pi) = \frac{g_c(c,\tilde{A}) + h_c(c,\pi)}{m'(\tilde{A}) - g_A(c,\tilde{A})}$$

and the denominator is positive on the relevant support. Then

$$\begin{aligned}\frac{dS}{dc} &= \int f_A\big(\tilde{A}(c,\pi)\big)\,\tilde{A}_c(c,\pi)\,f_\pi(\pi)\,d\pi,\\ \frac{d\bar{\pi}}{dc} &= \frac{1}{S}\int f_A\big(\tilde{A}(c,\pi)\big)\,\tilde{A}_c(c,\pi)\,\big(\pi-\bar{\pi}(c)\big)\,f_\pi(\pi)\,d\pi,\\ \frac{d\bar{A}}{dc} &= \frac{1}{S}\int f_A\big(\tilde{A}(c,\pi)\big)\,\tilde{A}_c(c,\pi)\,\big(\tilde{A}(c,\pi)-\bar{A}(c)\big)\,f_\pi(\pi)\,d\pi.\end{aligned}$$

Without additional restrictions on (g, h, f) or on (f_A, f_π), ***each derivative is sign-indeterminate****. Yet, there exists a threshold $\delta > 0$ such that:*

$$\frac{d\bar{\pi}}{dc} < 0 \quad \textit{if} \quad |\pi^*(c) - \bar{\pi}(c)| < \delta$$

That is, as long as the marginal entrant (pivot type) is not too distant from the average applicant, ***the anti-corruption shock*** $(c\downarrow)$ ***always improves average applicant integrity****.*

Proof.

$$\begin{aligned}\frac{d\bar{\pi}}{dc} &= \frac{1}{S}\int f_A\big(\tilde{A}(c,\pi)\big)\,\tilde{A}_c(c,\pi)\,\big(\pi-\bar{\pi}(c)\big)\,f_\pi(\pi)\,d\pi\\ &= \frac{1}{S}\int(\pi-\pi^*)\tilde{A}_c f_A f_\pi d\pi + \frac{1}{S}\int(\pi^*-\bar{\pi})\tilde{A}_c f_A f_\pi d\pi\end{aligned}$$

By Proposition 1, the first term always satisfies

$$\frac{1}{S}\int(\pi-\pi^*)\tilde{A}_c f_A f_\pi d\pi < 0$$

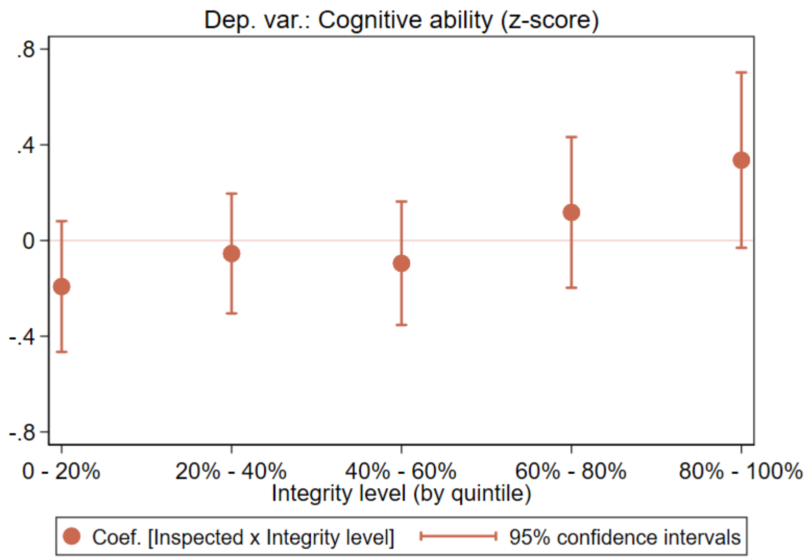
By continuity, there exists a $\delta > 0$ such that if $|\pi^* - \bar{\pi}| < \delta$, the second term is small enough that it does not offset the first. That is:

$$\frac{d\bar{\pi}}{dc} < 0 \quad \textit{if} \quad |\pi^*(c) - \bar{\pi}(c)| < \delta$$

Notably, if there exist regions where $m'(A_i) - g_A(c, A_i) < 0$, all the signs *flip* relative to our earlier statements. This may be more likely to arise at very senior levels, where state-sector rents scale *faster* with ability than private-sector returns. In such cases, highly capable individuals in top posts may extract more from state power than from private employment. Nevertheless, allowing for such extensions does not alter the key takeaway: the overall effect of corruption on the applicant pool's quality remains ambiguous and thus requires empirical investigation.

I close by empirically testing the single-crossing property of $\frac{\partial \tilde{A}}{\partial c}$ in my data: while the *net* effect of anti-corruption visits on state-sector applicant ability is near zero, the *local* effect may differ by individual integrity ($\pi_i < \pi^*(c)$, $-\frac{\partial \tilde{A}}{\partial c} < 0$; $\pi_i > \pi^*(c)$, $-\frac{\partial \tilde{A}}{\partial c} > 0$). To do so, I use the individual-level applicant data from five state organizations and allow the impact of anti-corruption on ability to vary by integrity level. Figure B1 presents the results. The empirical patterns are broadly consistent with the predictions of **Proposition 1**. However, since it is difficult to verify whether the marginal return condition $m'(A_i) - g_A(c, A_i) > 0$ holds across the full support, I interpret these findings as heuristic and suggestive.

Figure B1: Heterogeneous effects on state-sector applicant ability (by integrity level)



Notes: The figure tests **Proposition 1**, by examining heterogeneous changes in applicant ability by integrity level in response to anti-corruption visits. $Inspected$ is a dummy that is 1 if an applicant's college city underwent an anti-corruption visit. The unit of observation is the individual. The sample covers five state organizations for which individual-level data are available. Sample period: 2011 - 2017. The stacked DiD estimator is used (Wing, Freedman and Hollingsworth, 2024). The 95% confidence intervals are constructed using standard errors clustered at the city level.

Extension 1: Perceived Recruitment Probability

Sophisticated applicants may also consider their probability of recruitment. In particular, anti-corruption shocks can increase perceived fairness in recruitment or signal a stronger emphasis on integrity, thereby disproportionately benefiting high-integrity types. For simplicity, I scale the expected payoff from state jobs by a perceived recruitment probability $Pr(A, \pi, c)$. This captures the fact that government jobs are rationed and subject to exams and approval processes, whereas private opportunities are closer to competitive markets and are thus represented by the deterministic payoff $m(A)$. The qualitative nature of the model remains unchanged as long as integrity and corruption play a relatively greater role in state-sector recruitment.

Let the perceived probability of being recruited into the state sector be

$$\Pr(A, \pi, c) = \Lambda(z), \qquad z := \alpha_0 + \alpha_A A + \alpha_\pi \pi + \alpha_{\pi c}\, \pi c,$$

where $\Lambda(x) = \frac{e^x}{1+e^x}$ is the logistic CDF. Assume $\alpha_A > 0$, $\alpha_\pi > 0$, and $\alpha_{\pi c} < 0$ so anti-corruption ($c \downarrow$) raises hiring chances more for high-integrity types ($\Pr_{c\pi} < 0$). An individual will prefer state jobs if and only if:

$$\Pr(A_i, \pi_i, c)\, U(c, A_i, \pi_i) > m(A_i), \qquad \text{where } U(c, A_i, \pi_i) = w + g(c, A_i) + h(c, \pi_i) \tag{9}$$

For each π, the indifferent type's ability is now defined by $\Pr\big(\tilde{A}, \pi, c\big)\, U\big(c, \tilde{A}, \pi\big) = m\big(\tilde{A}\big)$. Differentiating with respect to c gives:

$$\frac{\partial \tilde{A}}{\partial c} = \frac{\Pr_c \times U + \Pr \times (g_c + h_c)}{m'(\tilde{A}) - \Pr_A \times U - \Pr \times g_A}. \tag{10}$$

Now, if we still assume that the denominator $\Big(m'(\tilde{A}) - \Pr_A \times U - \Pr \times g_A\Big) > 0$ — that is, the marginal return to ability in private jobs exceeds that in the state sector even after accounting for recruitment processes — then the single-crossing property established in **Proposition 1** still holds and becomes even more robust.

Compared to the baseline model where one only considers expected job utility, considering recruitment likelihood further introduces a compounding effect of corruption c for high integrity groups, as a higher c also worsens the odds of being hired for high-π types. The implications for candidate quality changes remain similar to the baseline case.

Extension 2: Endogenous Integrity Changes

If individuals adapt their personality quickly enough to changes in societal reward structures, we may expect the population's overall integrity to rise following the corruption crackdown.[32] As this could constitute a potential mechanical channel for interpreting my core results, I present a theoretical discussion below.

Allow integrity to adapt to corruption: $\pi(c) = \pi^0 + \rho(c)$ with $\rho'(c) < 0$, so greater corruption reduces population's integrity. Similar to the former analysis, we can study how the indifferent

[32] Empirical evidence on this is mixed. In Mexico, Ajzenman (2021) finds that political corruption temporarily increases citizens' dishonesty norms. In Italy, Gulino and Masera (2022) shows that corruption scandals raise the propensity of supermarket customers to steal, though the effect is highly short-lived. By contrast, in Brazil, Guida-Johnson (2025) finds that anti-corruption audits do not significantly alter citizens' law-breaking behavior in the short run.

type's ability cutoff responds to corruption:

$$\frac{\partial \tilde{A}}{\partial c} = \frac{g_c\big(c, \tilde{A}\big) + h_c\big(c, \pi(c)\big) + h_\pi\big(c, \pi(c)\big) \times \rho'(c)}{m'(\tilde{A}) - g_A\big(c, \tilde{A}\big)}. \tag{11}$$

Compared to the baseline version (Equation 6), the only difference is the additional term $h_\pi \times \rho'(c) < 0$ that makes $\frac{\partial \tilde{A}}{\partial c}$ strictly smaller than in the exogenous-integrity case. The single-crossing property will remain valid if we assume a mild curvature:

$$h_{c\pi} + h_{\pi\pi}\, \rho'(c) \; < \; 0 \quad \text{for all relevant } (c, \pi^0),$$

so that $\frac{\partial \tilde{A}}{\partial c}$ is strictly decreasing in π.

The impact of c on average applicant quality remains indeterminate regardless of whether the curvature condition holds. However, when integrity is endogenous, we can show that it typically tilts the effect on average integrity ($\frac{d\bar{\pi}}{dc}$) downward. This suggests that curbing corruption ($c \downarrow$) is more likely to improve applicant integrity when individual traits are malleable. Such a potential mechanism carries important long-run implications for the evaluation of anti-corruption programs, as population-level traits are more appropriately viewed as endogenous in the longer run (Tirole, 1996; Ehrlich and Lui, 1999).

Proposition 3 (Changes in applicant quality with endogenous integrity)**.** *Let $A \perp \pi^0$ with densities f_A and f_{π^0}. Assume the indifferent ability cutoff $\tilde{A}(c, \pi)$ exists and is differentiable with*

$$\frac{\partial \tilde{A}}{\partial c} = \frac{g_c\big(c, \tilde{A}\big) + h_c\big(c, \pi(c)\big) + h_\pi\big(c, \pi(c)\big) \times \rho'(c)}{m'(\tilde{A}) - g_A\big(c, \tilde{A}\big)},$$

and the denominator is positive on the relevant support. Then

$$\frac{dS}{dc} = \int f_A(A^*)\, \frac{\partial \tilde{A}}{\partial c}(c, \pi^0)\, f_{\pi^0}(\pi^0)\, d\pi^0,$$

$$\frac{d\bar{\pi}}{dc} = \rho'(\mathbf{c}) \; + \; \frac{1}{S} \int f_A(\tilde{A})\, \frac{\partial \tilde{A}}{\partial c}(c, \pi^0)\, \big(\pi^0 + \rho(c) - \bar{\pi}(c)\big)\, f_{\pi^0}(\pi^0)\, d\pi^0,$$

$$\frac{d\bar{A}}{dc} = \frac{1}{S} \int f_A(\tilde{A})\, \frac{\partial \tilde{A}}{\partial c}(c, \pi^0)\, \big(\tilde{A}(c, \pi^0) - \bar{A}(c)\big)\, f_{\pi^0}(\pi^0)\, d\pi^0.$$

*Without additional restrictions on (g, h, f) or on (f_A, f_π), **each derivative is sign-indeterminate**. Yet, there exists a threshold $\delta > 0$ such that:*

$$\frac{d\bar{\pi}}{dc} < 0 \quad \textit{if} \quad |\pi^*(c) - \bar{\pi}(c)| < \delta$$

*That is, as long as the marginal entrant (pivot type) is not too distant from the average applicant, **the anti-corruption shock** ($c \downarrow$) **always improves average applicant integrity**.*

Since the first term in $\frac{d\bar{\pi}}{dc}$ is $\rho'(c) < 0$ (a direct integrity drift), and the second term is the same selection composition term as before (now in baseline π_0), ***endogenous integrity typically tends to tilt*** $\frac{d\bar{\pi}}{dc}$ ***downward*** *(so $-\frac{d\bar{\pi}}{dc}$* ***upward****) relative to the exogenous-integrity case.*

It is worth noting that the above analysis should be understood as partial equilibrium in the short run, while the general equilibrium implications (e.g. how changes in population types might feed back into c), which may further augment the "self-reinforcing corruption equilibrium" (Acemoglu, 1995), remain an important open avenue.

[Back to Page 21]

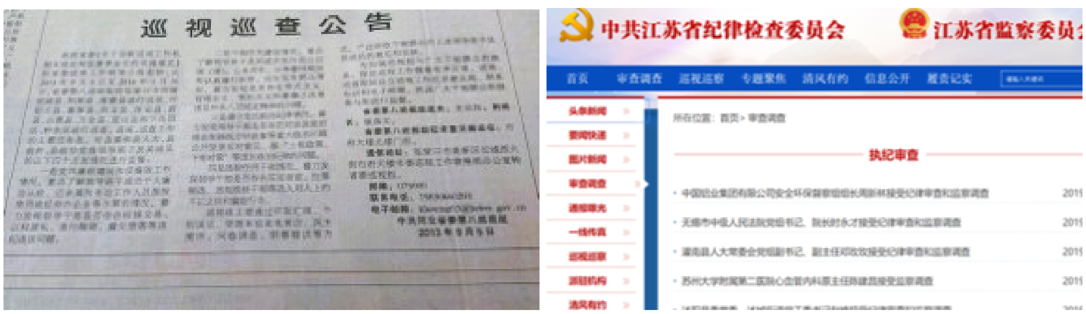
Appendix C Data Appendix

Appendix C.1 Anti-Corruption Visits

The timing of each province-to-city inspection visit is collected manually from local newspapers and authorized websites. I also elicit the number of local senior officials investigated due to corruption from the website of each *Provincial Commission for Discipline Inspection* (PCDI). Sample period: 2012 - 2017. The unit of observation is the *city-year-month*.

Right after the arrival of inspection teams, the provincial official website and local (city-level) newspapers will publicly announce the target prefectures, duration, inspection team leaders and contact information. Data on anti-corruption visits from 2014 to 2017 are obtained from the official websites of each province's PCDI. Data on anti-corruption visits from 2012 to 2013 are digitized from local newspapers, as most provincial websites only keep records for a certain period of time.

Figure C1: Sample of data sources for anti-corruption visits



Notes: The left panel shows a sample of the inspection visit announcement from a local newspaper in Hebei Province in 2014. The right panel shows a sample of the PCDI website (Jiangsu Province).

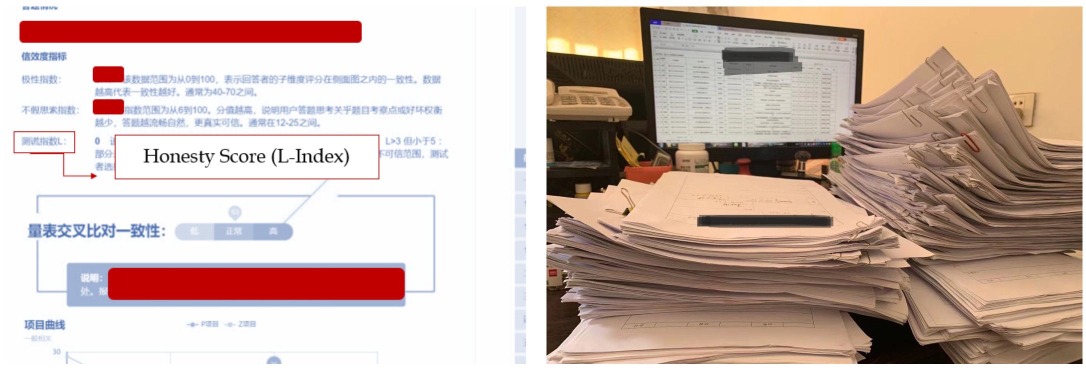
Appendix C.2 Applicant Psychometric Outcomes

To better illustrate the nature and validity of the integrity score used in my analysis, I present a set of selective, shareable sampled questions used in the past. As noted, the psychometric algorithm evaluates respondent integrity based on three key components: indirect cross-referencing, lie detection, and adjustments for inattention and cognitive ability.

1. Cross-Referencing and Lie-Detecting Questions

A web of cross-referencing questions is interspersed throughout the psychometric test. Specifically, questions with similar content will be presented in various forms multiple times, and these questions always pertain to socially desirable traits. First, inconsistencies within an applicant's responses contribute to the assessment of individual integrity. Second, the

Figure C2: Sample of data sources for state organization applicant/employee traits



Notes: The left panel shows a sampled personality test result from the background system, which contains a measure of integrity based on psychometric methods. This sampled screenshot is provided by the personality test provider. The right panel shows a collection of administrative employee assessment reports being digitized.

applicant's raw score on lie-detection questions is compared with the score distribution of a representative norm group, and extreme deviations are also used as a component of integrity assessment. These instruments are relatively well-established for assessing honesty in the psychometric literature (Edwards, 1957; Crowne and Marlowe, 1960; Schuessler, Hittle and Cardascia, 1978; Van Hooft and Born, 2012). Some canonical lie-detecting questions include: "*I never conceal my mistakes*", "*If necessary, I lie in some cases*", and "*I sometimes gossip about others*".

2. Other Lie-Detecting Designs

When technically feasible, honesty-game-style questions may also be incorporated to help detect cheating. Similar methods have been employed in recent experimental economic literature (e.g., Fischbacher and Föllmi-Heusi, 2013). An example of an outdated test is: "*Memorization test - the last question mentioned [some information], please choose the right answer below according to [that information]. Please do not return to the page noted to find answers.*" Applicants are not informed that their time and frequency on each page will be recorded. Returning to the last page may indicate a greater tendency toward dishonesty.

3. Accounting for Confounding Factors

Inattention, cognitive skills, and other personality traits will be taken into account and adjusted to ensure that the score is comparable across individuals. This addresses concerns that inconsistent answers may arise from carelessness or impatience. The following is an example of an inattention check question: "*Please select 5 in this question*" (with the time spent on the question recorded).

[Back to Page 12]

Appendix C.3 Validation of the Psychometric Integrity Measure

A validation study is conducted in collaboration with the human resource platform in their internal studies in December 2025. The sample consists of 580 pre-existing adults recruited for the internal test of their updated cognitive assessment system. After the assessment, each participant is required to take the standard personality test and also an incentivized cheating task online. Following the experimental economics literature (Fischbacher and Föllmi-Heusi, 2013), test-takers participate in a dice-rolling task online. Participants are instructed to roll a dice twelve times in private, with their monetary payoff determined solely by the number of sixes they reported. One additional "six" yields 50 CNY (about 7 USD). Since the actual rolls were unobserved, this setup ensures that while individual lying is undetectable, the incentive to cheat is salient. Consequently, an aggregate frequency of reported sixes exceeding the statistical probability provides a revealed-preference measure of dishonesty.

Specifically, I construct the behavioral dishonesty measure following Barfort et al. (2019). Suppose θ_i is the individual dishonesty propensity. Each individual participates in a series of K rounds of a dice game, which we index by k. y_{ik} is an indicator for whether individual i reports winning in round k. $Y_i = \Sigma^K y_{ik}$ is the *reported* total number of wins by respondent i. Finally, $p^*=\frac{1}{6}$ is the probability of (truthfully) having a dice roll as six. The probability of observing a six for a respondent with a given cheat rate is:

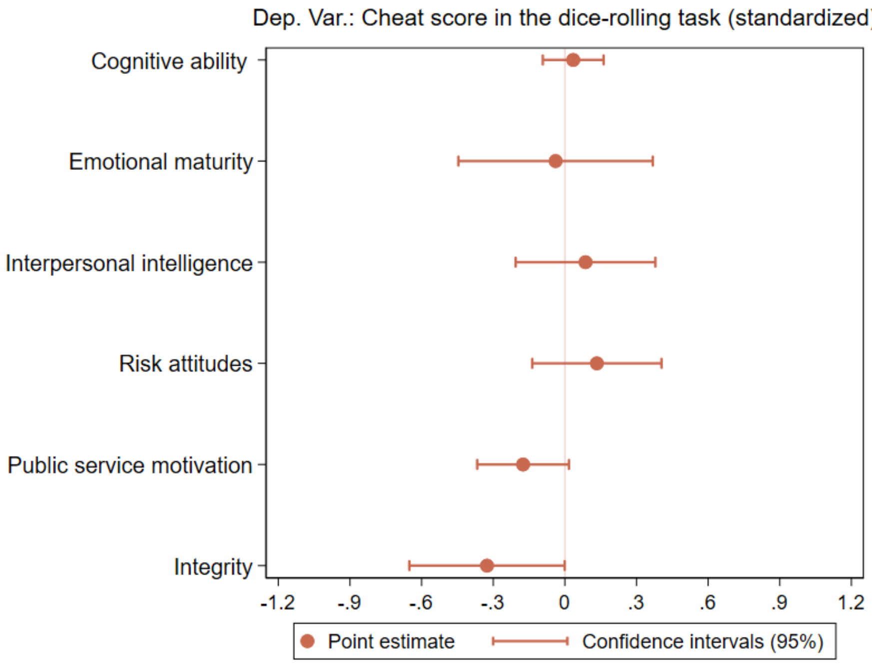
$$Pr(y_{ik} = 1|\theta_i) = E(y_{ik}|\theta_i) = p^* + (1 - p^*)\theta \tag{12}$$

Replacing the population moment $E(y_{ik}|\theta_i)$ with the empirical moment $(\frac{Y_i}{K})$, we can solve out the empirical cheat rate:

$$\widehat{Cheat\ Rate}_i = \frac{6}{5} \times (\frac{Y_i}{12} - \frac{1}{6}) \tag{13}$$

Figure C3 depicts the association between the behavioral cheat score and various psychometric traits assessed by the platform. We observe a significant negative correlation between the psychometric integrity score and behavioral dishonesty. This indicates that individuals scoring higher on their psychometric integrity are significantly less likely to report inflated outcomes in the incentivized dice task. Similarly, public service motivation — which is often linked to integrity — also displays a significant negative association with cheating behavior. These strong correlations provide validation that the psychometric measures effectively capture the latent traits of honesty and prosociality in an incentive-compatible setting. Furthermore, consistent with the previous studies, we find no significant association between behavioral dishonesty and cognitive ability or risk attitudes (Hanna and Wang, 2017; Barfort et al., 2019). This suggests that the integrity measure captures a distinct character trait rather than merely reflecting intelligence, test-taking skills, or risk-taking.

Figure C3: Correlations between psychometric measures and behavioral dishonesty



Notes: The figure reports the correlations between behavioral dishonesty and major psychometric measures. The dependent variable is the individual "cheat rate" in the incentivized dice-rolling task (standardized to have a mean of zero and a standard deviation of one). The independent variables are the respective scores from the psychometric test, also standardized. 95% confidence intervals are constructed using robust standard errors. N = 580.

[Back to Page 12]

Appendix C.4 Student Job Outlook Surveys

To study the mechanisms, I use the College Student Job Outlook (CSJO) surveys, which are jointly conducted by the provincial human resource center and the bureau of statistics in the studied province. The surveys target randomly selected third-year undergraduates from 34 local colleges and are administered with the direct assistance of college authorities. They consist of an online psychometric test, enabling students to better assess their personality traits, and a short questionnaire on job outlooks, which provides policymakers with insights into students' perceptions of labor markets.

The second module of the questionnaire contains questions on occupational preferences and job search perceptions, which are consistently asked across the 2015 – 2017 waves.

Q1. How do you view the current job market for college graduates?

1. Favorable
2. Stable
3. Somewhat challenging
4. Very challenging

Q2. What factors most influence your view of the job market?

1. Weak job market, concern about not finding a job
2. Record number of graduates, intense competition
3. Unfamiliar with the job market, worried about finding a suitable position
4. Concern that my qualifications/major/skills may not meet employer requirements
5. Confident in my own abilities
6. Economic recovery is driving demand and creating more opportunities

Q3. What type of employment do you prefer?

1. Traditional employment (government agencies, public service programs, state-owned enterprises, foreign companies, private firms, etc.)
2. Emerging employment forms (e-commerce, online education, digital entertainment, gig platforms, freelance writing, etc.)
3. Starting my own business
4. Other

Q4. What is your first-choice type of employer?

1. Government agency
2. Public institution
3. State-owned enterprise (administrative/public service)
4. State-owned enterprise (commercialized)
5. Private company
6. Foreign company
7. Entrepreneurship or other options

Q5. How do you assess your likelihood of securing your ideal job following graduation?

(*1 = Not confident at all – 10 = Very high*)

Q6. Are you preparing for or currently applying to positions in the public sector?

1. Civil service exam
2. Recruitment for public institutions
3. Recruitment for state-owned enterprises
4. Other related recruitment
5. None

Q7. Would you be willing to work at the grassroots level or in small and micro businesses?

(*1 = Not willing – 5 = Very willing*)

Q8. When choosing a job, how important are the following factors to you?

(*For each item: 1 = Not important – 5 = Very important*)

1. Contributing to national development
2. Industry prospects
3. Salary and benefits
4. Alignment with my major and skills
5. Job security
6. Location
7. Incentives such as residency ("Hukou") status or subsidies
8. Type of organization
9. Opportunities for personal growth
10. Work-life balance
11. Family's opinion

Q9. What difficulties do you expect to face in the job search?

(*For each item: 1 = Not important – 5 = Very important*)

1. Limited access to job information
2. Lack of job-search skills
3. Attending a less prestigious university
4. Ordinary academic qualification (e.g., associate degree)
5. Niche major with low demand
6. Oversupply of graduates in my field, tough competition, high barriers
7. Weak professional or technical skills
8. Lack of internship or practical experience
9. Limited personal connections/networking
10. Employment discrimination (gender, ethnicity, region, etc.)
11. Unclear career goals
12. Limited knowledge of job/entrepreneurship support policies
13. Overhigh personal expectations

[Back to Page 22] [Back to Page 24]

Appendix C.5 Household Panel Outcomes

The full set of questionnaires of the China Education Panel Survey (CEPS) is available via its official website.[33] Here I present key questions used in this paper.

[33] http://ceps.ruc.edu.cn/__local/C/67/E7/F7BA6FBDFBC3C8B808AA0F6E0FA_905BECDA_7A84E.pdf?e=.pdf.

1. Measuring Household State Job Preferences

[Parent, A30; Child, B19] *Ideally, what kind of occupation would you expect* [*your child*] *to do in the future? (1) Government official; (2) Business owner or manager; (3) Scientist/engineer; (4) Teacher/doctor; (5) Designer; (6) Artistic performer/actor/host; (7) Athlete; (8) Technical worker; (9) Other; (10) I don't know.*

To measure household preferences for state jobs, I construct a binary variable equal to one if both the student and their parents indicate such a preference. In the Chinese context — where education and health sectors are dominated by the state — I classify households reporting (1), (3), or (4) as "preferring state jobs", and those reporting only (1) as "preferring government official positions". The empirical results are robust to using either measure.

The questions are available from all waves of the survey.

2. Measuring Ex-ante Student Traits

[Parent, A27-28] *To what extent your* [*child's*] *behavior is consistent with the following dimension described (1, never - 5, highly)? (1) Helping disadvantaged; (2) Sense of discipline (e.g., lining up consciously); (3) Sincerity (being sincere and honest toward others), (4) Nonviolence (language violence, fight, or bullying); (5) Having a mild temper toward others; (6) Staying focused; (7) Truancy; (8) Cheating and plagiarism; (9) Delinquent behavior or others (e.g., violating student codes, such as going to adult cybercafes).*

I create an equally weighted z-score index by averaging these scores, which serves as a direct proxy for child integrity and prosociality perceived by their parents. To maintain consistency, I reverse the responses to (7) – (9) in the z-score calculation, with a higher score indicating greater prosociality and behavioral ethics.

Household socioeconomic status indexes and student cognitive scores are prepackaged by the CEPS research team and can thus be directly utilized in the analysis. All trait variables are only available from the first wave of the survey (before the treatment), making them time-invariant and suitable for identifying the talent allocation mechanism.

[Back to Page 22]