Corruption and Human Capital: Evidence from China

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Abstract

Corruption may disincentivize productive activities and undermine human capital. This paper examines how reducing corruption reshapes talent allocation and accumulation. I do so by exploiting China's staggered anti-corruption inspections that dampen perceived returns to rent-seeking sharply. Using a representative child-parent panel and unique personnel data from a provincial state-owned enterprise, I find that reducing corruption induces positive selection for integrity and public-mindedness into the state sector, which remains present even when conditioning on ability and family background. The results hold for both occupational preferences and realized labor market outcomes. I also show suggestive evidence that parents put greater emphasis on human capital accumulation following anti-corruption, increasing inputs in their children's education. Taken together, the allocation and accumulation margins may reinforce each other, collectively pointing to the short-run and long-run human capital costs of rent-seeking.

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1 Introduction

In the canonical work of Baumol (1990), corruption alters the relative returns to rent-seeking versus productive pursuits, which in turn may distort human capital allocation and accumulation (see also Murphy, Shleifer and Vishny (1991, 1993)). In this framework, rent-seeking may be self-reinforcing (and social welfare reducing), as society settles into a "rent-seeking equilibrium" in which individuals invest in rent extraction, which can further reduce the returns to productive human capital, thus impeding long-run growth (Acemoglu, 1995). Despite the well-established theoretical literature, there is little empirical evidence on how rent-seeking opportunities causally impact occupational preferences and human capital development – which is of relevance both to researchers as well as policymakers.

This paper studies the impact of China's corruption crackdown on household talent allocation and accumulation. Since 2013, the country has implemented staggered anti-corruption visits by dispatching inspection teams to local governments. These visits create a heightened awareness of potential corruption risks, thereby generating sharp variation in perceived returns to rent-seeking across regions and time. This nationwide program with fine-grained variation allows me to provide a credible measure of how rent-seeking opportunities affect career aspirations and human capital investments across an entire country. Exploiting representative household panels, I am able to observe household occupational preferences, educational investment, as well as their children's ability and integrity. The personnel data from a large-scale state sector organization further provides realized occupational outcomes.

I begin by the reality check that anti-corruption visits are associated with lower perceived returns to (or greater costs of) rent-seeking. Using a nationally representative survey conducted by the Chinese Academy of Social Sciences, I find that residents perceive greater anti-corruption enforcement after their city has been inspected. Combining this observation with findings from placebo tests validates the staggered anti-corruption visit as the source of variation in returns to rent-seeking.

My main analysis explores how the corruption crackdown shapes talent allocation, holding the human capital stock static. Motivated by the literature on political selection, I focus on selection into the state sector, where individuals can significantly influence resource allocation and thus have more opportunity to extract rents (Krueger, 1974). The first set of analyses investigates household occupational preferences by utilizing the Chinese Education Panel

Survey (CEPS), a representative panel of middle-school student-parent pairs since 2013.[1] Importantly, the survey elicits information about children's misconduct from the responses of children and their parents separately, the inconsistency of which enables identification of a group of households with particularly dishonest children. The survey also includes measures of children's cognitive ability, public-mindedness, and household socioeconomic status.[2] I can therefore explore how occupational preferences for public jobs respond to changes in reward structures, as a function of integrity and ability – the key talent traits that speak to the current discussion on the personnel economics of the state (e.g., Finan, Olken and Pande, 2017; Hanna and Wang, 2017; Besley et al., 2021).[3]

Leveraging the child-parent panel, I observe no significant impact of anti-corruption visits on the average occupational preference for state sector jobs. However, I find substantial differences as a function of children's integrity. In particular, the triple-difference estimates show that households with children with relatively high integrity and public-mindedness scores reveal a greater preference for public jobs following anti-corruption inspections, compared to relatively dishonest types; this result is unaffected by the inclusion of controls for child ability and family background. I do not find that inspection visits are associated with differential selection on ability (proxied by children's academic and cognitive performance). These findings speak to the insight of Hanna and Wang (2017) that screening on ability may not alter the selection problem in the state sector. Instead, my results suggest that sharply dampening perceived returns to rent-seeking and reshaping social norms surrounding corruption may be helpful to improve selection into public service.

To move beyond not-yet-realized preferences, the second set of my main analyses utilizes applicant and personnel evaluation data from a provincial state-owned enterprise (SOE). For applicant traits, I am able to observe the mean honesty scores of applicants by their hometown and college cities each year.[4] For employee traits, the data includes all administrative employees recruited by the headquarters since 2007, and the evaluation reports comprise assessments of their probity and work effectiveness in the fiscal year 2020. To identify how changes in rent-

[1] Exploiting not-yet-realized occupational preferences can largely alleviate the concern over confounding factors. For instance, a shift in the number of civil servant applicants or their entry scores can be driven by demand-side changes (e.g., recruitment criteria) or short-term uncertainty (e.g., reshuffle of department leaders due to investigations).

[2] All measures of these traits (integrity, ability, and family background) were based on the survey prior to anti-corruption shocks and are thus time-invariant.

[3] As noted by Hanna and Wang (2017), the variation in observed political corruption in developing countries may in part be driven by selection (specifically, they show experimental evidence that individuals with a greater propensity for dishonesty are more likely to enter the state sector in India). It is therefore important to understand how changes in reward structures (e.g., decreased returns to corruption) shape the types of individuals who select into public jobs.

[4] Due to confidentiality of personality test results, I can only access the mean honesty score of applicants by the hometown/college city $\times$ year cell at the time of this study. For employee traits, I am able to obtain individual-level information and thus control for demographic characteristics.

seeking opportunities affect the supply of state employees, I exploit the variation generated by anti-corruption visits to individuals' hometowns or cities of college attendance ("college cities"). Specifically, I compare the traits of individuals applying to/entering the organization *before* their hometown/college cities have been inspected to that of individuals applying to/entering the organization *after* their hometown/college cities have been inspected, holding the recruitment margin constant.[5] Consistent with unrealized preferences, individuals from region-year cells that experienced an anti-corruption visit but still preferred the SOE score higher on integrity and public-mindedness, while there is no significant difference in work effectiveness.[6] A complement analysis based on non-administrative track applicants confirms that the results are not primarily driven by a sharp increase in the whole population's honesty in the city inspected. I also provide additional discussions to rule out the confounding roles of career uncertainty and socioeconomic conditions.

To establish the broader implications, the final piece of my empirical analysis incorporates the idea of endogenous growth models, allowing household investment to respond to changes in corruption (Ehrlich and Lui, 1999). Given human capital accumulation is typically long-term and provides poor collateral, households may have less incentive to invest in productivity when corruption is rampant (Pecorino, 1992; Murphy, Shleifer and Vishny, 1993). I provide suggestive evidence that reducing corruption may induce greater human capital investment. Anti-corruption visits are associated with an average 0.3-years increase in desired educational attainment, plausibly featuring increased expected returns to human capital. I also observe behavioral changes: parents increase investment in education and focus more attention on their children's human capital. Although speculative, these results conform to the canonical theory that proposes the potential long-term human capital gains of countering corruption.

Taken together, corruption can hamper productive human capital by inducing negative selection on incentives into the state sector and reducing talent stock across all sectors over the long term (Murphy, Shleifer and Vishny, 1993; Acemoglu, 1995). The allocation and accumulation margins may likely reinforce each other and collectively impede sustainable growth.

This paper's findings contribute most directly to the literature on the consequences of corruption for human capital. A well-documented empirical channel through which corruption affects human capital is misallocation of public resources (Mauro, 1998). While the primary focus of such inquiries is the indirect course via which distorted incentives of politicians and bureaucrats undermine the delivery of education and health services (Gupta, Davoodi

[5]The identification strategy here – exploring variation in "origins" instead of "destinations" – is to some extent in the spirit of Fisman and Miguel (2007) and cultural economic liteature, which can help eliminate confounders induced by the external environment and demand-side changes.

[6]The compatiable changes in applicant traits and employee traits also add to the limited work bridging how endogenous changes in the applicant pool quality affect the quality of the final hire (Besley et al., 2021).

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; Zamboni and Litschig, 2018), I concentrate on the more direct channel via which corruption alters incentives for individuals to accumulate and allocate human capital. My work thus provides causal evidence speaking to the long-standing theoretical literature concerning rent-seeking and talent formation (Baumol, 1990; Murphy, Shleifer and Vishny, 1991; Acemoglu, 1995; Ehrlich and Lui, 1999).

Numerous empirical studies have examined how rent-seeking undermines productivity. This strand of work has focused almost exclusively on misallocation across firms and their innovation activities (e.g., Paunov, 2016; Xu and Yano, 2017; Haveman et al., 2019; Fisman et al., 2021; Kong and Qin, 2021). Fisman et al. (2018) make progress by documenting distortions in knowledge production caused by hometown favoritism in Chinese Science. This paper delves into a micro-level scenario in which productivity has not yet been realized, by examining corruption's role in shaping the human capital of children – an important source of constant economic growth over the long term.[7] In this regard, the study most relevant is that of Ajzenman (2021), which shows that corrupt leaders erode the civic values of students in Mexico.

By documenting that anti-corruption induces positive selection for integrity into public sector organizations, my study also complements the flourishing work on the personnel economics of the state (e.g., Finan, Olken and Pande, 2017; Dal Bó et al., 2017; Besley et al., 2021). Existing literature has demonstrated that the selection of talent traits can be an important determinant of public sector performance, leveraging cross-sectional variation in experimental settings (e.g., Hanna and Wang, 2017; Barfort et al., 2019; Bai et al., 2021; Gans-Morse, 2021).[8] However, the lack of variation *within* a society makes it challenging to know how different social norms and reward structures affect the types of individuals who select into the state sector. This paper makes progress by exploting staggered anti-corruption inspections and rich micro-level data to understand how changes in rent-seeking returns reshape selection into public service.[9] The finding is relevant, as negative selection into the state sector may serve as an aggravator to spur further corruption (Acemoglu, 1995; Ajzenman, 2021; Chen

[7] The result on children's human capital accumulation also speaks to the notion of Cao (2022) that the costs of corruption may be latent and hard to observe.

[8] Hanna and Wang (2017) and Gans-Morse (2021) show a negative selection of rent-seekers into public service in Indian and Ukrianian contexts, in which corruption is pervasive. In contrast, Barfort et al. (2019) document a positive self-selection of honest types in Denmark, one of the world's least corrupt countires.

[9] In this regard, the work by Lai and Li (2021) is relevant but focuses on a somewhat distinct aspect: they exploit the central government inspections on leading departments and document a decrease in the aggregate number of applicants to departments inspected in the national-level civil service exam. However, they lack information on applicants' talent traits, and they also lack individual preferences for other public jobs (as the national positions only account for about 14% of the total civil servant job openings). The representative household sample here allows me to evaluate based on a more general population, with a focus on the selection by talent traits.

et al., 2022), which further undermines human capital.[10] Accordingly, policymakers should be conscious of whether the risk of deterring "capable" rent-seekers or generating economic shocks induced by curtailing rent-seeking opportunities (e.g., via anti-corruption campaigns) outweighs the potential human capital benefits in the long term.[11]

Finally, this paper echoes the recent work in labor economics linking perceived returns to actual human capital outcomes (Arcidiacono et al., 2020; Gong, Stinebrickner and Stinebrickner, 2020; Kuka, Shenhav and Shih, 2020; Wiswall and Zafar, 2021). I find that parents' expected returns to human capital and rent-seeking indeed translate into behavioral changes, reshaping investment in their children's education.

2 Background

As in the case of any other economic activity, individuals and households in China respond to the prospects of rent-seeking relative to productive pursuits by modifying talent allocation and accumulation (Kuka, Shenhav and Shih, 2020). Meanwhile, the Chinese setting features a relatively strong responsiveness to rent-seeking opportunities and child-rearing issues (Li, 2011; Zhu and Chang, 2019), suggesting that a sharp corruption crackdown can be noticeably large in magnitude to identify.

2.1 Human capital and rent-seeking in China

Talent allocation and China's state sector. A long history of *Keju* (imperial civil exam system) culture and strong intergenerational solidarity in China have embedded preferences for state sector jobs and a parenting style focusing on children's achievement (Chen, Kung and Ma, 2020; Zhang, Qin and Zhou, 2020). Accordingly, many talented Chinese individuals prefer the state sector, an outcome with mixed implications for economic development (Bai et al., 2021). Compared with private sector jobs, state sector positions in China before Xi's anti-corruption campaign were generally associated with better welfare and plentiful discretionary power in resource allocation, making them particularly attractive to rent seekers.[12] Although

[10] The presence of a corrupt public sector induces allocative inefficiency and encourages rent-seeking norms among citizens. This may disincentivize human capital accumulation, leading to a greater supply of rent-seekers, which in turn can aggravate the negative selection into public service and produce even more corrupt government officials, which yield further negative externality on human capital.

[11] As noted by Dal Bó, Dal Bó and Di Tella (2006), in some circumstances, a temporary crackdown might take the system from a bad equilibrium to a good one, permanently improving matters.

[12] As highlighted by the official anti-corruption documentary series "Zero Tolerance" issued in 2022, officials engaged in rent-seeking and misconduct can enjoy tremendous perks, get all their whims pampered and live a privileged lifestyle that even money can not buy. See for example https://www.nytimes.com/2022/02/09/business/china-corruption-documentary.html.

the recruitment of the Chinese bureaucracy puts a great emphasis on screening ability – which has benefited the rapid growth of state capacity in recent decades (Bell, 2016; Yao, 2018) – the negative selection of rent-seekers remains a challenge for the Chinese Communist Party (CCP) (Liu, 1983; Manion, 2004; Jiang, Shao and Zhang, 2020).[13]

Rent-seeking vs. human capital. While substantial inputs in education have benefited human capital development in modern China (Heckman, 2003), it has also produced severe anxiety for households living under the specter of unprecedentedly fierce competition. As human capital accumulation requires long-term investment and provides poor collateral, it provides rent seekers with plenty of opportunities for expropriation (Murphy, Shleifer and Vishny, 1993). In an institutional setting in which corruption is pervasive, rent-seeking can thus lower expected returns to productive activities and crowd out talent investment (Ehrlich and Lui, 1999). For instance, it is widely acknowledged that being connected to powerful officials or managers can considerably increase an individual's chance of obtaining a desirable internship or job in China, even in the case of a relatively weak profile. A vast literature has also documented the role of social ties in Chinese political economy (e.g., Fisman et al., 2018; Kung and Ma, 2018).

Underpinning the need for engagement in rent-seeking reflects the long tradition of *guanxi* – developing and maintaining social relationships for favor-exchange. Responding to fierce competition in China, citizens may tend to engage in rent-seeking activities to ensure their upward mobility: some parents spend heavily on Teachers' Day gifts to ensure teachers give "special attention" to their children, recruitment officers elicit bribes by making exceptions or exchanging majors via the independent college recruitment system,[14] and millions of candidates (certainly not all of them) sign up each year for the cut-throat exam to join the bureaucracy to enjoy the welfare benefits and rent-seeking power it can provide. Notably, as anecdotal evidence demonstrates, even if many households find it improper to offer expensive gifts or enter the state sector for potential rent, they feel forced to do so because others do.[15]

This aligns with the canonical theory on rent-seeking versus productive activity, which considers corruption to render the society as a whole more vulnerable to expropriation and

[13] China was ranked 80th in 2013 in terms of the Corruption Perception Index (CPI), according to *Transparency International*. For rent-seeking returns in Chinese bureaucracy, see for example: Cai, Fang and Xu (2011), Chu, Kuang and Zhao (2019), Chen and Kung (2019), and Fang, Gu and Zhou (2019).

[14] For instance, Cai Rongsheng, the former Director of Student Recruitment Offices at the Remin University of China (among China's top universities), was arrested for corruption and illegally contravening regulations during the independent recruitment process in 2015. Hu Juan, the acting principal, and Ji Baocheng, the school's then-president, were also inspected.

[15] Even if a household does not intend to extract rent, a public sector occupation can make it less vulnerable to expropriation. See for example https://www.chinadaily.com.cn/china/2014-10/21/content_18779200.htm. This widely-held perception echoes the negative externality of rent-seeking behaviors (or the well-known corruption-as-equilibrium framework). Relatedly, using global firm-level data, Fisman et al. (2021) document that non-bribers are often excluded entirely from growth opportunities in high-corruption settings.

to lure away talent (Acemoglu, 1995). Consequently, households are incentivized to allocate some of their talent and resources to rent-seeking to either obtain extra favoritism or to prevent their children from being left behind, skewing human capital allocation and investment.[16] While the deep-rooted *guanxi* culture might be specific to the Chinese context, these facts, however stylized, are likely to reflect the general insight that rampant corruption distorts reward structures and encourages talent to engage in rent-seeking (Murphy, Shleifer and Vishny, 1993; Ehrlich and Lui, 1999).

2.2 Anti-corruption inspection visits

China's Anti-corruption campaign. The anti-corruption campaign was initiated after the conclusion of the 18th National Congress of the CCP at the end of 2012, when Xi Jinping took power.[17] In his inaugural speech, President Xi made special mention of corruption, emphasizing it as a great threat to the Party's survival. The harshness of the crackdown was both unprecedented and unanticipated. Before Xi's era, few senior officials were investigated,[18] making Xi's campaign notable for breaking the unspoken rule regarding the immunity of (former) national leaders that had persisted since the post-Mao-Tse-tung era. By the end of 2020, 394 provincial (and higher) ranking officials had been investigated, contrasting starkly with the 151 investigated between 1993 and 2012.[19]

The inspection visit institution. The inspection visit institution contributes prominently to the top-down anti-corruption campaign. The institution (the *Leading Group for Inspection Work*) was formally established in November 2009 to complement local internal control, especially the supervision of local senior officials. However, not until the re-centralization of Xi and Wang (the head of the *Central Commission for Discipline Inspection*, Xi's most important ally) in 2013 did the inspection institution gain special significance. A powerful tool in Xi's corruption crackdown, this organization dispatches inspection teams to local governments and public sector organizations to conduct intensive audits and inspections to derive insights

[16] A vivid example is the "identity theft" scandal impacting earlier college enrollments. In June 2020, Chinese media revealed that 242 graduates from several universities in Shandong province had stolen the identity and college entrance exam score of another person to enroll themselves between 1999 and 2006. In most cases, the victims did not realize their enrollment had been taken, with the imposters' families investing substantially to bribe education authorities and school managers to help children with poor academic performance to enter better colleges. See for example https://www.theguardian.com/world/2020/jul/07/china-to-criminalise-college-exam-after-identity-thefts.

[17] On December 6th of that year, the Deputy Party Secretary of Sichuan Province was abruptly investigated, marking the beginning of Xi's massive anti-corruption campaign, the largest corruption crackdown in recent history.

[18] Investigation (announced publicly), arrest, and conviction are in essence synonymous in the Chinese context. The discipline committee will only formally investigate someone in the presence of solid clues and evidence.

[19] The investigation of Zhou Yongkang, a former member of the Politburo Standing Committee (the seven-to-nine-member top CCP leadership committee), represents the most influential case. The crackdown has also led to accusations against several military generals and other Politburo members.

regarding corruption and misconduct at different timings.

Particularly relevant to the identification strategy, the inspection visit institution contributes to the staggered variation in returns to rent-seeking for local households. To exploit finer variation and mitigate the confoundedness of purges, I consider the first anti-corruption inspection visits on the prefecture (city) level, exploiting inspections conducted by the *Provincial Commission for Discipline Inspection* (PCDI).[20] The PCDI, a province-level institution, supervises the CCP members within its jurisdiction.[21] Xi's anti-corruption institution dictates that the PCDI dispatches inspection teams to prefecture-level and county-level local governments. According to the *Rules for CCP Inspection Work*, the list of selected regions is confidential before the PCDI is notified, with selected prefectures informed 10 days before the inspection. Members of inspection teams are appointed only temporarily, avoiding potential conflicts of interest. Inspection teams are responsible for extracting information and potential evidence rather than handling cases directly. That is, they "audit records and documents from local CCP committees; participate in disciplinary meetings; manage information provided by whistle-blowers and local residents; organize forums to gather public opinions of local officials; individually interview with local bureaucrats; [and] spot-check to obtain better understanding of local corruption issues".[22] To date, the inspection visit institution remains the most prominent instrument for anti-corruption in China and has even been adopted by other countries (e.g., Vietnam and Cambodia).

A large body of qualitative evidence demonstrates that anti-corruption visits sharply deter rent-seeking. For example, reports and documentaries released by the Discipline Commissions and think tanks highlight both the unexpected harshness of anti-corruption investigations and the decline of rent-seeking norms:

> "Before the inspection visit, most cadres held optimism bias, reading it as a scarecrow. However, the inspection and rectification indeed deterred every local official. " (*Central Commission for Discipline Inspection*, 2014)

[20] Compared to the center-to-province inspection visits of which the timing only varies at the provincial level and that focus primarily on "big tigers" (e.g., Chen and Kung, 2019), the newly digitized PCDI inspection data enables me to exploit the variation at a much lower and concrete level that has a direct bearing on local residents.

[21] 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.

[22] See https://www.wsj.com/articles/BL-CJB-28767 for details. The inspection teams typically "publicize their contact information and install a 'petition box' to encourage individuals with information on corruption and disciplinary violations to report through letters, phone calls, or face-to-face meetings. Inspection teams may also conduct personal interviews with current and former state personnel members, especially the leading cadres. In addition, they may review each organization's documents, audit its accounts, sit in on its meetings, and issue questionnaires among its staff. If necessary, they can also bring in other agencies with expertise in specialized areas to assist their visits."

> "I thought I might be able to slip through the inspection visit, yet I was wrong." (Zhenchao Yang, Former deputy governor of Anhui Province, in the documentary *Sharp Sword of Inspection*, 2017)

> "A survey conducted by the Canton Public Opinion Research Center shows that more than 80% citizens perceive a lower need to engage in *guanxi* and gift-giving after the corruption crackdown." (China Youth Daily, 2014)

The evidence is consistent with the findings of Francois, Trebbi and Xiao (2016) and Lorentzen and Lu (2018) that, apart from the intention of consolidating power, the anti-corruption campaign, at least to some extent, features a sincere attempt to reduce corruption. I later provide quantitative evidence buttressing the idea that inspection visits lower the tendency towards rent-seeking.

3 Data and Empirical Strategy

3.1 Data and measures

The empirical analysis combines digitized anti-corruption visit data with nationally representative surveys. Here, I briefly describe the data to be used, with variables of interest detailed as they become relevant.

Corruption crackdown. I conduct a large-scale data digitization to construct a city-level anti-corruption visit dataset for the period 2012-2018. The unit is the *city-year-month* spell. The timing of each province-to-city inspection 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. Figure 1 visualizes the timing of the first inspection visit to each city.

China Education Panel Survey (CEPS). The CEPS is managed by the National Survey Research Center at Renmin University of China. Based on a stratified, multistage sampling design, the CEPS comprises a middle-school-based, nationally representative sample of approximately 10,000 student-parent pairs from 112 schools in 28 prefectures in mainland China. Each child-parent pair is surveyed annually (in either the fall or spring semester), and I have access to two waves of data between 2013 to 2015. To exploit fine-grained variation, my empirical analysis treats this balanced annual panel as an unbalanced panel featuring four time points (i.e., Fall 2013, Spring 2014, Fall 2014, Spring 2015).[23] Importantly, the CEPS elicits

[23]For example, one household might have been interviewed in Fall 2013 and Fall 2014, and another household might have been interviewed in Fall 2013 and Spring 2015. We may alternatively regard this structure as an augmented version of pooled cross-sectional data with four points in time. The corresponding identification assumption is discussed in the next section.

occupational preferences and beliefs about returns on education from both parents and their children in each wave. It also collects information on household investment in education (pecuniary and non-pecuniary), children's academic performance and public mindedness, and other demographic characteristics. The responses from children and their parents are elicited separately.

Applicant and employee data from a state-owned enterprise. A limitation of the CEPS data is that we do not observe realized occupational choices. To overcome this issue, I utilize applicant data and performance evaluation reports from a large-scale provincial state-owned enterprise (SOE) in southern China to complement.[24] For applicant traits, I can observe the mean honesty score of applicants for each hometown/college city $\times$ year cell since 2011. The honesty score of each applicant is automatically assessed by the personality test system on a 12-point integer scale, where 1 is the lowest and 12 is the highest.[25] For employee traits, I can observe individual-level assessments of one's integrity and work effectiveness in the fiscal year 2020, each of which is scored on a 4-point integer scale, where 1 is the lowest and 4 is the highest. The assessment is based on feedback from managers, colleagues, and performance appraisal systems that follows uniform evaluation criteria. I compile the evaluation reports of all 434 administrative employees (i.e., bureaucrats in public organizations) recruited between 2007 and 2020 by its headquarters and link this sample to inspection visit data of employees' hometown and college cities.

Complementary data sources. The Chinese Social Survey (CSS) and the Chinese Family Panel Studies (CFPS) are also used in my analysis. The CSS is a nationwide biennial survey project initiated by the Chinese Academy of Social Sciences, which includes about 7,000-10,000 individuals in 151 sampled counties. I pool its 2011, 2013, 2015, and 2017 waves, which elicit perceived anti-corruption harshness from respondents. The CFPS is a nationally representative, biennial longitudinal survey of Chinese families and individuals conducted by Peking University. It includes interviews with a total of 14,798 households in 162 counties, eliciting responses from every individual in those households. I use its data from 2012 to 2016 (three waves) on household expenditures to examine changes in investment in social capital and gift-giving.[26] Given the exact month of an interview is available, I again treat this biennial panel as an unbalanced panel using the *respondent-year-month* unit.

[24]The total number of employees of this SOE is about 20,000 in 2020 (about 16% are administrative employees/bureaucrats). The population and GDP of the province are comparable to that of Poland.

[25]This honesty score is calculated based on each applicant's responses to their online personality test, using several psychological techniques to indirectly validate the consistency of their answers in different scenarios. Due to the confidentiality of personality test results, the author cannot access the individual-level applicant data at the time of this study.

[26]The 2018 and later waves of the CFPS are not used in my analysis according to the recent advancement in difference-in-differences literature: since all cities have been inspected by the end of 2017, there are no not-yet-treated control groups in the 2018 and later waves.

3.2 Empirical strategy

Building on the recent routine, I estimate the following stacked-by-event difference-in-differences (DiD) specification[27]:

$$y_{hsct} = \beta * Inspected_{st} + \lambda_{sc} + \tau_{tc} + \varepsilon_{hsct} \quad (1)$$

where y_{hst} is the outcome of interest; h indexes the household (or individual), c is the treatment cohort, s is the prefecture of residence, and t is the time of interviewing[28]. Specifically, I create a separate dataset for each point in time, which I call a "treatment cohort" (c). Cities that have received their first anti-corruption visits at that time are labeled "treated" ($Treated_{sc}$=1); cities that have not yet been inspected serve as clean controls ($Treated_{sc}$=0). For instance, for the cities that experienced their first inspection visits in Spring 2014, I use all cities that are yet to be inspected as controls, calling this synthetic dataset the "Spring 2014" cohort. Accordingly, we can generate a cohort-specific event time dummy $Post_{ct}$ that is 1 for observations in and after Spring 2014. I then stack all datasets into one and merge that dataset with household observations.

$Inspected_{st} = Treated_{sc} * Post_{ct}$ is the DiD term that 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 effects of the anti-corruption visit across different treatment cohorts. The *cohort-time* fixed effects, τ_{tc}, absorb all unobserved common temporal shocks. The standard DiD specification also requires the *city-cohort* fixed effects, λ_{sc}, to absorb any time-invariant differences in city characteristics (given the city represents the identifying source of staggered variation). The longitudinal nature of the CEPS and CFPS data enables further introduction of λ_{hsc}, the *city-household-cohort* fixed effects. Exploiting within-household variation largely alleviates the threat of compositional changes in household characteristics before and after the anti-corruption visit within a city.[29] Finally, I allow idiosyncratic error terms, ϵ_{hsct}, to be correlated at the city-cohort level, corresponding to the level of treatment.

To investigate the effects of reducing corruption on talent allocation, I further interact the

[27] For econometric discussions, see De Chaisemartin and d'Haultfoeuille (2020), Borusyak, Jaravel and Spiess (2021), Baker, Larcker and Wang (2022), Callaway and Sant'Anna (2021), Goodman-Bacon (2021), and Sun and Abraham (2021). For empirical studies using stacked-by-event regressions, see, for instance, Deshpande and Li (2019), Cengiz et al. (2019), and Vannutelli (2021).

[28] In the CEPS, the unit of t is the semester; in the CFPS, the unit of t is the year-month; in the CSS and the SOE analysis, the unit of t is year.

[29] One additional concern is the self-selection of residency. The Chinese household registration institution imposes strict limits on changing permanent residency, with students only eligible to attend primary and junior-high schools at the location of their permanent residency. Accordingly, estimates holding the *household-prefecture* constant (using λ_{hsc}) are highly comparable to those using only household fixed effects (using λ_{hc}).

DiD term with children's traits to estimate the following triple-difference specification:

$$\begin{aligned} y_{hsct} =& \beta_0 * Inspected_{st} + \beta_1 * Treated_{sc} * \overline{Trait}_h + \beta_2 * Post_{ct} * \overline{Trait}_h \\ &+ \beta_3 * Inspected_{st} * \overline{Trait}_h + \lambda_{hsc} + \tau_{tc} + \varepsilon_{hsct} \end{aligned} \tag{2}$$

where β_3 is the triple difference term of interest (note that $Inspected_{st} = Treated_{sc} * Post_{ct}$). $\overline{Trait}_h$ is a pre-determined proxy for the child's characteristic (e.g., integrity or ability).

3.3 Identification assumption and threats to identification

The key identification assumption is the absence of differential trends between sampled households 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 interview. This study setting does not require the timing of inspection visits to be randomized. Instead, the primary concern is whether the timing of receiving the first anti-corruption visit correlates with other determinants of changes in talent allocation and accumulation over time. For instance, if the survey interviewed only households with more human capital following the anti-corruption shock, then the DiD on human capital investment would likely yield upward-biased estimates. Qualitatively, the timing of the anti-corruption shock appears to be not systematically correlated with the interview timing and thus the evolution of household characteristics. As noted, the timing of visits is not available to the public ex-ante. Meanwhile, the exact timing of each school's survey roll-out is determined several months earlier on the basis of negotiations between the research team and school leaders.

Beyond qualitative arguments, I provide two sets of quantitative evidence. First, because my analysis employs pooled cross-sectional data (CSS) and treats the CEPS and CFPS as unbalanced panels with finer points in time, I can explicitly test the compositional changes in household characteristics. The exercise is essentially a placebo test that assuages the possibility that our estimated effects are purely driven by changes in demographic features before and after a city's anti-corruption shock.[30] Table 1 provides descriptive statistics for household/individual characteristics and their conditional differences. Holding constant the city and timing of the interview, I do not observe respondents interviewed after the anti-corruption visit to differ systematically from those interviewed before the visit. The balance holds for all three surveys. Second, in the robustness check later, I take the advantage of the CEPS allow-

[30] If the characteristics of households interviewed before/after the anti-corruption visit are not comparable, then their talent allocation and accumulation may likely display differential trends.

ing multiple observations of a household. If the timing of anti-corruption inspections is not correlated with the evolution of household characteristics over time, we should expect that, conditional on city and time fixed effects, adding or omitting household fixed effects may not significantly alter the empirical patterns.

Timing of treatment and city characteristics. However, one may concern that the evolution of city-level characteristics may be simultaneously associated with the negotiated timing of the interview and the timing of the first anti-corruption visit. Table 2 examines whether cities treated at different times exhibit significant differences in a set of city characteristics in the pre-inspection period.[31] Columns (1) - (5) display the average levels and growth rates of outcomes by treatment timing. To test whether cities inspected at different times display significant differences in the pre-crackdown period, Columns (6) - (7) 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.[32] This balance corroborates the earlier observation that local households are unlikely to anticipate the exact timing of anti-corruption visits. I later show that my empirical findings remain robust when controlling for a host of city characteristics.

Pre-trends and anticipation effects. Most importantly, for each set of my empirical results, I utilize event study plots to examine whether outcome variables exhibit differential trends in the pre-inspection period. The absence of pre-inspection trends serves as a necessary condition for assessing the plausibility of parallel trends.

A related concern is the anticipation (or spillover) effect (Malani and Reif, 2015) – residents of a not-yet-inspected city may anticipate an upcoming anti-corruption visit. Expecting lower returns on rent-seeking in the future, households may begin responding before the treatment. Conceptually, the anticipatory change is not particularly problematic in my setting, as it would likely drive the estimated effects towards zero. Empirically, we may examine the event study plots for continuously upward or downward time trends. Later, I also show that the estimated coefficients remain largely unchanged when controlling for inspection visits in neighboring cities, suggesting the limited role of spillover effects.

Confounding policy shocks. A special concern about exploiting China's corruption crackdown as a natural experiment is that the shock is accompanied by broader institutional changes in Xi's era, which might affect individuals in dimensions other than anti-corruption.[33] An advantage of exploiting the variation generated by province-to-city inspection visits is that treat-

[31] The absence of a significant difference in pre-treatment characteristics is a stronger test since the DiD only requires no counterfactual differential trends of the outcomes.

[32] I later also use event study plots to show that the timing of the anti-corruption visit is not systematically associated with the trend of local investigations in the pre-treatment period.

[33] This concern is particularly salient when simply adopting the year 2013 as the cutoff.

ment occurs in a highly staggered way (as shown in Figure 1), and any other broader shock is absorbed by *cohort-time* fixed effects. Moreover, the inspection visit institution is highly independent of other policies and local governmental affairs. In the next section, I conduct reality checks and placebo tests to empirically examine whether inspection visits truly alter the perceived returns to rent-seeking.

4 Empirical Results

4.1 Reality check: anti-corruption visits and returns to rent-seeking

I begin by checking that anti-corruption visits are associated with lower perceived returns to rent-seeking. As noted, concern may surround whether inspection visits genuinely generate first-order shocks to local corruption. While I provide qualitative evidence to describe the empirical background, this section focuses on quantitative evidence.

First, given the primary (stated) goal of inspection visits is to detect and dampen corruption, I compare the number of local officials arrested before and after the shock. Given some lower-ranking bureaucrats may be internally punished (thus not fully disclosed), I focus on the prefecture-level ranking officials, powerful 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 anti-corruption enforcement. Figure A2 further presents the corresponding event study plots using the stacked-by-event DiD specification.[34]

However, it is worth noting that citizens may not necessarily perceive the inspection as a sincere effort to deter corruption if the crackdown is highly politically oriented and that inspection visits may focus attention on the broader message of reforms, which have little to do with corruption. Conceptually, 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 suffer less from this concern.

To better examine whether inspection visits generate relevant shocks, I utilize the CSS pooled data, which feature a consistent measure of the perceived returns to rent-seeking in the perceived anti-corruption harshness outcome variable, where 1 is the lowest and 4 the highest.[35] Columns (1) - (2) of Table 3 present the DiD estimates, which associate inspection

[34]The absence of pre-trends also assuages the concern that the anti-corruption visit is triggered by the increasing trend in corruption due to non-observable characteristics in the first place.

[35]The corresponding survey question is "How do you rate the current anti-corruption work and its strength?"

visits with higher 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 that use visits from central government leaders since the beginning of Xi's term as the treatment. In the Chinese polity, it is common for top leaders to visit local municipalities following a leadership reshuffle to signal their reform agenda. None of my previous results are associated with these short-term visits, suggesting that the observed changes are unlikely to be driven by messages concerning broader institutional shifts unrelated to corruption or falsification (citizens simply say good about any political reform).

Finally, Figure 2 visualizes the estimated coefficients using the event study specification. I do not observe any discernible differential pre-trends. Moreover, the figures highlight the persistent changes in anti-corruption perception, with the effects remaining significant even three years after the first inspection visit. Later, I also discuss other possible channels (e.g. career uncertainty) to show that they are not likely to account for my findings.

4.2 Talent allocation: selection into the state sector

Talent allocation shapes both short- and long-term economic outcomes, which are central to understanding how corruption affects human capital (Murphy, Shleifer and Vishny, 1993; Acemoglu, 1995). This set of analyses focuses on occupational preferences for public sector organizations. The selection into the public sector (bureaucracy, state-owned enterprises, and public institutions), where individuals have plentiful rent-seeking opportunities and profound influence on resource allocation, is of particular relevance for not only the Chinese setting (in which the state sector prominently contributes to economic growth) but also the growing literature on the personnel economics of the state (Finan, Olken and Pande, 2017). Specifically, my analysis examines occupational preferences using the representative household panel to establish baseline results and strengthens the findings using the realized selection into a state-owned enterprise.

4.2.1 Measuring occupational preferences and traits

Household occupational preferences. The CEPS panel elicits occupational preferences from both parents and their children, providing an ideal opportunity for moving beyond aggregate labor market outcomes. An advantage of using occupational preferences is that it suffers less from short-term confounding shocks to labor markets, such as changes in selection processes and temporary uncertainty induced by reshuffles of department leaders due to investigations. Meanwhile, as noted by Wiswall and Zafar (2018), ex-ante beliefs about edu-

cation and career preferences tend to be systematically related to later life realized outcomes.

In the Chinese context, parental opinions play a non-negligible role in occupational choices. Therefore, my analysis utilizes an augmented dependent variable represented by a binary that is 1 if both the child and their parents prefer public sector jobs. This measure captures the idea that aligned occupational preferences increase the likelihood of that preference being realized.[36]

Motivated by the literature on political selection (Dal Bó et al., 2017; Finan, Olken and Pande, 2017; Besley et al., 2021), I introduce three proxies for child traits to better understand the implications on talent allocation: integrity, ability, and family background. All measures are time-invariant, based on responses from the survey prior to anti-corruption shocks.

Measuring integrity. A common challenge for measuring propensity to demonstrate rent-seeking behavior is that individuals may not reveal their integrity genuinely (Banerjee, Mullainathan and Hanna, 2012; Fischbacher and Föllmi-Heusi, 2013). To alleviate the falsification issue, I exploit the (in)consistency between the responses of children and parents. Specifically, I create a dummy that is 0 if the child ever denies academic misconduct (cheating/truancy) whereas their parent(s) admits. This measure benefits from the survey eliciting responses from children and parents separately, allowing us to identify a group of households with particularly dishonest children. In the Chinese setting, academic misconduct of students will be reported to parents seriously once detected. Given that children generally have more information on their past misconduct compared to their parents (Dizon-Ross, 2019; Bergman, 2021), if the parents admit their children's misbehavior, then their children should provide consistent responses if they are honest.[37]

Closely related to the integrity measure, the data also provide an auxiliary direct measure – the public mindedness index – that builds on parental responses to a set of questions evaluating the conduct and prosocial behavior of their children. Each dimension is scored on a 5-point scale, where 1 is the lowest score (poor) and 5 is the highest (excellent). I average these scores to create a time-invariant z-score index that acts as a complement proxy for agents' integrity and intrinsic motivation.[38] The index is positively associated with the aforementioned

[36] This augmented dependent variable also helps alleviate the concern over falsification, since the responses from children and their parents were elicited separately. I later show that the empirical pattern remains largely unchanged if the occupational preference of children or parents is used as the outcome variable.

[37] If both the parents and the child of a household admit that the child ever engaged in misconduct, then the child is still on average more honest than the falsification group. The empirical results remain comparable if we code the aforementioned case as "dishonest". However, I am keenly aware that the measure is still not perfect – if the parents and children both falsify, then these dishonest households will be assigned to the "honest" group ($Integrity$=1), which may likely drive the estimated effects toward zero. I later use the realized occupational choices in the state sector to help validate the findings on reported occupational preferences.

[38] The questions cover: helping the disadvantaged, sense of discipline, sincerity, nonviolence, emotional control, concentration, truancy, cheating, delinquent behavior and others. Appendix D.2 provides detailed descriptions.

indirect integrity binary (correlation=0.343, statistically significant at the 1% level), providing an internal validation.

Finally, one may be concerned that integrity may also be a function of rent-seeking opportunities (Ajzenman, 2021). Nevertheless, my analysis only exploits the time-invariant differences, aiming at capturing the *relative* propensity to engage in rent-seeking on average over time. Although reducing corruption might cause some dishonest children to behave more honestly, the occupational preference of a household may generally rely on the information of all past behaviors of its children. Consequently, the time-invariant integrity measure still captures the difference in children's traits developed from a very young age that affect household occupational choices.[39] The SOE sample analysis later further corroborates the discernable but limited role of the endogenous change in the whole population's integrity.

Measuring ability and family background. Agents' ability and family background represent two other key dimensions in the study of political selection (corresponding to competence and representativeness). The CEPS provides two proxies for children's ability: their academic performance (standardized grades in Fall 2013) and cognitive scores (language and math tests in the first wave survey). I construct an ability index by averaging the two scores, each of which captures some aspects of a child's talent.[40] Finally, I directly utilize the household socioeconomic status index (0 [low] to 5 [high]), which is prepackaged in the first wave of the CEPS.

4.2.2 Household occupational preferences for public jobs

Table 4 presents the main results. The estimated coefficient in Column (1) is indistinguishable from zero, suggesting that there is no significant change in the average preference for public sector jobs during the study period (2013 - 2015). Column (2) enables the impact to vary according to the children's integrity. Notably, given that integrity and ability in our setting are all in "relative terms", all the following estimates should be interpreted in a relative sense.[41] The anti-corruption inspection is associated with an 8% relative decrease in the probability of households with relatively dishonest children desiring public sector occupations, suggesting that reducing corruption deters the relatively dishonest type from entering (or motivates the honest type to select into) the state sector. Although the integrity index alleviates

[39] For instance, suppose child A was initially "honest" while child B was "dishonest", and that following the anti-corruption shock both A and B are now "honest" types. The occupational choices of the household will incorporate all the information available in the past so that A is still on average more "honest" than B in any period.

[40] The empirical results remain robust when using solely the cognitive score as the proxy.

[41] For instance, observations with $Integrity$=1 only indicate that they are relatively more honest than those observations with $Integrity = 0$ on average. However, we can not simply claim that individuals with $Integrity = 1$ are objectively "honest" types. Therefore, I focus on the interaction terms while interpreting the findings, which are not sensitive to whether we demean the variables or not.

concerns regarding falsification to some extent, the binary nature of the measure only allows us to identify a group of particularly dishonest children (9.9% of sample households). To exploit finer variation, I replicate the triple-difference estimation using the public-mindedness index as the mediator.[42] The results presented in Column (1) of Table B1 corroborate the positive selection for integrity induced by the anti-corruption visit.

In Column (3) of Table 4, I turn to selection on ability. The triple-difference estimate for the pre-determined ability index ($Inspected \times Ability$) is close to zero with no statistical significance, implying that the corruption crackdown has not generated differential responses between households with high-ability children and households with low-ability children in terms of occupational preferences for the state sector in the sample. Importantly, the estimated effect on selection for integrity remains virtually unchanged when controlling for ability. Additionally, Columns (3) and (6) of Table B1 allow the impact of reducing corruption on talent allocation to vary by family background.[43] Nevertheless, positive selection for integrity persists even when considering both ability and family background.

Finally, Columns (4) - (6) of Table 4 and Table B1 repeat the main exercise using a more stringent outcome variable: occupational preference for being a government official. All of the aforementioned empirical patterns remain robust. Table B2 also considers children's and parents' preferences separately, and the empirical patterns remain largely unchanged.[44] Figure 3 further presents the event study plots, alleviating the concern that our estimated effects on talent allocation are driven by trait-specific trends rather than anti-corruption visits.

These combined results suggest that rent-seeking can alter human capital allocation, with a sharp reduction in corruption improving the selection of incentives into public service. The empirical pattern also speaks to the speculation of Hanna and Wang (2017) that screening based on ability may neither exacerbate nor mitigate the problem of negative selection into the state sector. My findings suggest a shift in reward structures (e.g., reducing rent-seeking opportunities) may in part improve political selection in terms of integrity and public service motivation; and it may not necessarily depreciate the average quality of potential appointees

[42] As noted, the public mindedness index is positively associated with the integrity dummy and, generally, prosocial preferences and honesty are likely correlated (Hanna and Wang, 2017; Barfort et al., 2019). Thus, I do not pool them together in one regression to avoid the "bad control" problem.

[43] Although not particularly statistically significant, higher socioeconomic status is associated with a slightly stronger intention to join the state sector, producing a potential downside to representativeness induced by deterring corruption in the short term. This might be explained by individuals of lower socioeconomic status finding it harder to stand out in the state sector, which requires relatively long-term orientation (e.g., seniority and accesses to key decision-makers) and generates lower pecuniary returns to relieve financial burden (Jiang, Shao and Zhang, 2020; Meriläinen, 2022).

[44] Although the estimates are less precise, the impacts on parents' occupational preferences are more salient, which seems reasonable given that adults are more likely to be aware of the change in reward structures compared to their children.

to public organizations.[45]

4.2.3 Realized occupational choices

To assuage the concern over subjective reported outcomes (e.g., falsification), I corroborate my findings on talent allocation by examining realized labor market outcomes. To do so, I utilize the applicant and employee data from a provincial-level SOE between 2007 and 2020 (see Section 3.1 for details).

In the absence of a similar panel structure, I cannot simply replicate the triple difference strategy as noted earlier. Alternatively, I exploit the variation generated by the anti-corruption visit to an individual's hometown/college city (in which the individual attended college) to examine how reducing corruption shapes the selection into public organizations. Specifically, I compare the honesty and effectiveness of administrative employees (i.e., bureaucrats in the SOE) entering the organization *before* their hometown/college cities have been inspected to that of employees entering the organization *after* their hometown/college cities have been inspected, holding the year of entry and hometown/college city constant. If reducing corruption induces positive selection for integrity into public service, then we expect that individuals who have experienced anti-corruption shocks and still joined the SOE bear greater integrity. To rule out the possibility of endogenous city-year-specific recruitment changes, I further obtain the city-year level data on applicants applying for administrative positions (2011 - 2020), which contains a credible measure of integrity from the personality test regardless of whether an individual is recruited or not.[46]

Panel A of Table 5 begins with the city-year level applicant analysis. Columns (1) - (2) shows that the anti-corruption shock from one's hometown city or college city does not significantly alter the likelihood of applying for the public job (although the estimated coefficients are slightly negative). Nevertheless, we see that, holding the demand side constant, the individuals applying for administrative positions after their hometown cities have been inspected score an average of 0.873 points higher for their honesty in the personality test. The effect is more salient when examining anti-corruption visits to college cities, with an estimated coefficient of 1.031, statistically significant at the 1% level. This pattern is in line with the studies on the importance of experiences during impressionable years in shaping preferences

[45]The notion bears relevant policy implications, as screening on ability for government positions (e.g., by cognitive tests and meritocratic processes) remains common in many countries (Bagchi, 2007; Bell, 2016).

[46]Qualitatively, the SOE does not impose any city-year specific recruitment restrictions (as it is illegal per se). Nonetheless, one may still concern that, if the SOE headquarters learned that some cities are associated with severe corruption cases (as a result of anti-corruption visits), it may take extra caution and implicitly impose stringent requirements when recruiting people from those cities. Utilizing the applicant data helps alleviate this concern. Due to the confidentiality of personality test results, I can only access the applicant data at the hometown/college city $\times$ year level at the time of this study.

and behaviors (e.g., Krosnick and Alwin, 1989; Giuliano and Spilimbergo, 2014; Acemoglu et al., 2021). To probe whether these findings are driven by a sharply increased honesty of the whole population following the anti-corruption, Table B3 replicates the exercise by examining the applicant traits for non-bureaucrat positions of the SOE.[47] The positive selection on integrity largely concentrates around administrative positions, suggesting that the increased honesty of the whole population can not fully account for the observed empirical pattern.[48] Figure 4-A further presents the event study plots to corroborate the pre-treatment comparability. These results together confirm that reducing corruption motivates individuals with (relatively) greater probity to sort into public jobs.

Panel B of Table 5 further complements by using the individual-level administrative employee data, which allow us to directly and credibly examine the performance of individuals *within* the state sector. Columns (1) - (2) concern changes in employees' integrity and public-mindedness (as proxied by their performance on the "probity and discipline" entry), and the results again indicate a positive selection of incentives into public service. An advantage of the employee-level data is that it also contains a comparable measure of ability. In Columns (3) - (4), I do not observe discernible differences in the "work effectiveness" entry, suggesting no significant impact on selection on ability.[49]

Together, although specific to the sample under study, the changes in realized occupational choices are consistent with those previously documented concerning household preferences for public organizations: reducing corruption improves talent allocation by inducing positive selection for integrity (even if conditioning on selection for ability), discouraging potential rent-seekers from entering the state sector, and thus alleviates the "rent-seeking equilibrium".

[47] Different from the administrative positions (which are essentially bureaucrats in the SOE and thus under the direct management of the Party Branch and Discipline Committee), there are a host of technical positions that do not belong to the *Bian-zhi* (bureaucrat-track) and are similar to the one in private firms (e.g., receptionists, technical specialists). Of course, given that the SOE is overall a state sector organization, the focus here is the relative difference in the selection between the administrative jobs and non-bureaucrat jobs.

[48] Nevertheless, all estimated coefficients are positive for the non-administrative positions, which is still partly compatible with the findings of Ajzenman (2021). In addition, since the location of the headquarters has been inspected in 2016, if the increased honesty within an individual dominates, then we should expect the "integrity" performance of employees joining the SOE earlier (when they did not perceive any anti-corruption shock) to converge to that of the treated employees (joining the SOE after experiencing an inspection visit). However, we still observe a significant probity gap in the fiscal year 2020 (Figure 4-B).

[49] The null effect on ability selection may also laterally speak to the notion that, while China's state sector recruitment institution is effective in screening "capable" individuals (as we do not observe a large performance gap in work effectiveness within the organization), it does not fully resolve the selection issue in terms of public service motivations (as we see a sharp change in selection on integrity when the returns to rent-seeking shrink).

4.2.4 Alternative explanations and further discussion

Apart from sharply dampening the returns to rent-seeking, anti-corruption visits might also affect career uncertainty and local socioeconomic conditions. Here I provide a discussion that these prominent channels may not likely explain the above findings on talent allocation.

Career uncertainty and risk-taking. The massive corruption crackdown may likely yield greater career uncertainty on two fronts: (1) as anti-corruption puts a particular emphasis on reducing misbehavior and undesirable bureaucratic practices, some informal practices and behaviors (that are hard to be clearly justified) may associate with the uncertainty of punishment (Shi, 2021; Wang, 2021); (2) meanwhile, the removal of corrupt officials also aggravates the uncertainty of a locality or department over the short run.

First, the usage of occupational preferences from child-parent surveys has largely alleviated the confounding impact generated by short-term career uncertainty. Because the middle school students are far from entering the workforce, the reshuffle of local/departmental leadership should have little relevance to their occupational outcomes. Second, in terms of perceived career uncertainty for public jobs, the SOE analysis can help shed some light. Given I only exploit the variation in applicants' hometown/college cities, once holding the year of recruitment constant, individuals face the same short-term career uncertainty (as they all apply to the same organization). If the primary channel is a shift in perceived uncertainty about public jobs in general, then we may expect a significant decrease in the number of applicants (as long as the majority of the population is risk-averse). If we further take into account the speculation that honest people are generally more risk-averse, then the presence of any career uncertainty would drive our estimated effects on positive selection for integrity toward zero. Moreover and interestingly, Barfort et al. (2019) and Gans-Morse (2021) both find no evidence that risk preferences systematically correlate with honesty or prosocial preferences. Finally, conceptually speaking, front-line public workers and new entrants are less likely to suffer from career uncertainty generated by the corruption crackdown, compared to senior officials (who are not the focus of this study).

Socioeconomic changes and occupational choices. Many studies have documented the impacts of anti-corruption on economic performance (in particular, firm-level outcomes) (Xu and Yano, 2017; Chen and Kung, 2019; Kong and Qin, 2021; Xu et al., 2021). Table B4 shows that the main results of this paper remain robust to a host of city characteristics, including local GDP, population, the number of public administrative workers, fiscal revenue, fiscal expenditures, and public education spending, as well as the interaction between time-invariant characteristics with the full vector of time fixed effects. Moreover, the SOE analysis holds the place of work (and thus the external environment) constant, and we still observe that employ-

ees entering the SOE after their hometown/college cities experienced anti-corruption visits outperform in terms of integrity in the fiscal year 2020 (at least three years after recruitment). Finally, as shown in Table B1 earlier, the empirical results remain unchanged when considering the selection on household socioeconomic background, which can be read as a finer proxy of the shock to socioeconomic conditions.

Other robustness checks. To strengthen the causal interpretation, I apply the notion laid out before: if the timing of inspections is not correlated with the evolution of household characteristics over time, we should expect that, conditional on city and time fixed effects, adding or omitting household fixed effects may not significantly alter the empirical patterns of the household-level results. Table B5 shows that the estimated effects on the main outcomes remain largely comparable to the inclusion or exclusion of household fixed effects.

To assess the anticipation effects, Table B6 and Table B7 show that the estimated coefficients remain virtually unchanged when controlling for inspection visits in neighboring cities, suggesting the limited role of spillover effects. This pattern is consistent with the notion that anti-corruption visits generate unprecedented shocks beyond the prior of local residents.

Further discussion on implications. Although changes in talent allocation in terms of integrity and public-mindedness may provide general economic insights– agents respond to changes in returns to rent-seeking (the first-order shock generated by anti-corruption) – we should interpret the results regarding ability and representativeness with caution. While it is tempting to associate rent-seeking with selection on ability, it is not clear how the distributions of public service motivations and ability correlate with each other, and the corresponding empirical pattern may thus vary by setting.[50] The results using the representative household panel and the provincial SOE sample both suggest there is no strong trade-off in the Chinese setting. Notably, the finding that selection on integrity remains unchanged even if considering the selection on ability and family background is actually consistent with the existing work suggestive of the independence between public workers' ability and integrity (Hanna and Wang (2017) and Mahzab (2020) in the Indian setting, Barfort et al. (2019) in the Danish setting, and Gans-Morse (2021) in the Ukrainian setting).

Moreover, even if the corruption crackdown might deter some capable rent-seekers, the CCP bureaucracy can still utilize its powerful screening mechanisms to retain talent and improve representativeness among eligible candidates, provided there remains a large pool of

[50] For instance, different societies may demonstrate different preference distributions (Hsieh et al., 2019), and there is heterogeneity in the weights assigned to non-financial motivations (Besley et al., 2021); meanwhile, the size of the bureaucracy, the role of the state sector in the economy, and the value of other options also matter (Murphy, Shleifer and Vishny, 1991; Ehrlich and Lui, 1999; Arcidiacono et al., 2020).

applicants.[51] By also considering the idea that an individual's ability is not time-invariant in their formative years – reducing corruption may trigger increased human capital investment and improve not-yet-realized productivity – then there could be potential long-term efficiency gains in both the private and state sectors.[52]

4.3 Further results on human capital investment

The previous analyses on talent allocation take human capital stock as static, inviting inquiry into whether reduced corruption reshapes human capital accumulation, the engine of growth in many endogenous growth models. As emphasized by Ehrlich and Lui (1999), individuals compete for the privilege of rent-seeking in the presence of pervasive corruption, which consumes economic resources that could otherwise be used for production or human capital investment; moreover, rent-seekers do not internalize their impact on other agents' incentives to invest in human capital. I thus now turn to examine the impact of reducing corruption on household talent accumulation. As previously noted, the focus is on intergenerational investment in education in China. Without a clean experimental setting leveraging information interventions only, this section attempts to articulate a tentative discussion that aims to better understand the broader implications for human capital of reducing corruption.[53]

I begin with desired educational attainment, which serves as a proxy for the expected return on human capital.[54] Columns (1) - (2) in Table 6 report the results, with Column (1) associating an anti-corruption inspection with a 0.27-year increase in children's ideal educational attainment reported by their parents, and Column (2) presenting the result that uses the desired years of schooling reported by children as the outcome variable, in which the estimated effect is positive but indistinguishable from zero. These empirical patterns are seemingly reasonable given the notion that adults, as more sophisticated decision-makers that are sensitive to changes in returns to rent-seeking vs. productive activities, tend to exhibit more salient reactions in terms of long-term-oriented human capital investment.

[51] In 2021, applicants registering for the national civil service exam in China still have only a 1-in-68 chance of being recruited. The null effect on employees' "work effectiveness" presented earlier also collaterally corroborates a strong screening on ability in the Chinese state sector.

[52] Bai et al. (2021) find that individuals with higher college entrance exam scores are more likely to join state sectors; yet, those capable individuals who create firms are also more successful than those of their lower-score counterparts.

[53] Intriguingly, the CCP initiated a sudden crackdown on after-school education in 2021 with the intention of removing burdens on families and easing pressure on children. Anecdotal evidence suggests that the anti-corruption movement may partially account for the subsequent education fever: given households could no longer excel via rent-seeking and favor-exchange, children's prospects hinged more substantially on diligence and academic performance.

[54] The corresponding survey question for parents is "the highest educational attainment you expect this child to receive (ideally)", and the corresponding question for children is "the highest educational attainment you intend to receive (ideally)". The expected years of schooling is inferred by the average years of schooling to obtain the degree (e.g., 16 years for a bachelor's degree).

Next, I examine whether there are corresponding behavior changes. Column (3) documents an increase in educational investment by households following the anti-corruption shock (29.6 USD on average). Table B8 corroborates this finding by further scrutinizing the structure of household expenditures. The estimation results based on the CFPS panel show that reducing corruption lowers investment in social ties and gift-giving and comparably increases personal consumption and investment in other areas (including education).[55] To move beyond pecuniary changes, Column (4) of Table 6 examines the effect on parents' attention allocated to their children's human capital, using an outcome variable that is 1 if the parent reports that they focus particular attention on their child's academic performance and conduct. The anti-corruption visit is associated with a 5% increase in the probability of allocating greater attention to children's education.

Finally, Figure 5 presents the event study plots for the aforementioned dependent variables and validates the absence of pre-trends. Given the prominent role of educational inputs from parents in shaping human capital in East Asia (Zhu and Chang, 2019), the empirical results align with the conceptual framework that reducing corruption may incentivize talent accumulation (Murphy, Shleifer and Vishny, 1993; Ehrlich and Lui, 1999). Although changes to children's human capital have not yet translated into production and innovation activities, they are likely to generate long-term economic gains across all sectors.

Notably, the corruption crackdown may have induced confounding changes (e.g., quality of public education (Ferraz, Finan and Moreira, 2012)) that also alter the demand for education. Appendix C provides further discussion in favor of the speculative interpretation that increased educational investment in response to anti-corruption is, at least partially, motivated by productive human capital accumulation.

5 Conclusion

This paper provides systematic causal evidence underscoring the human capital consequences of corruption. Existing empirical studies have largely explored the indirect channel, which sees corruption distort the provision of public goods and in turn undermine human capital. Building on theoretical work on rent-seeking and economic growth, this paper goes beyond the existing literature by focusing on the direct channel – rent-seeking crowds out productive human capital and skews talent allocation (Murphy, Shleifer and Vishny, 1991, 1993;

[55] The "personal consumption" here, following the standard of the National Bureau of Statistics, comprises spending on food, clothing, renting, household facilities, health care, transportation and communications, education and recreation, and other services. Given the CFPS does not elicit educational investment and other relevant expenses on a semester basis and does not consistently define finer details across years, I cannot conduct further analyses using fine-grained expenditure categories.

Ehrlich and Lui, 1999). The Chinese setting provides an ideal setting for empirical analysis given rent-seeking remains salient in Chinese society, with staggered anti-corruption inspections generating rich and sharp variation enabling valid identification.

My main analysis shows that reducing corruption induces positive selection for integrity and public-mindedness into state sector organizations, helping to alleviate the "rent-seeking equilibrium" talent-allocation trap (Acemoglu, 1995; Hanna and Wang, 2017). Importantly, the effect remains virtually unchanged even if taking the selection of ability and family background into account, implying that screening based on ability or representativeness may not necessarily mitigate rent-seeking in the public sector. I also endogenize human capital stock and document suggestive evidence of increased household investment in human capital in response to reduced corruption, featuring potential long-term productivity gains for society as a whole. The allocation and accumulation margins can be mutually reinforcing and together shape long-term growth.

These findings bear relevant implications. First, public perception of dampened returns to rent-seeking may effectively improve the allocation and selection margin of talent. This can be achieved by either monitoring and punishing misconduct (Ferraz and Finan, 2008; Vannutelli, 2021) to reshape social norms surrounding corruption, or improving the screening process via increasing the emphasis on integrity and public service motivation (e.g., Ashraf, Bandiera and Jack, 2014; Hanna and Wang, 2017; Gulzar and Khan, 2021; Spenkuch, Teso and Xu, 2021). In particular, improved political selection (in terms of public service incentives) may further attenuate corruption norms and bolster more sustainable reward structures. Of course, the relative cost-effectiveness of different approaches hinge on institutional features (e.g., auditing costs and the prevalence of corruption norms). Second, my combined results point to the broader implications associated with long-term economic growth: reducing corruption may prompt human capital accumulation, including the not-yet-realized productivity of subsequent generations, a necessary engine of sustainable economic growth that can further interact with the talent allocation margin. It follows that policymakers should be conscious of the long-term human capital consequences that are latent and difficult to observe in the short run, even if, in many circumstances, corruption can have short-term benefits (and anti-corruption may associate with acute pain).

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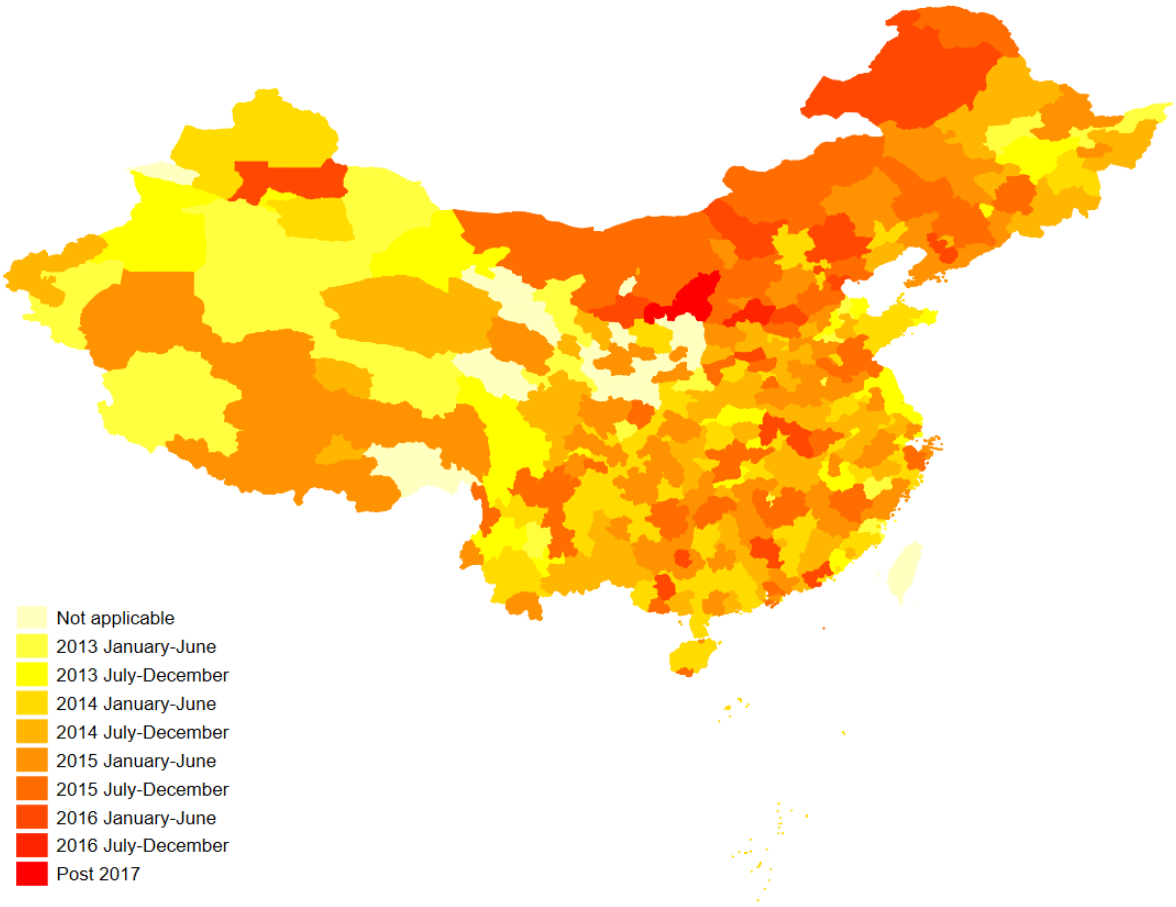
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Figures

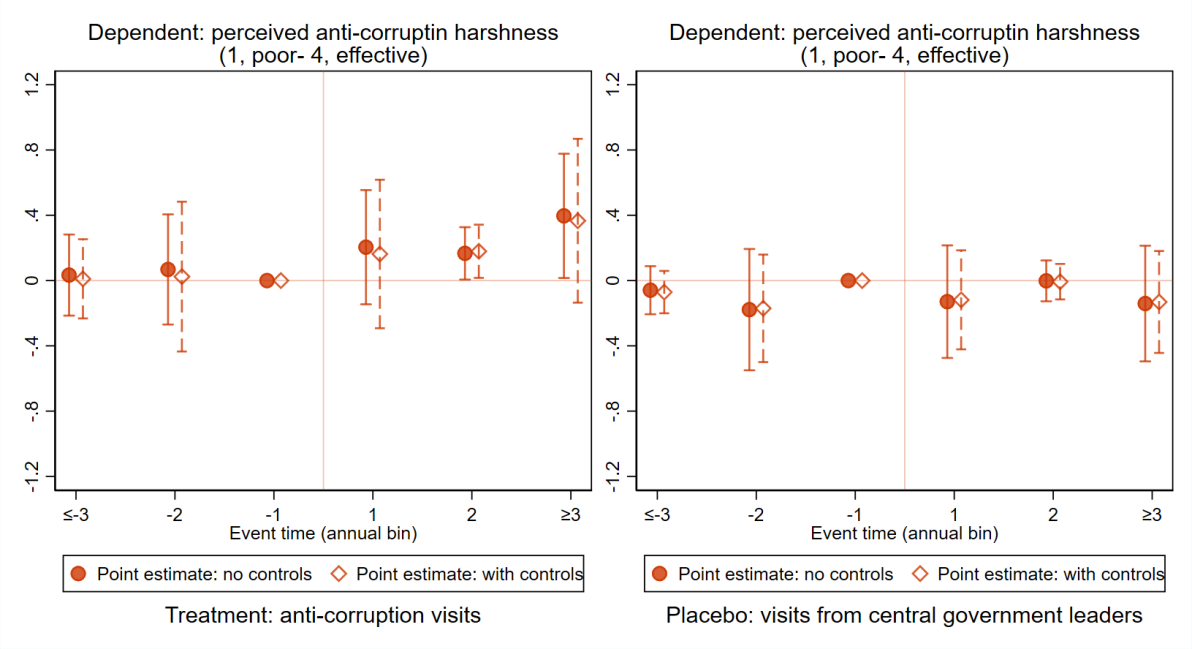
Figure 1: Timing of the first anti-corruption visit (2013-2017)



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

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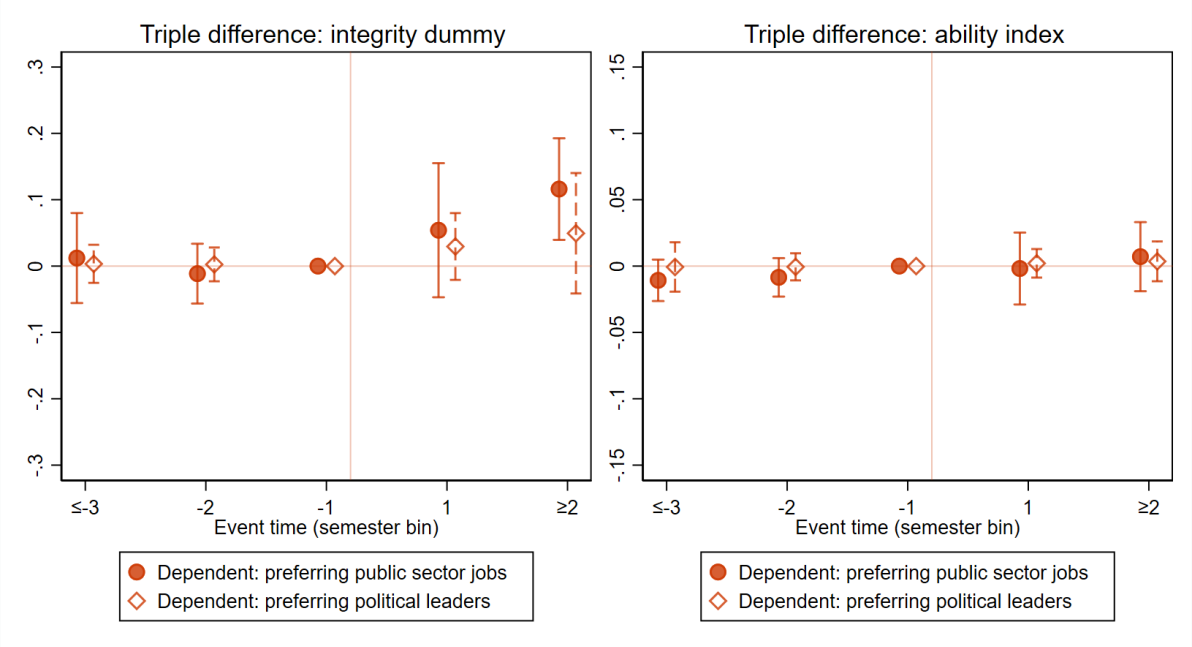
Figure 2: Corruption crackdown and perceived returns to rent-seeking



Notes: The left panel shows the dynamic estimated effects of anti-corruption inspection visits, and the right panel shows the placebo test using short-term visits from center government leaders. The unit of observation is the individual. The specification corresponds to that of Table 3 but allowing the effect to vary by each event-time period. For example, "+1" refers to individuals surveyed one year after their cities have experienced the shock, compared to individuals surveyed one year prior to the shock ("-1" as the baseline period). The 95% confidence intervals are reported using standard errors clustered at the city-cohort level. Data source: Chinese Social Survey (2011 - 2017).

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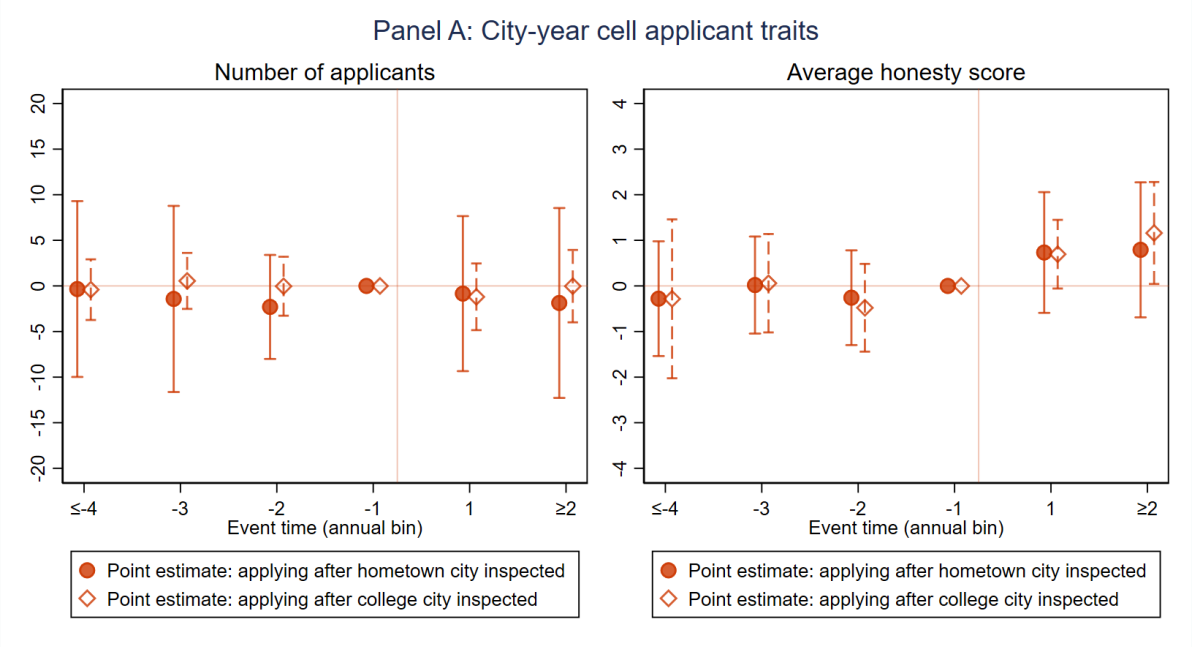
Figure 3: Corruption crackdown and occupational preferences - heterogeneity by integrity and ability



Notes: The figure shows the dynamic heterogeneous effects (triple-difference estimates) of anti-corruption inspection visits on household occupational preferences by children's integrity and ability (time-invariant scores). The unit of observation is the household-semester. The specification corresponds to that of columns (3) and columns (6) in Table 4 but allowing the estimated effect to vary by each event-time period. For example, "+1" refers to the heterogeneous impact by traits on households surveyed one semester after their cities have experienced anti-corruption visits, compared to households surveyed one semester before their cities have experienced anti-corruption visits ("-1" as the baseline period). The 95% confidence intervals are reported using standard errors clustered at the city-cohort level. Data source: China Education Panel Survey (2013 - 2015).

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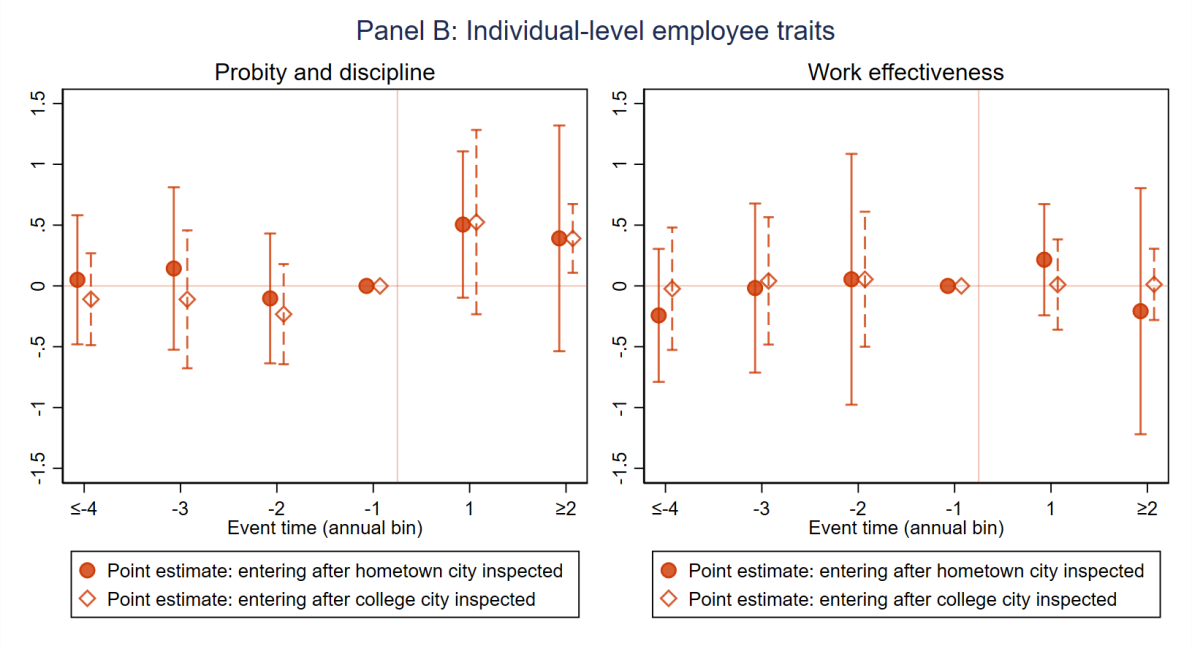
Figure 4-A: Selection into state sector organizations
(Panel A: applicant traits)



Notes: The figure shows the dynamic estimated effects of anti-corruption on applicants for administrative positions of a provincial state-owned enterprise. Unit of observation in **Panel A** is the city-year. Sample period: 2011 - 2020. "Average honesty score" is the mean honesty score of applicants of a city-year cell; the score is based on the L-index from the personality test result (automatically assessed by the system using several psychological techniques). The specification corresponds to that of **Panel A** of Table 5 but allowing the estimated effect to vary by each event-time period. For example, "+1" refers to applicants from cities that have experienced anti-corruption visits one year ago, compared to applicants from cities one year prior to anti-corruption visits ("-1" as the baseline period). The 95% confidence intervals are reported using standard errors clustered at the city-cohort level.

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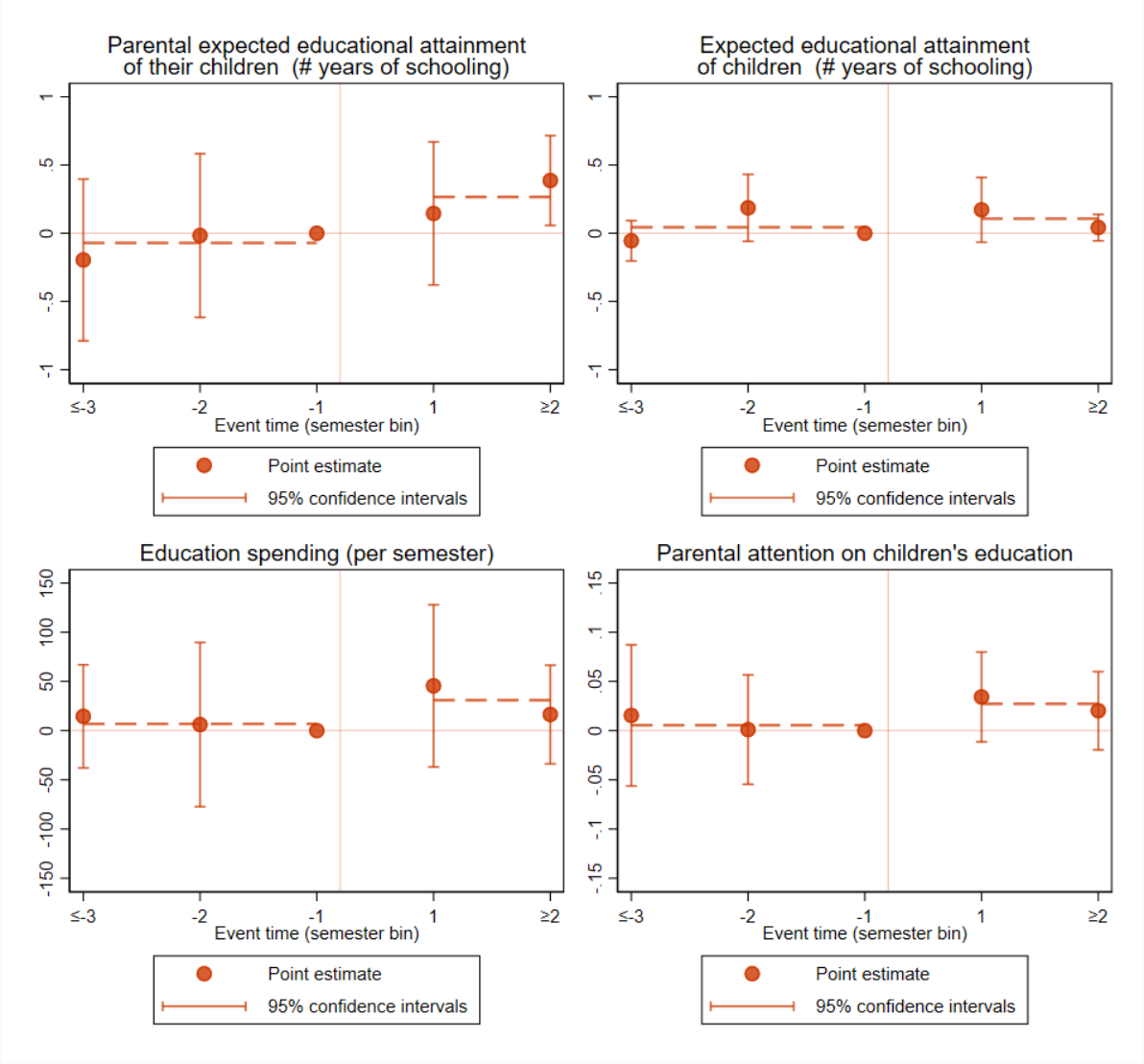
Figure 4-B: Selection into state sector organizations
(Panel B: employee traits)



Notes: The figure shows the dynamic estimated effects of anti-corruption on employee traits of a provincial state-owned enterprise. Unit of observation in **Panel B** is the individual. The sample comprises all administrative employees recruited from 2007 to 2020. The specification corresponds to that of **Panel B** of Table 5 but allowing the estimated effect to vary by each event-time period. For example, "+1" refers to individuals entering the organization one year after their hometown/college cities have experienced anti-corruption visits, compared to those individuals entering the organization one year before their hometown/college cities have experienced anti-corruption visits ("-1" as the baseline period). The 95% confidence intervals are reported using standard errors clustered at the city-cohort level.

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Figure 5: Corruption crackdown and human capital investment



Notes: The figure shows the dynamic estimated effects of anti-corruption inspection visits. The unit of observation is the household-semester. The specification corresponds to that of Table 6 but allowing the estimated effect to vary by each event-time period. For example, "+1" refers to households surveyed one semester after their cities have experienced anti-corruption visits, compared to households surveyed one semester before their cities have experienced anti-corruption visits ("-1" as the baseline period). The horizontal dash lines delineate the average pre-treatment and post-treatment estimated effects. The 95% confidence intervals are reported using standard errors clustered at the city-cohort level. Data source: China Education Panel Survey (2013 - 2015).

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Tables

Table 1: Summary statistics and balance check - survey respondents

CEPS (2013-2015)			CFPS (2012-2016)			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	0.495	0.000	Female	0.454	0.009
	(0.499)	(0.006)		(0.500)	(0.003)		(0.498)	(0.015)
Rural household	0.527	-0.029	Rural resident	0.550	0.017	Rural resident	0.450	-0.018
	(0.499)	(0.022)		(0.498)	(0.014)		(0.497)	(0.023)
Only child	0.616	0.012	Family size	4.331	-0.007	Family size	4.276	-0.124
	(0.486)	(0.013)		(1.995)	(0.041)		(1.897)	(0.097)
Child's height	158.517	0.134	Age	45.812	-0.266	Age	46.702	0.455
	(8.364)	(0.091)		(17.304)	(0.310)		(14.009)	(0.492)
Father: CCP member	0.124	0.011	CCP member	0.072	-0.001	CCP member	0.103	0.009
	(0.330)	(0.008)		(0.259)	(0.002)		(0.303)	(0.008)
Father: $\geq$ high school	0.398	0.013	$\geq$ high school	0.266	0.006	$\geq$ high school	0.304	0.014
	(0.489)	(0.013)		(0.442)	(0.007)		(0.460)	(0.016)
Mother: CCP member	0.057	-0.003	Socioeconomic	2.803	-0.025			
	(0.231)	(0.004)	index	(1.038)	(0.023)			
Mother: $\geq$ high school	0.360	0.012						
	(0.480)	(0.013)						
Unit of observation	Child-Year		Unit of observation	Individual-Year		Unit of observation	Individual-Year	
Observations	20,558		Observations	101,192		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; "CFPS" refers to the Chinese Family Panel Studies; "CSS " refers to the Chinese Social Servery. "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 the anti-corruption visit team. In Columns (2), (4) and (6), standard errors clustered at the city level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table 2: Summary statistics and balance check - city characteristics

	Treatment timing (cohort)						
	Earlier treated	2014 Spring	2014 Fall	2015 Spring	Later treated	Uncond. F-test (P-value)	Cond. F-test (P-value)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Level (2012)							
Log. Distance to the coast (km)	5.090	6.174	5.623	5.746	6.123	0.148	0.454
Log. Local GDP (million)	16.508	16.284	16.395	16.378	15.895	0.729	0.869
Log. Population (ten thousand)	5.891	5.892	6.050	5.823	5.529	0.644	0.338
Log. # public workers	10.566	10.459	10.684	10.526	10.121	0.484	0.519
Log. Fiscal expenditure	13.574	13.486	13.434	13.546	13.421	0.901	0.364
Log. Fiscal revenue	13.017	12.811	12.762	12.960	12.547	0.814	0.647
Log. Public education spending	9.194	9.071	8.988	9.157	8.901	0.928	0.577
Average years of schooling (2010)	8.862	8.476	8.819	8.890	8.582	0.473	0.421
Panel B: Growth rate (2012)							
Local GDP	0.118	0.124	0.112	0.119	0.119	0.546	0.328
Population	0.122	0.096	0.195	0.034	0.108	0.876	0.430
Number of public workers	0.040	0.068	0.029	0.048	0.036	0.439	0.536
Fiscal expenditure	0.207	0.232	0.166	0.187	0.227	0.437	0.300
Fiscal revenue	0.253	0.281	0.202	0.203	0.234	0.169	0.323
Public education spending	0.303	0.405	0.402	0.338	0.425	0.484	0.860

Notes: The table shows summary statistics of outcomes in cities for which the first anti-corruption inspection visit starts, respectively, at different time points. The sample comprises all cities in Mainland China. 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 are only available from the 2010 Population Census. Data source: the 2010 Population Census and the National Bureau of Statistics of China.

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Table 3: Corruption crackdown and perceived returns to rent-seeking

	Perceived anti-corruption harshness (1, poor - 4, effective)			
	Reality checks		Placebo tests	
	(1)	(2)	(3)	(4)
Mean of dep. var	3.423	3.426	3.490	3.493
Inspected	0.154***	0.160***		
	(0.050)	(0.049)		
Visited by center government leaders			0.021	0.024
			(0.037)	(0.037)
Time (year) × Cohort FEs	Y	Y	Y	Y
City × Cohort FEs	Y	Y	Y	Y
Individual controls	-	Y	-	Y
Observations	37,397	36,834	51,552	51,023
Adjusted R-squared	0.064	0.074	0.071	0.082
Rambachan & Roth's M Bound (%)				
Data source	Chinese Social Survey (2011 - 2017, pooled)			

Notes: "Inspected" is a dummy that is 1 if the city of residence has been inspected by the 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. "Cohort" denotes a set of dummies for treatment cohorts in the stacked-by-event design. Individual controls comprise full sets of dummies for gender, age, family size, educational attainment, and the Party membership. Unit of observation is the individual-year . Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table 4: Corruption crackdown and household occupational preferences

	Occupational preferences: public sector jobs			Occupational preferences: government officials		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of dep. var	0.352	0.352	0.352	0.061	0.061	0.062
Inspected	0.014	-0.082*	-0.077	-0.005	-0.044	-0.044
	(0.019)	(0.048)	(0.048)	(0.008)	(0.028)	(0.028)
× Integrity index (dummy, time-invariant)		0.105**	0.098**		0.043*	0.044*
		(0.042)	(0.042)		(0.025)	(0.025)
× Ability index (standardized, time-invariant)			0.004			0.005
			(0.010)			(0.006)
Household × City × Cohort FEs	Y	Y	Y	Y	Y	Y
Time (semester) × Cohort FEs	Y	Y	Y	Y	Y	Y
Lower order interactions (direct effects)	Y	Y	Y	Y	Y	Y
Observations	37,078	37,078	36,211	37,078	37,078	36,211
Adjusted R-squared	0.294	0.294	0.291	0.216	0.216	0.220
Rambachan & Roth's M Bound (%)						
Data source	China Education Panel Survey (2013 - 2015)					

Notes: Unit of observation is the child(-parent)-year. The dependent variable is a binary that is 1 if both the child and their parents prefer the corresponding occupation. "Inspected" is a dummy that is 1 if the city has been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts in the stacked-by-event design. Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table 5: Selection into state sector organizations - SOE bureaucrat samples

Panel A: **City-year cell applicant traits (2011-2020)**	Number of applicants		Average honesty score (1, low - 12, high)	
	(1)	(2)	(3)	(4)
Mean of dep. var	21.429	11.104	8.334	8.498
Applying after hometown city inspected	-0.118		0.873*	
	(3.250)		(0.490)	
Applying after college city inspected		-0.891		1.031***
		(0.927)		(0.252)
Entry year × Cohort FEs	Y	Y	Y	Y
Hometown city × Cohort FEs	Y	-	Y	-
College city × Cohort FEs	-	Y	-	Y
Observations	392	772	354	538
Rambachan & Roth's M Bound (%)				
Panel B: **Individual employee traits (2007-2020)**	"Probity and discipline" (1, poor - 4, outstanding)		"Work effectiveness" (1, poor - 4, outstanding)	
	(1)	(2)	(3)	(4)
Mean of dep. var	2.830	2.830	2.844	2.844
Entering after hometown city inspected	0.462*		0.100	
	(0.243)		(0.341)	
Entering after college city inspected		0.575**		0.008
		(0.264)		(0.139)
Entry year × Cohort FEs	Y	Y	Y	Y
Hometown city × Cohort FEs	Y	-	Y	-
College city × Cohort FEs	-	Y	-	Y
Demographic controls	Y	Y	Y	Y
Observations	1,274	1,292	1,274	1,292
Rambachan & Roth's M Bound (%)				

Notes: Unit of observation in **Panel A** is the city-year (hometown city × year cell in Columns 1 and 3, and college city × year cell in Columns 2 and 4). "Average honesty score" is the mean honesty score of applicants of a city-year cell; the score is based on the L-index from the personality test result (automatically assessed by the system using several psychological techniques). Unit of observation in **Panel B** is the individual. The sample comprises all administrative employees in a provincial state-owned enterprise recruited from 2007 to 2020. Each dependent variable is constructed based on the annual evaluation reports in the fiscal year 2020 (cross-sectional). "Applying/entering after hometown (college) city inspected" is a dummy that is 1 if the employee applied for/joined the SOE after their hometown (college) city had been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts. Demographic controls comprise the fixed effects for gender, age, and ethnicity. Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table 6: Corruption crackdown and human capital investment

	Desired educational attainment of children		Household investment in human capital	
	(1) Years of schooling (from parent)	(2) Years of schooling (from child)	(3) Education spending (in USD)	(4) Parental attention allocated on education (binary)
Mean of dep. var	16.660	16.300	260.159	0.741
Inspected	0.269**	0.085	29.587*	0.054***
	(0.121)	(0.110)	(14.876)	(0.019)
Household×City×Cohort FEs	Y	Y	Y	Y
Time (semester)×Cohort FEs	Y	Y	Y	Y
Data source	China Education Panel Survey (2013-2015)			
Observations	37,348	35,711	35,298	37,348
Adjusted R-squared	0.439	0.527	0.407	0.299

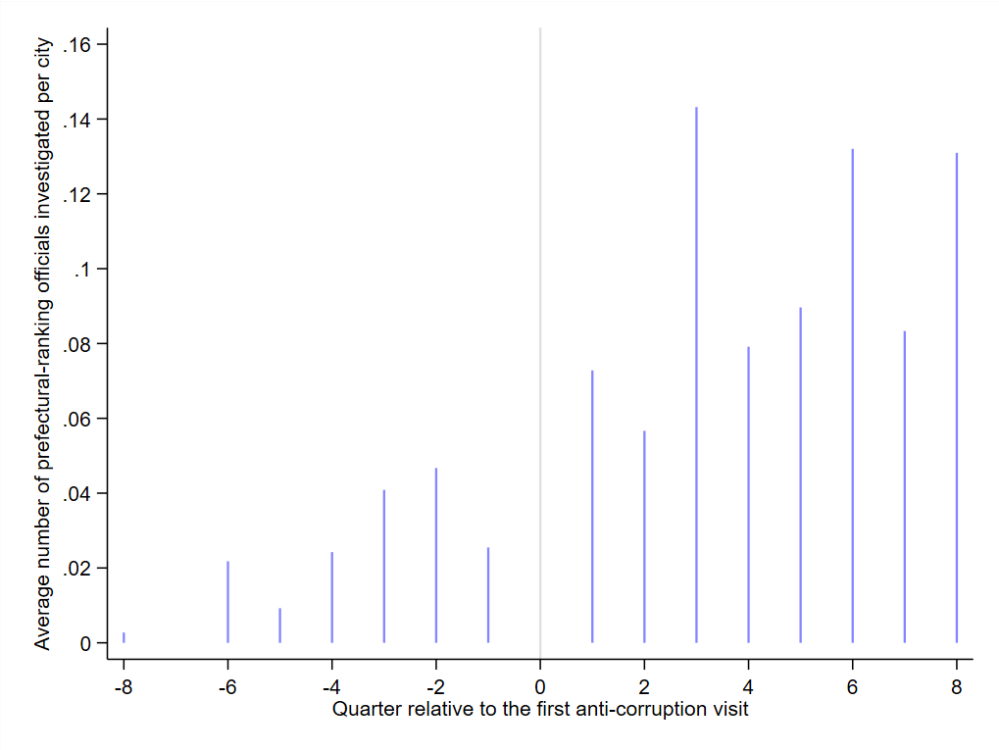
Notes: Unit of observation is the child(-parent)-year. "Inspected" is a dummy that is 1 if the city of residence has been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts in the stacked-by-event design. Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Appendix A Appendix Figures

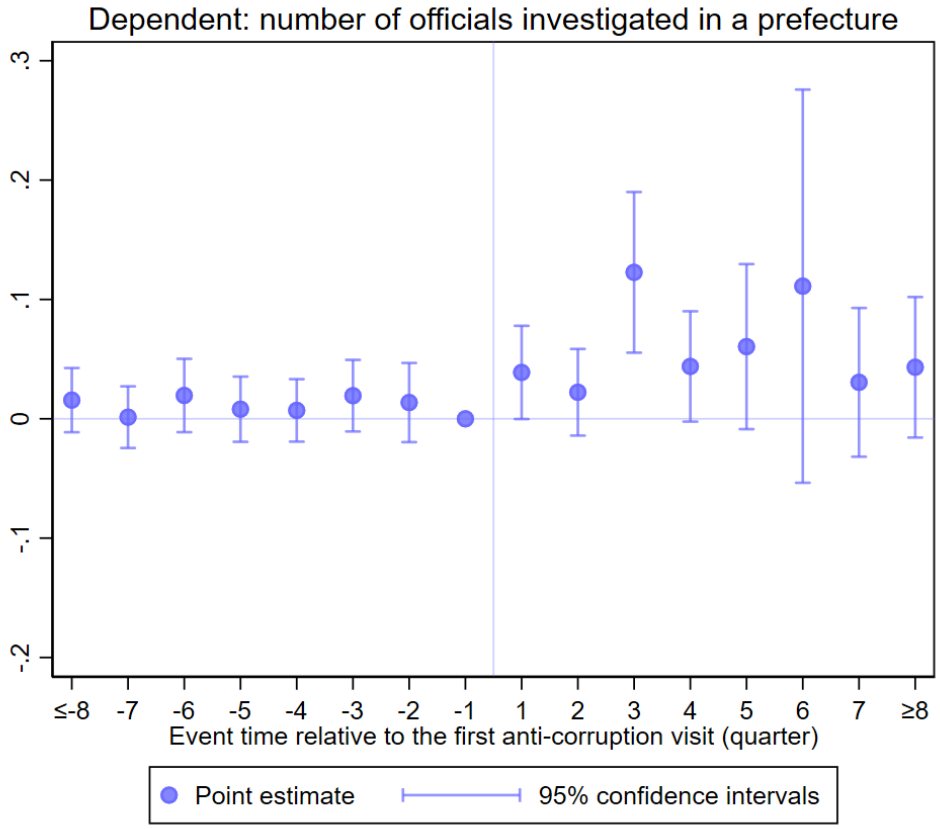
Figure A1: Anti-corruption visits and # officials investigated per city



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

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Figure A2: Anti-corruption visits and # 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 stack-by-event design. The unit of observation is the prefecture-quarter.

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Appendix B Appendix Tables

Table B1: Occupational preferences - robustness to alternative measures of traits

	Occupational preferences: public 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	0.014	0.012	0.010	-0.005	-0.002	-0.002
	(0.019)	(0.019)	(0.019)	(0.008)	(0.008)	(0.008)
× Public mindedness index (standardized)		0.035***	0.034***		0.014**	0.014**
		(0.011)	(0.011)		(0.007)	(0.007)
× Ability index (standardized)		-0.006	-0.009		-0.006	-0.007
		(0.016)	(0.016)		(0.008)	(0.008)
× Socioeconomic index (standardized)			0.028**			0.006
			(0.011)			(0.006)
Household × City × Cohort FEs	Y	Y	Y	Y	Y	Y
Time (semester) × Cohort FEs	Y	Y	Y	Y	Y	Y
Data source	China Education Panel Survey (2013-2015)					
Observations	37,078	33,431	33,384	37,078	33,431	33,384
Adjusted R-squared	0.294	0.294	0.294	0.216	0.217	0.217

Notes: Unit of observation is the child(-parent)-year. The dependent variable is a dummy that is 1 if both the child and their parent prefer the corresponding occupation. "Inspected" is a dummy that is 1 if the city of residence has been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts in the stacked-by-event design. All regressions in Columns (1) - (4) also control for lower-order interaction terms (direct effects). Standard errors are clustered at the city-cohort level. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

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Table B2: Occupational preferences - considering separate responses

	Occupational preferences: public sector jobs			Occupational preferences: government officials		
	(1)	(2)	(3)	(4)	(5)	(6)
	Parent	Child	Aggregate	Parent	Child	Aggregate
Mean of dep. var	0.595	0.456	0.351	0.205	0.103	0.061
Inspected	-0.058	-0.022	-0.077	-0.066**	-0.029	-0.044
	(0.042)	(0.044)	(0.048)	(0.032)	(0.037)	(0.028)
× Integrity index (dummy)	0.060	0.049	0.098**	0.065*	0.040	0.044*
	(0.040)	(0.040)	(0.042)	(0.039)	(0.033)	(0.025)
× Ability index (standardized)	0.014	0.008	0.004	0.010	-0.010	0.005
	(0.015)	(0.009)	(0.010)	(0.008)	(0.008)	(0.006)
Household × City × Cohort FEs	Y	Y	Y	Y	Y	Y
Time (semester) × Cohort FEs	Y	Y	Y	Y	Y	Y
Data source	China Education Panel Survey (2013-2015)					
Observations	36,211	36,211	36,211	36,211	36,211	36,211
Adjusted R-squared	0.299	0.281	0.291	0.289	0.250	0.220

Notes: Unit of observation is the child(-parent)-year. Each dependent variable in Columns (3) and (6) is a household-level binary that is 1 if both the child and their parent prefer the corresponding occupation. "Inspected" is a dummy that is 1 if the city of residence has been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts in the stacked-by-event design. All regressions also control for lower-order interaction terms (direct effects). Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table B3: Selection into the SOE – non-bureaucrat track applicants

	Average honesty score (1, low - 12, high)			
	Administrative positions		Non-administrative positions	
	(1)	(2)	(3)	(4)
Mean of dep. var	8.334	8.498	8.170	8.457
Applying after hometown city inspected	0.873*		0.330	
	(0.490)		(0.327)	
Applying after college city inspected		1.031***		0.292
		(0.252)		(0.199)
Entry year × Cohort FEs	Y	Y	Y	Y
Hometown city × Cohort FEs	Y	-	Y	-
College city × Cohort FEs	-	Y	-	Y
Observations	354	538	386	602

Notes: Unit of observation is the city-year: "hometown city × year" cell in Columns (1) and (3), and "college city × year" cell in Columns (2) and (4). "Average honesty score" is the mean honesty score of applicants of a city-year cell; the score is based on the L-index from the personality test result (automatically assessed by the system using several psychological techniques). Columns (1) - (2) investigate the applicants for administrative (bureaucrat) positions (the same as that in our main analysis), Columns (3) - (4) investigate the applicants for non-administrative positions (e.g., front-line technical workers that do not belong to the bureaucrat-track) in the headquarters . "Applying after hometown (college) city inspected" is a dummy that is 1 if the employee applied to the SOE after their hometown (college) city had been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts. Sample period: 2011 - 2020. Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table B4: Robustness to city controls - household outcomes

	Occupational preferences: public sector jobs		Occupational preferences: government officials		Household spending on education		Parental attention allocated on education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of dep. var	0.351	0.351	0.061	0.061	260.159	260.159	0.741	0.741
Inspected	-0.077	-0.117***	-0.044	-0.061**	29.587*	26.899*	0.054***	0.032*
	(0.048)	(0.041)	(0.028)	(0.026)	(14.876)	(15.899)	(0.019)	(0.012)
× Integrity index (dummy)	0.098**	0.100**	0.044*	0.044*				
	(0.042)	(0.042)	(0.025)	(0.025)				
× Ability index (standardized)	0.004	0.008	0.005	0.005				
	(0.010)	(0.009)	(0.006)	(0.006)				
Time (semester) × Cohort FEs	Y	Y	Y	Y	Y	Y	Y	Y
Household × City × Cohort FEs	Y	Y	Y	Y	Y	Y	Y	Y
City controls (time-variant)	-	Y	-	Y	-	Y	-	Y
City characteristics (2010) × Event time FEs	-	Y	-	Y	-	Y	-	Y
Data source	China Education Panel Survey (2013-2015)							
Observations	36,211	36,211	36,211	36,211	35,298	35,298	37,348	37,348
Adjusted R-squared	0.291	0.294	0.220	0.221	0.407	0.408	0.299	0.303

Notes: "Inspected" is a dummy that is 1 if the city of residence has been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts in the stacked-by-event design. All regressions in Columns (1) - (4) also control for lower-order interaction terms (direct effects). Time-variant city controls comprise (log) GDP, population, the number of public administration workers, fiscal expenditure, fiscal revenue, and public education expenditures. Time-invariant city characteristics include the distance to the coast, average years of schooling (2010) and share of urban residents (2010), which are only available from the 2010 Population Census. Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table B5: Inclusion/exclusion of household fixed effects

	Occupational preferences: public sector jobs		Occupational preferences: government officials		Household spending on education		Parental attention allocated on education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of dep. var	0.351	0.351	0.061	0.061	260.159	260.159	0.741	0.741
Inspected	-0.064	-0.077	-0.024	-0.044	31.293*	29.587*	0.053**	0.054***
	(0.043)	(0.048)	(0.021)	(0.028)	(16.680)	(14.876)	(0.021)	(0.019)
× Integrity index (dummy)	0.061*	0.098**	0.031*	0.044*				
	(0.036)	(0.042)	(0.018)	(0.025)				
× Ability index (standardized)	0.003	0.004	0.005	0.005				
	(0.010)	(0.010)	(0.005)	(0.006)				
Time (semester) × Cohort FEs	Y	Y	Y	Y	Y	Y	Y	Y
City × Cohort FEs	Y	-	Y	-	Y	-	Y	-
Household × City × Cohort FEs	-	Y	-	Y	-	Y	-	Y
Data source	China Education Panel Survey (2013-2015)							
Observations	36,211	36,211	36,211	36,211	35,298	35,298	37,348	37,348
Adjusted R-squared	0.021	0.291	0.009	0.220	0.407	0.716	0.299	0.621

Notes: "Inspected" is a dummy that is 1 if the city of residence has been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts in the stacked-by-event design. All regressions in Columns (1) - (4) also control for lower-order interaction terms (direct effects). Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table B6: Robustness to spillover effects - household outcomes

	Occupational preferences: public sector jobs		Occupational preferences: government officials		Household spending on education		Parental attention allocated on education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of dep. var	0.351	0.351	0.061	0.061	260.159	260.159	0.741	0.741
Inspected	-0.077	-0.080	-0.044	-0.044	29.587*	29.920*	0.054***	0.053***
	(0.048)	(0.049)	(0.028)	(0.029)	(14.876)	(15.232)	(0.019)	(0.018)
× Integrity index (dummy)	0.098**	0.100**	0.044*	0.044*				
	(0.042)	(0.044)	(0.025)	(0.026)				
× Ability index (standardized)	0.004	0.005	0.005	0.004				
	(0.010)	(0.010)	(0.006)	(0.006)				
Neighboring cities inspected		0.017		-0.001		9.968		-0.018
		(0.049)		(0.015)		(14.437)		(0.013)
× Integrity index (dummy)		-0.018		-0.003				
		(0.049)		(0.016)				
× Ability index (standardized)		0.007		0.004				
		(0.008)		(0.005)				
Time (semester) × Cohort FEs	Y	Y	Y	Y	Y	Y	Y	Y
Household × City × Cohort FEs	Y	Y	Y	Y	Y	Y	Y	Y
Data source	China Education Panel Survey (2013-2015)							
Observations	36,211	36,211	36,211	36,211	35,298	35,298	37,348	37,348
Adjusted R-squared	0.291	0.291	0.220	0.221	0.407	0.407	0.298	0.299

Notes: "Inspected" is a dummy that is 1 if the city of residence has been inspected by the anti-corruption visit team. "Neighboring cities inspected" is a dummy that is 1 if any neighboring city has been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts in the stacked-by-event design. All regressions in Columns (1) - (4) also control for lower-order interaction terms (direct effects). Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table B7: Robustness to spillover effects - SOE outcomes

Panel A: City-year aggregate applicant traits	Number of applicants				Average honesty score (1, low - 12, high)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of dep. var	21.429	21.429	11.104	11.104	8.334	8.334	8.498	8.498
Applying after hometown/college city inspected	-0.118	-0.295	-0.891	-0.777	0.885*	1.021*	0.968***	1.086***
	(3.250)	(3.328)	(0.927)	(0.994)	(0.488)	(0.527)	(0.234)	(0.237)
Applying after neighboring cities inspected		-0.818		1.179		0.435		0.569***
		(4.415)		(0.922)		(0.492)		(0.163)
Entry year × Cohort FEs	Y	Y	Y	Y	Y	Y	Y	Y
City × Cohort FEs	Y	Y	Y	Y	Y	Y	Y	Y
Source of variation	Hometown city		College city		Hometown city		College city	
Observations	392	392	772	772	354	354	538	538

Panel B: Employee traits (individual-level)	Probity and discipline (1 - 4)				Work effectiveness (1 - 4)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of dep. var	2.904	2.904	2.896	2.896	2.873	2.873	2.847	2.847
Entering after hometown/college city inspected	0.415*	0.404**	0.523**	0.565**	-0.047	0.098	0.146	0.175
	(0.213)	(0.200)	(0.216)	(0.228)	(0.135)	(0.232)	(0.160)	(0.179)
Entering after neighboring cities inspected		-0.112		0.222		-0.285		0.151
		(0.158)		(0.215)		(0.178)		(0.271)
Entry year × Cohort FEs	Y	Y	Y	Y	Y	Y	Y	Y
City × Cohort FEs	Y	Y	Y	Y	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
Source of variation	Hometown city		College city		Hometown city		College city	
Observations	1,274	1,274	1,292	1,292	1,274	1,274	1,292	1,292

Notes: Unit of observation in **Panel A** is the city-year (hometown city × year or college city × year cell). "Average honesty score" is the mean honesty score of applicants of a city-year cell; the score is based on the L-index from the personality test result (automatically assessed by the system using several psychological techniques). Unit of observation in **Panel B** is the individual. The sample comprises all administrative employees in a medium-scale state-owned enterprise recruited from 2007 to 2020. Each dependent variable is constructed based on the annual evaluation reports in the fiscal year 2020 (cross-sectional). "Applying/entering after hometown (college) city inspected" is a dummy that is 1 if the employee applied for/joined the SOE after their hometown (college) city had been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts. Demographic controls comprise the fixed effects for gender, age, and ethnicity. Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table B8: Corruption crackdown and household expenditures

	Household expenditures per capita (annual, USD)			
	Total	Personal consumption	Transfer and social ties	Welfare
	(1)	(2)	(3)	(4)
Mean of dep. var	2394.970	2097.396	243.994	53.581
Inspected	50.477	134.020*	-79.278***	-4.265
	(95.497)	(78.904)	(30.573)	(5.482)
Time × Cohort FEs	Y	Y	Y	Y
Household × City × Cohort FEs	Y	Y	Y	Y
Data source	CFPS Household Panel (2012-2016)			
Observations	225,538	225,538	225,538	225,538
Adjusted R-squared	0.799	0.804	0.664	0.781

Notes: Unit of observation is the household-year. "Inspected" is a dummy that is 1 if the city of residence has been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts in the stacked-by-event design. "CFPS" refers to the Chinese Family Panel Studies. Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Appendix C Discussion on human capital accumulation

Greater investment in education is not a perfect equivalence to productive human capital accumulation – for instance, in Ehrlich and Lui (1999), educational inputs in competing for the privilege of becoming bureaucrats will be read as an investment in political capital. Meanwhile, it is likely that the anti-corruption visit alters investment in education via channels other than perceived returns to human capital versus rent-seeking (e.g., the quality of public education, teachers' behavior, parents' time budgets). I provide a tentative discussion here on whether the observed changes reflect, at least partially, the intention of accumulating productive human capital as suggested by theories.

Non-pecuniary investment and time budgets. This set of empirical analyses is an extension of the result in Column (4) of Table 6. Differing from pecuniary inputs, non-pecuniary investment (e.g., attention allocation and interaction with children to reduce information frictions) is more in harmony with the productive human capital accumulation explanation (Bergman, 2021). The CEPS designs a set of questions to elicit parental attention allocated to their children across five different domains. Each dependent variable is a binary that is 1 if the parent reports they pay particular attention to the corresponding domain.

Figure C1 visualizes the estimated coefficients with 95% confidence intervals. The anti-corruption shock is associated with a greater probability of allocating attention on children's' academic performance (Row 1) and conduct (Row 2 and Row 5).[56] These empirical patterns are consistent with greater inputs in the quality of human capital, plausibly featuring a greater intention of accumulating productivity. Meanwhile, we see parents put less emphasis on children's network with classmates and friends after the shock on average (Row 3). Nevertheless, this result should be interpreted with caution: it could partially reflect the idea that social capital, as the key nexus for rent-seeking in China, has become less valuable after the corruption crackdown; alternatively, it could simply be due to limited attention – parents reallocate their primary focus of attention. Row 4 provides a placebo test, showing that the estimated effect of attention on children's dressing – which has little relevance to human capital and rent-seeking – is close to zero. The figure also visualizes the estimated effects on parental attention perceived by children. The results are consistent with that of using parents' responses (note that responses from children and their parents are elicited separately).

[56] Row 5 focuses on restricting children's usage of entertainment media and Internet (e.g., limiting screen time and online video gaming, blocking inappropriate content), about which most Chinese parents are particularly concern, as they read them as "electronic drugs" that undermine their children's development. In September 2021, Chinese government weighs in formally, imposing the rules that individuals under the age of 18 are restricted to three hours of play per week, with the intention of protecting children and reintegrating them with their families and nature.

In addition, Figure C2 investigates changes in children's subjective well-being. I do not find that anti-corruption generates negative impacts on children's mental health (Rows 1-4); instead, the treated children exhibit greater self-perceived prospects, which is compatible with the observed improvement in the quality of human capital investment.[57]

Finally, in Column (8) of Table C1, I further eliminate the possibility that the increased non-pecuniary investment is driven by reduced working hours of parents following the corruption crackdown.

Perceived quality of public education in the short run. Middle-school education is largely provided by the state sector, the quality of which may be weakened by corruption (Reinikka and Svensson, 2005; Ferraz, Finan and Moreira, 2012). Alternatively, corruption crackdown may also disincentivize local bureaucrats in the short run (Wang, 2021), which affects public good provision. Therefore, one may be concerned about, instead of the direct incentivizing effect, the observed increased investment in human capital is largely driven by changes in the quality of schooling. Since anti-corruption visits have primarily focused on bureaucracy before 2016, and the time span of the CEPS data used in my analysis is from 2013 to 2015, one would not necessarily expect changes in education quality as a prominent mechanism over the short term. I provide several pieces of quantitative evidence to buttress this argument.

Table C1 reports the empirical results on a wide range of measures of perceived quality of schooling. Columns (1) - (3) investigate the changes in student performance, measured by their midterm grades (standardized by each school) in Chinese, Math, and English (three key subjects in middle school). These outcomes generate perhaps the most crucial signals for Chinese parents to adjust their educational investment. All estimated coefficients are indistinguishable from zero, implying my result is not driven by changes in children's grades in the short run.

In Columns (4) - (7) of Table C1, I turn to parents' perceived quality of education. Column (4) shows that the anti-corruption visit is associated with greater responsiveness of teachers perceived by parents. Yet, the underlying mechanism remains ambiguous: as teachers are public sector employees (though not directly targeted), the deterrence effect of the corruption crackdown may provide better incentives for them to be responsive; alternatively, it might be that treated parents put greater attention to human capital accumulation and increase their communication with teachers. In Column (7), I look at the overall perception of educational provision by exploiting the CFPS individual panel. The estimated effect is close to zero, rejecting the first-order role of education quality. I further corroborate this result

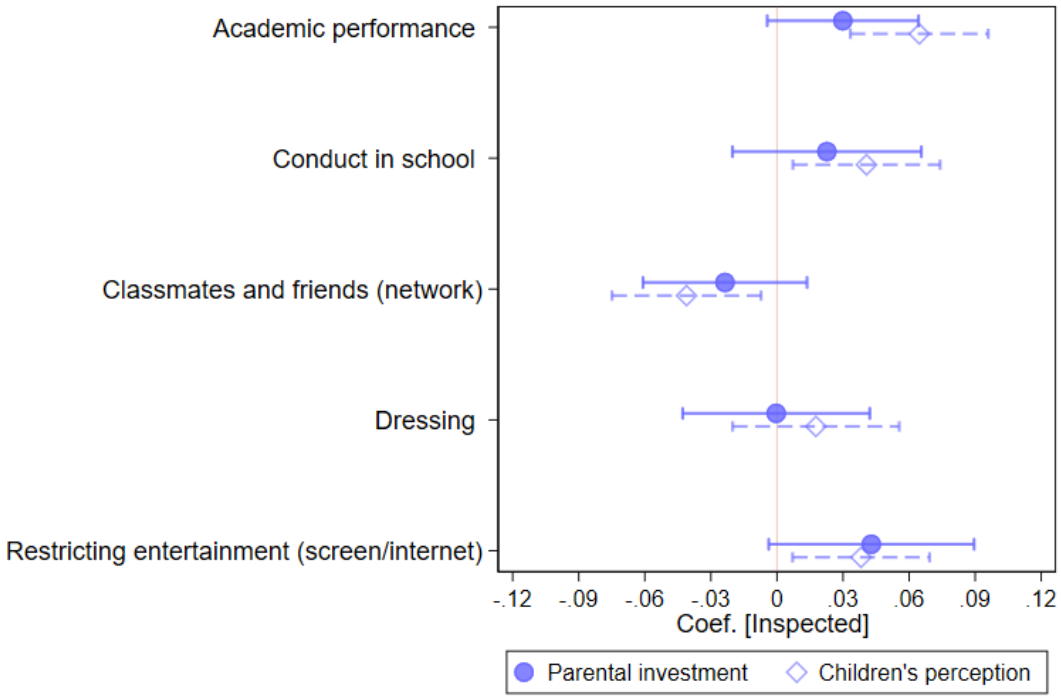
[57] The result may also reflect the vertical transmission of optimism (e.g., "diligence and hard-working pay") from parents induced by the anti-corruption campaign.

by examining the quality of schooling perceived by children in Figure C3. Finally, Columns (5) and (6) show that greater educational investment following anti-corruption inspections is not evoked by changes in parental beliefs about the relative importance of the teacher or parental engagement to their children's achievement.[58]

Taken together, my core findings are unlikely to be purely driven by changes in the quality of schooling. It is important to note that, in the long run, reducing corruption can improve human capital by both the incentivizing effect (direct channel) and improved provision of education (indirect channel). The analysis here only intends to bolster the presence and perhaps the dominant role of the direct channel over the short term.

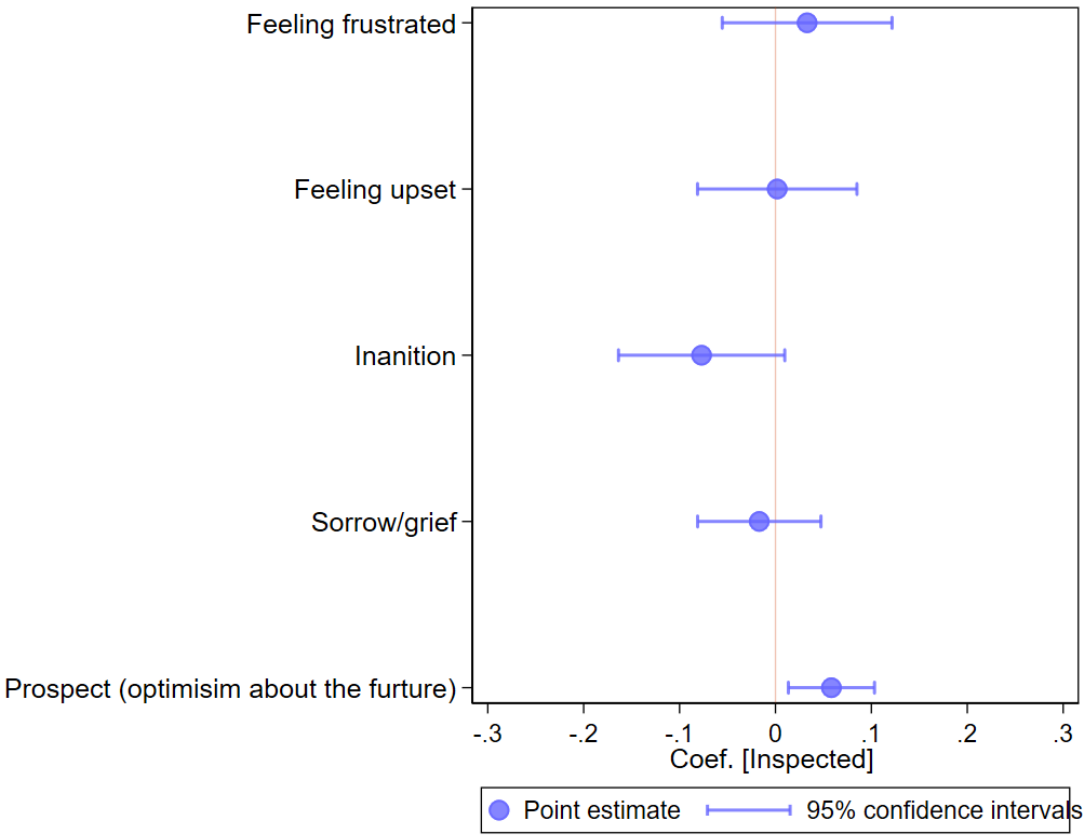
[58] That is to say, the changes in non-pecuniary investment are more in line with the "productivity accumulation" explanation - they are not driven by the case in which parents somehow find their engagement or teachers' inputs more important, compared with other factors.

Figure C1: Corruption crackdown and allocation of parental attention



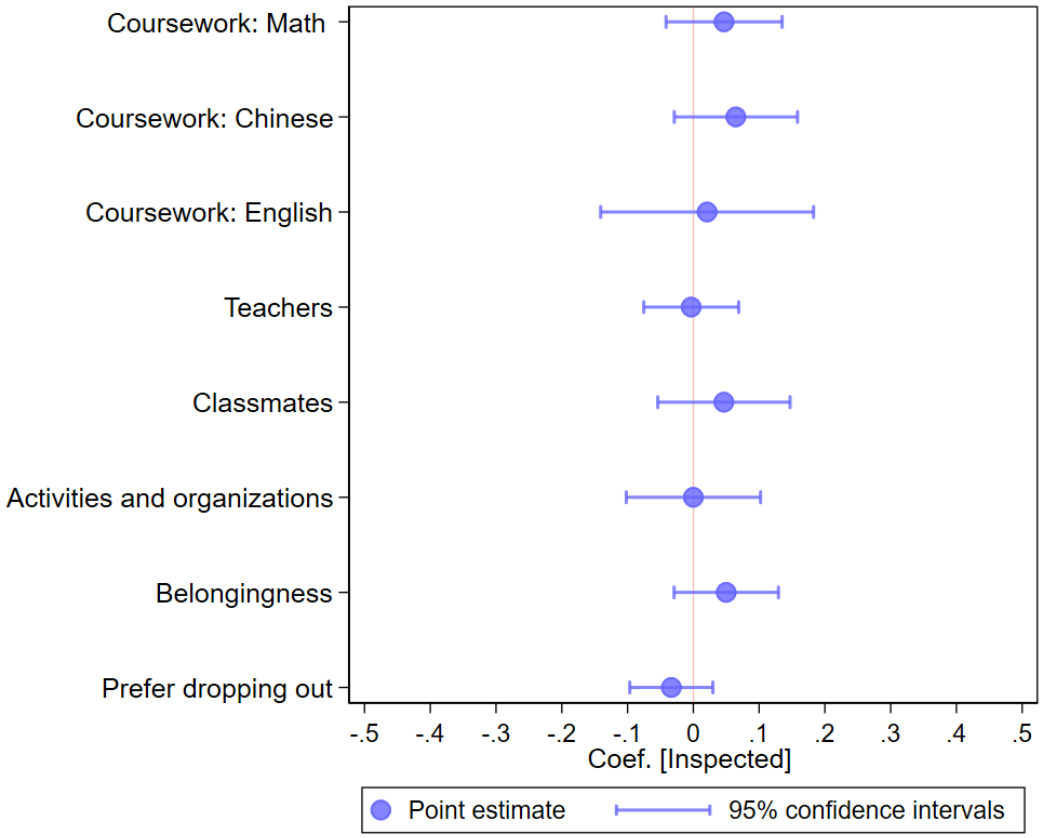
Notes: The figure shows the estimated effects of anti-corruption inspection visits on parental non-pecuniary investment in their children's human capital. The dots visualize the results based on parental responses, and the hollow diamonds visualize the results using children's responses. The 95% confidence intervals are reported using standard errors clustered at the city-cohort-level.

Figure C2: Corruption crackdown and children's subjective well-being



Notes: The figure shows the estimated effects of anti-corruption inspection visits on children's subjective well-being. Each outcome variable is on a 5 point integer scale, where 1 is the lowest ("never") and 5 the highest ("frequent"). The 95% confidence intervals are reported using standard errors clustered at the city-cohort-level.

Figure C3: Corruption crackdown and quality of education
(children's responses)



Notes: The figure shows the estimated effects of anti-corruption inspection visits on the quality of schooling perceived by children. Each outcome variable is on a 4 point integer scale, where 1 is the lowest and 4 the highest. The 95% confidence intervals are reported using standard errors clustered at the city-cohort-level.

Table C1: Corruption crackdown, public education provision and time budgets

	Standardized midterm grade			Parental perception				Working hours per week
	Chinese	Math	English	Teacher's responsiveness	Importance of teacher quality	Importance of parental engagement	Satisfaction with education provision	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of dep. var	0.021	0.018	0.023	4.270	0.625	0.651	5.738	13.997
Inspected	-0.020	-0.022	-0.091	0.052*	-0.001	-0.019	-0.033	-0.776
	(0.049)	(0.032)	(0.073)	(0.029)	(0.011)	(0.013)	(0.080)	(0.961)
Time × Cohort FEs	Y	Y	Y	Y	Y	Y	Y	Y
Household × City × Cohort FEs	Y	Y	Y	Y	Y	Y	-	-
Individual × City × Cohort FEs	-	-	-	-	-	-	Y	Y
Data source	CEPS (2013-2015)						CFPS Individual Panel (2012-2016)	
Observations	38,083	38,077	38,066	37,146	37,336	37,336	569,155	618,093
Adjusted R-squared	0.680	0.691	0.742	0.393	0.278	0.235	0.243	0.351

Notes: "Inspected" is a dummy that is 1 if the city of residence has been inspected by the anti-corruption visit team. "Cohort" denotes a set of dummies for treatment cohorts in the stacked-by-event design. Standard errors are clustered at the city-cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

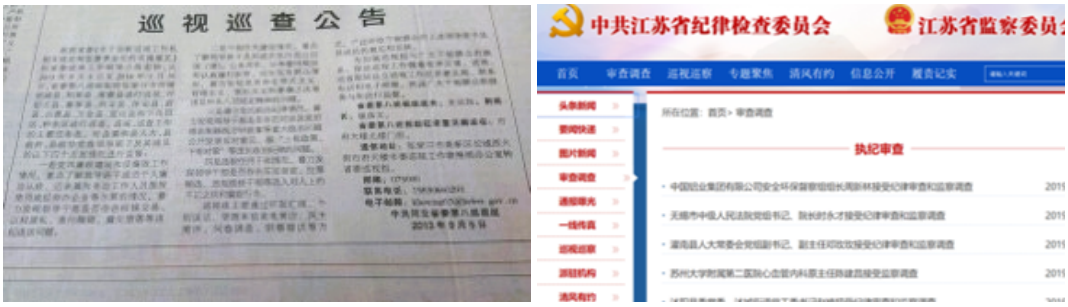
Appendix D Data appendix

Appendix D.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 PCDI. Sample period: 2012-2018. 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 from each province's *Provincial Commission for Discipline Inspection* (PDIC). 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 D1: 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 PDIC website (Jiangsu Province).

Appendix D.2 Household outcomes

The full set of questionnaires of the China Education Panel Survey (CEPS) are available via its official website.[59] Here I present several key questions used in this paper.

[Parent, A30; Child, B19] *What kind of occupation would you expect* [*your child*] *to do in the future? (1) Government official/ bureaucrat; (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.* In the Chinese context (in which education and health sectors are dominated by the state) , I code households reporting (1)(3)(4) as "preferring

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

public jobs" and households reporting (1) as "preferring being government officials". The empirical results are robust to the usage of either measure.

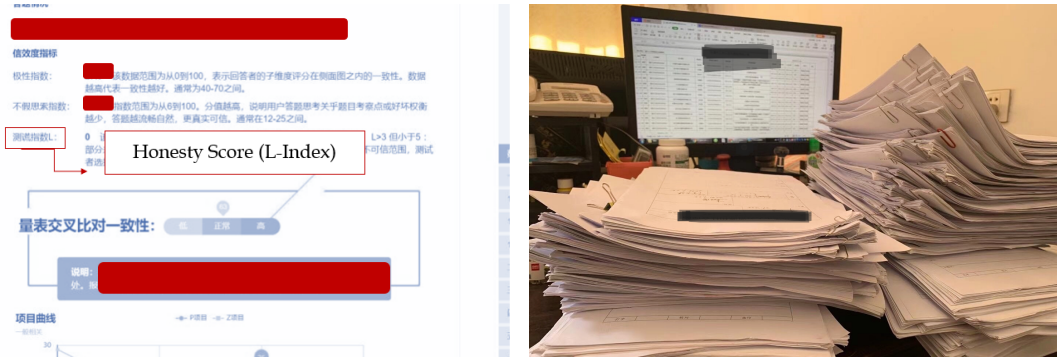
[Parent, A29; Child, B18] *What is the highest level of education do you expect* [*your child*] *to obtain? (1) Drop out now; (2) Junior high school; (3) Vocational secondary school; (4) Vocational high school; (5) Senior high school (7) Junior/vocational college; (8) Bachelor; (9) Master; (10) Doctor.* The expected years of schooling is inferred by the average years of schooling to obtain the corresponding degree in China (e.g., 16 years for a bachelor's degree) .

[Parent, A27-28; Child, D1-2] *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) Emotional control (having a mild temper toward others); (6) Concentration (staying focused); (7) Truancy; (8) Cheating and plagiarism; (9) Delinquent behavior (e.g., smoking, drinking); Others (violating student codes - e.g., going to adult cybercafes).* I average these scores to create a z-score index that acts as a complement proxy for children's integrity/pro-social preferences.

Household socioeconomic status indexes and children's cognitive scores are prepackaged by the CEPS research team and can thus be directly utilized in the analysis.

Appendix D.3 SOE outcomes

Figure D2: Sample of data sources for SOE applicant/employee traits



Notes: The left panel shows a sampled personality test result, which contains a measure of honesty based on one's responses (automatically calculated by the test system using several psychological techniques). This sampled screenshot is provided by the personality test provider; due to confidentiality, the author can only access the summary honesty score of applicants by the city × year cell at the time of this study. The right panel shows a collection of administrative employee assessment reports being digitized.