From Kickbacks to Care: Anti-Corruption Enforcement, Hospital Resource Allocation, and Patient Welfare

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ABSTRACT

Does anti-corruption enforcement in a public-service sector improve resource allocation and welfare for the population it serves? We address this question using staggered investigations of Chinese public hospital officials by the Commission for Discipline Inspection between 2013 and 2024. Using a difference-in-differences design, we estimate the effect of enforcement on local healthcare markets. Hospitals in treated cities expand physical capacity and clinical employment, adding roughly eight beds and four doctors per hospital, and hospital entry increases, especially among smaller institutions. Patients pay less out of pocket and report higher satisfaction with local healthcare. Procurement prices fall by about 20%, and hospitals reorganize their pharmaceutical supplier base toward higher-quality suppliers. The evidence supports a supply-chain financing mechanism: as kickbacks become less viable, pharmaceutical suppliers expand lawful trade credit to hospital clients, relaxing hospitals' working-capital constraints. The findings show that hospital anti-corruption enforcement can reallocate resources from illicit private rents toward productive capacity and patient welfare, and identify a previously unexplored margin of inter-firm finance: substitution from illicit relational payments to lawful trade credit.

Keywords: Hospital Anti-corruption Enforcement; Resource Allocation; Patient Welfare; Healthcare Procurement; Trade Credit

JEL Classification: I11, I18, H51, K42, G32

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1. Introduction

Does anti-corruption enforcement in a public-service sector improve resource allocation and welfare for the population it serves? This question is central to the design of governance reforms, yet the answer remains empirically uncertain. Anti-corruption enforcement may deter rent extraction, lower input prices, and redirect resources toward productive use. At the same time, enforcement may disrupt established supplier relationships, delay procurement decisions, or induce public officials to avoid discretionary actions. These competing forces are especially important in healthcare, where procurement corruption can directly affect hospital capacity, treatment costs, and patients' trust in medical services.

Healthcare-procurement kickbacks are widespread and costly. Globally, 10% to 25% of health spending is estimated to be lost to corrupt practices (Transparency International, 2019), and the OECD (2017) reports that fraud, abuse, and corruption absorb approximately 6% of global health expenditure. In hospital procurement, kickbacks generate private rents for suppliers and procurement officials, but impose costs on hospitals and patients by inflating input prices, distorting supplier selection toward firms willing to pay bribes rather than those offering the best value, diverting supplier-side resources away from the hospital institution, and increasing the financial burden borne by patients. These distortions make healthcare an important setting in which to examine whether anti-corruption enforcement produces real welfare gains rather than merely punishing individual wrongdoers.

Despite the policy importance of this question, causal evidence on the welfare consequences of anti-corruption enforcement in public-service sectors remains limited. Extant healthcare studies examine monitoring or audit interventions in relatively specific settings, such as a single city (Di Tella and Schargrodsky, 2003), a single program (Shi, 2024), or a single disease setting (Bossuroy, Delavallade, and Pons, 2025). The broader anti-corruption literature focuses largely on corporate responses to political corruption, including financial reporting, investment, innovation, and other corporate policies (Smith, 2016; Ellis, Smith, and White, 2020; Hope, Yue, and Zhong, 2020; Griffin, Liu, and Shu, 2022). Much less is known about whether enforcement against corruption inside public-service institutions changes how

those institutions allocate resources and whether the effects reach the people who rely on them. This gap is particularly important because the transmission of enforcement is not mechanical. In procurement-intensive sectors, corruption is not only a transfer of rents; it is also an informal contracting device through which suppliers maintain relationships with public buyers. Thus, enforcement may improve welfare only if it changes the form of supplier competition, reallocating resources from illicit private payments toward lower prices, better inputs, or formal financing terms that benefit the institution and its users.

China provides a particularly informative setting for addressing this question. Public hospitals dominate Chinese healthcare delivery, provide the vast majority of patient care, and dispense roughly 80% of all medications. Historically, these public hospitals operated under regulated low service prices, limited fiscal subsidies, and weak formal financing, creating strong incentives to rely on drug sales and informal supplier relationships to sustain operations. Under these conditions, kickbacks became embedded in procurement relationships between hospital officials and pharmaceutical and medical-device suppliers (Fu, Lai, Li, Zhu, and Yip, 2023; Liu, Chen, and Bao, 2021). Beginning in late 2013, the Central Commission for Discipline Inspection (CCDI) and its provincial counterparts began investigating hospital officials for accepting bribes from suppliers in exchange for procurement contracts, prescription decisions, and other discretionary actions involving public power and public funds. By the campaign's recent count, two-thirds of medical-corruption cases tried between 2013 and 2019 involved supplier kickbacks (Fu et al., 2023; Liang, 2025).

This setting differs from the firm-side and political-corruption settings studied in prior work in two important ways. First, the targets of enforcement are public officials inside public-service institutions (i.e., hospitals) rather than private firms or political elites. Second, the corrupt exchange directly concerns the allocation of healthcare resources, with potential consequences for hospital capacity, procurement costs, patient expenses, and perceived service quality. The staggered rollout of investigations across cities and over time provides useful variation in local exposure to enforcement, supporting a difference-in-differences (DiD) design.

We assemble a multi-level dataset combining information on CCDI investigations of hospital officials, city-level hospital operations and entry, individual-level patient outcomes from the China Family Panel Studies, transaction-level pharmaceutical procurement contracts, and firm-level financial data for pharmaceutical suppliers. Our empirical strategy proceeds in two layers. At the city and province level, we estimate the aggregate regional effect of enforcement on the local healthcare market. We use the term "regional effect" to refer to the aggregate reduced-form response of the local healthcare market, combining changes at investigated hospitals and deterrence responses among local peer hospitals. Thus, it is the policy-relevant quantity for outcomes measured at the city or province level, such as hospital capacity, hospital entry, aggregate trade credit, patient out-of-pocket expenses, and patient satisfaction. Where transaction-, hospital-, and firm-level data permit, we further decompose the effect into a detection channel, operating at directly investigated hospitals, and a deterrence channel, operating among local peer hospitals and related suppliers. This empirical structure allows us to ask three related questions. Does anti-corruption enforcement improve hospital resource allocation across investment, employment, and entry margins? Does enforcement reach patients through lower out-of-pocket expenses and improved perceptions of healthcare quality? And what financing mechanism links enforcement to these real outcomes?

Our findings are consistent with anti-corruption enforcement reducing the kickback wedge in hospital procurement and reallocating resources toward productive uses. Treated cities experience significant expansion in hospital capacity and clinical employment. Relative to control cities, hospitals in treated cities add about eight beds and four doctors per hospital, corresponding to approximately 4.7% and 4.5% of their respective sample means. The number of hospitals also rises by about 0.040 hospital per 10,000 people, corresponding to 11.4% of the sample mean, while the registered capital of newly established hospitals declines by about 3.6% of the sample mean, consistent with the interpretation that enforcement lowers relational entry barriers that had previously favored larger incumbent healthcare providers. Dynamic DiD estimates show no obvious pre-treatment trend and reveal persistent post-enforcement increases in hospital capacity, employment, and entry. The baseline results are robust to using

population-scaled dependent variables, entropy balancing, and modern staggered-DiD estimators, including Callaway and Sant'Anna (2021), Sun and Abraham (2021), and Cengiz et al. (2019), as well as to controlling for investigations of medical regulatory authorities, local corruption culture, and the rollout of centralized volume-based procurement, and to excluding China's four directly administered municipalities. Cross-sectional tests further support the resource-allocation interpretation: the effects are stronger in less-developed cities, cities with weaker initial hospital endowments, and cities with greater public-sector involvement in healthcare, where rent extraction and financing constraints are likely to be more consequential.

We also find that enforcement reaches patients and procurement markets. At the individual level, the share of medical expenses paid out of pocket falls by approximately 11 percentage points, and patient-reported satisfaction with local hospitals and perceived medical quality both increase by about 3% relative to their sample means. Province-level evidence shows a smaller but consistent decline in the aggregate self-paid medical-expense ratio, while coarse province-level mortality measures do not exhibit statistically significant changes. Procurement evidence provides a direct link to the kickback channel. Within the same hospital-supplier pair, medical product, procurement method, and year, unit procurement prices fall by approximately 20% after enforcement. Hospitals also reorganize their supplier portfolios: investigated hospitals add more new suppliers, increase the share of high-quality new suppliers, and replace existing suppliers more actively. The detection-versus-deterrence decomposition is informative. Procurement-price reductions appear at both directly investigated hospitals and peer hospitals, suggesting market-wide deterrence, while supplier-portfolio reorganization is concentrated among investigated hospitals, consistent with direct restructuring of exposed procurement relationships.

We further examine the financing mechanism through which enforcement affects hospital resource allocation. Our central argument is that kickbacks and trade credit both operate within supplier-hospital relationships, but they allocate benefits to different recipients. Kickbacks transfer private benefits to individual procurement officials and distort procurement decisions. By contrast, trade credit transfers liquidity benefits to the hospital institution by extending

payment terms. When enforcement makes kickbacks riskier and less viable as a relational instrument, suppliers may preserve commercial relationships by shifting from illicit private payments toward lawful contractual concessions. In this sense, anti-corruption enforcement does not simply eliminate a corrupt transfer; it changes the form through which suppliers compete for hospital business. The evidence is consistent with this substitution. At the city level, trade credit provided by local pharmaceutical suppliers rises by about 17% relative to the sample mean, and trade credit received by local hospitals rises by about 14%. At the firm level, pharmaceutical firms located in treated cities increase accounts receivable by about 7% relative to the sample mean. Suppliers with prior contracting relationships with hospitals exposed to local enforcement increase their trade credit by about 19% relative to the sample mean after enforcement. Moreover, the firm-level response is concentrated among suppliers with prior contracting relationships with investigated hospitals, while the response among suppliers linked to peer hospitals is positive but smaller and less robust to controls. Together, these patterns suggest that enforcement reallocates supplier-side resources from illicit relational payments toward lawful supply-chain financing, thereby relaxing hospitals' working-capital constraints and supporting their capacity expansion documented in the main results.

This study contributes to three strands of literature. First, it contributes to the literature on anti-corruption enforcement and public-service provision. Prior healthcare studies show that monitoring and audits can reduce waste or improve service delivery in specific settings (Di Tella and Schargrodsky, 2003; Shi, 2024; Bossuroy, Delavallade, and Pons, 2025). The broader anti-corruption literature examines how firms respond to political corruption and anti-corruption campaigns through financial reporting, investment, innovation, and other corporate policies (Smith, 2016; Ellis, Smith, and White, 2020; Hope, Yue, and Zhong, 2020; Griffin, Liu, and Shu, 2022; Tang, 2023; Megginson, Wang, and Xia, 2023). We study a related but distinct setting: enforcement against the corrupt exchange of public power and public funds for private benefit inside a public-service sector. The new evidence from the study shows that such enforcement can reallocate resources from informal rents toward hospital capacity, procurement efficiency, and patient financial relief. The mechanism is also distinct. It is not

simply the recovery of wasteful spending, nor a private firm's response to a changed political environment. Rather, enforcement restructures supplier-hospital contracting by inducing substitution from illicit relational payments to lawful supply-chain financing.

Second, the study contributes to the literature on hospital financing and resource allocation. Prior studies show that hospital investment and service provision depend on financing constraints, reimbursement design, capital-market access, and ownership structure (Calem and Rizzo, 1995; Duggan, 2000; Sloan, Picone, Taylor, and Chou, 2001; Lynch, 2003; Adelino, Lewellen, and Sundaram, 2015; Rossi and Yun, 2024; Gao, Kim, and Sevilir, 2025). In particular, Okeke (2026) shows in a randomized cash-grant experiment that exogenously increasing public-clinic resources raises productivity through investment in physical and human capital and lowers patient prices. Our findings suggest that removing rent extraction can operate as an analogous relaxation of hospitals' financial constraints. The institutional mechanism, however, differs from a direct cash injection. Enforcement redirects supplier-side resources from informal private payments to formal contractual financing, thereby expanding hospitals' effective working capital and supporting capacity growth. This mechanism highlights governance quality as an important determinant of healthcare resource allocation.

Third, the study identifies a margin of trade credit that has not been examined in the existing literature: trade credit as a substitute for an illicit relational instrument. Prior work views trade credit as a substitute for bank credit (Petersen and Rajan, 1997; Cuñat, 2007; Love, Preve, and Sarria-Allende, 2007), a buffer against supplier-side or buyer-side shocks (Boissay and Gropp, 2013; Ersahin, Giannetti, and Huang, 2024), or a commitment device for relationship-specific investment (Dass, Kale, and Nanda, 2015; Chod, Lyandres, and Yang, 2019). Recent studies show that informal networks and community ties shape access to trade credit (Ding et al., 2023; Tomy and Wittenberg-Moerman, 2024), but they do not examine an external enforcement shock that closes an illicit channel and induces formal financing to expand in its place. Our setting allows us to study this substitution directly, showing how trade credit can emerge as a lawful contracting response when enforcement raises the expected cost of illicit private payments.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 introduces the data. Section 4 sets out the empirical strategy and identification. Section 5 defines the variables used in the analyses. Section 6 reports the main results on hospital investment, employment, entry, robustness, and heterogeneity. Section 7 presents evidence on the trade-credit mechanism. Section 8 examines patient outcomes, procurement prices, and changes in hospitals' pharmaceutical supplier base. Section 9 concludes.

2. Institutional Background

2.1 Chinese Public Hospitals, Financing Pressure, and Procurement Kickbacks

Public hospitals dominate China's healthcare delivery system. They provide the vast majority of patient care and dispense about eighty percent of all medications (Liang, 2025). These government-run hospitals are also the primary purchasers of pharmaceuticals and medical devices, substantially outweighing the private hospital sector in procurement volume and market influence. As a result, pharmaceutical and medical-device firms depend heavily on public hospitals for access to the healthcare market. This institutional dominance makes public hospitals the key setting through which procurement corruption, anti-corruption enforcement, and supplier responses can affect healthcare resource allocation and patient outcomes.

Despite their central role, public hospitals historically operated under tight financial constraints. Government subsidies accounted for only about ten percent of public hospital revenue by 2018, while prices for many medical services remained regulated at relatively low levels (U.S.-China Economic and Security Review Commission, 2021). To cross-subsidize operations, hospitals relied heavily on drug sales, including legally permitted markups before the full implementation of zero-markup reforms.[1] Low public funding also translated into modest physician salaries and constrained hospital budgets, creating incentives for both hospitals and their staff to seek alternative sources of revenue. These conditions blurred the

[1] Markups of up to 15% above procurement cost were legally permitted until the phased zero-markup reforms initiated in the late 2000s and completed nationwide for public hospitals by 2017.

boundary between clinical care and commercial activity, fostering practices such as excessive prescribing and supplier kickbacks (Wang, 2017). In short, weak formal financing, regulated service prices, and misaligned incentives created an institutional environment in which off-book rent-sharing arrangements could become embedded in hospital procurement.

Supplier kickbacks became pervasive in such an environment. Prior to the mid-2010s reforms, pharmaceutical and medical-device suppliers routinely provided illicit private benefits, often in the form of cash-filled red envelopes (*hongbao* in Chinese) or under-the-table rebates, to doctors, department heads, and hospital administrators in exchange for high drug utilization, favorable formulary placement, or procurement contracts (Liu, Chen, and Bao, 2021; Fu et al., 2023). These illicit payments enriched individual hospital insiders but distorted procurement decisions and undermined efficiency. Rather than selecting suppliers solely on price, quality, and reliability, hospitals often became tied to suppliers willing to share rents privately with procurement decision-makers.[2] Such arrangements raised hospital procurement costs, weakened supplier competition, and eroded public trust in the medical profession (Shi et al., 2018; Fu et al., 2023). They also diverted supplier-side resources toward private transfers to hospital officials rather than toward contractual terms that could benefit the hospital institution, such as lower prices, higher-quality products, or more favorable payment terms.

The structure of hospital procurement further facilitated these arrangements. Drug and medical-device purchases were historically decentralized and opaque, with many hospitals negotiating individually with suppliers. This fragmentation generated substantial pricing dispersion and information asymmetry, allowing hospital staff to solicit kickbacks and enabling suppliers or sales agents to influence procurement and prescription decisions through cash payments, rebates, gifts, or other illicit private benefits (Fu et al., 2023). Authorities introduced centralized drug-procurement reforms in the mid-2010s to improve transparency and reduce procurement-related corruption. Although these reforms increased formal transparency,

[2] The U.S.-China Economic and Security Review Commission (2021) and Liang (2025) report that seventy-eight percent of Chinese doctors agreed pharmaceutical firms could not compete without paying bribes, and over half admitted to accepting improper benefits.

uneven implementation and monitoring allowed some hospitals and suppliers to preserve rent-seeking opportunities through alternative arrangements. These procurement reforms are important institutional background, and we account for their staggered rollout in the empirical analysis. Our identifying variation, however, comes from the timing of corruption investigations of hospital officials rather than from procurement-policy adoption itself.

This institutional setting is central to the mechanism we study. Supplier kickbacks were not merely isolated illegal payments; they operated as a relational instrument within hospital-supplier contracting. By transferring private benefits to officials with procurement or prescription influence, suppliers could maintain access to hospital demand and protect existing commercial relationships. Once anti-corruption enforcement intensified, these illicit relational arrangements became riskier and less viable. Hospitals and suppliers therefore had incentives to reorganize procurement relationships and financing terms, potentially reallocating supplier-side resources from private kickbacks toward formal concessions to the hospital institution.

2.2 Anti-Corruption Enforcement and Transmission Channels

Curbing entrenched supplier kickbacks in hospital procurement required a stronger enforcement response. Before the 2010s, oversight of corruption in China's healthcare system was fragmented. Health bureaus and professional regulators issued rules, and local authorities sporadically pursued bribery cases, but enforcement was decentralized and often lacked a unified mechanism for investigation and discipline. This changed after the Chinese Communist Party's Central Commission for Discipline Inspection (CCDI) became the central institution responsible for public-sector anti-corruption enforcement, including healthcare. The CCDI and its local branches obtained authority to investigate and sanction corrupt hospital officials, often working with health, insurance, and drug regulators. This enforcement architecture raised the expected cost of participating in supplier kickback arrangements for hospital administrators, physicians with procurement influence, and pharmaceutical and medical-device firms.

The hospital-sector crackdown we study was part of the nationwide anti-corruption campaign launched in late 2012. Healthcare was quickly prioritized because bribery in medical

services and procurement had become widespread (Liu et al., 2021). Beginning in 2013, discipline inspectors targeted public hospitals across provinces, and investigations of hospital presidents, department heads, chief physicians, and other officials with procurement or prescription influence became increasingly common.[3] Bribery by pharmaceutical and medical-device firms was the dominant form of hospital corruption, consistent with the supplier-kickback system described above (Fu et al., 2023). The staggered rollout of investigations across regions and over time provides variation in local exposure to enforcement, which we use to study how anti-corruption enforcement affects hospital resource allocation, supplier contracting, and patient outcomes.

The announcement of a hospital corruption investigation is consequential because it changes expectations within the local supplier-hospital contracting environment. At the investigated hospital, the announcement exposes or disrupts existing corrupt relationships, removes or disciplines hospital officials with procurement authority, and forces the hospital and its suppliers to renegotiate procurement and financing terms. For peer hospitals in the same city, visible enforcement raises the perceived probability and expected cost of detection, making similar kickback arrangements much riskier even without a direct investigation. Enforcement can thus affect not only the investigated hospital but also the broader local healthcare market in which hospitals, suppliers, and regulators interact.

These institutional features imply two transmission channels. The *detection channel* operates at directly investigated hospitals, where anti-corruption enforcement directly disrupts exposed relationships and induces procurement and financing restructuring. The *deterrence channel* operates among non-investigated peer hospitals, where visible enforcement changes beliefs about the risk of maintaining similar supplier kickback arrangements. Because

[3] Court records show that between 2013 and 2019, Chinese courts prosecuted 3,546 healthcare corruption cases, over two-thirds of which involved illicit kickbacks paid by suppliers (Fu et al., 2023). One reported case is Wang Tianchao, former president of Yunnan First People's Hospital, who accepted more than 110 million yuan in bribes from suppliers and contractors in exchange for awarding procurement contracts. By the campaign's recent count, more than forty thousand individuals had been disciplined in the medical-corruption crackdown (Liang, 2025).

investigated hospitals and peer hospitals operate in the same procurement, supplier, and regulatory environment, the welfare-relevant effect of enforcement may extend beyond the investigated hospital to the regional healthcare market. The empirical strategy in Section 4 therefore estimates the aggregate regional effect at the city and province level and, where transaction-, hospital-, and firm-level data permit, decomposes this effect into detection and deterrence components.

3. Data

We construct a multi-level dataset combining information on hospital corruption investigations, hospitals' medical decisions and outcomes, patients' medical outcomes, pharmaceutical firms' trade-credit decisions, and procurement transactions between hospitals and pharmaceutical firms in China. The investigation sample covers 2013 to 2024; outcome data at the firm, transaction, hospital, and individual levels also extend through 2024 where available, to observe post-investigation outcomes. We begin the sample in 2013 because the anti-corruption campaign has mainly emerged since then.

3.1 Anti-Corruption Investigation Data

A distinctive element of our dataset is the hand-collected information on hospital corruption investigations. We gather data on anti-corruption investigations involving hospital officials from official announcements of central, provincial, prefectural, and county-level Commissions for Discipline Inspection.[4] We treat these announcements as the initiation of official investigations, which typically coincide with the suspension or removal of the implicated official. In most cases, investigations culminate in disciplinary action, although we do not always observe the final verdict. We use the announcement date to time the shock because the disclosure itself breaks the prior informal arrangement and immediately affects

[4] Our hand-collection focuses on cases where the implicated party held an administrative or procurement-related (or prescription-making) position at the hospital, since these are the positions through which kickbacks operate.

counterparties' expectations. This information serves as our key treatment variable capturing the anti-corruption shock. We provide anecdotal evidence on hospital corruption investigations in Appendix Table A2. Appendix Table A3 further shows the geographic and temporal distribution of these cases. The provinces with the largest number of hospital corruption cases are Sichuan, Anhui, and Yunnan. The number of cases increases markedly after 2018 and peaks in 2023.

3.2 Medical Data

Because the welfare consequences of anti-corruption enforcement may operate through both investigated hospitals and peer hospitals in the same regional healthcare market, we use city- and province-level data to capture aggregate market responses. Hospital-level operating and patient-outcome data are not comprehensively available, so these regional data also allow us to examine a broader set of welfare-relevant outcomes. City-level data on hospitals' investment, employment, and the number of hospitals are collected from the China City Statistical Yearbooks. Data on hospital entry and exit are obtained from the Business Registration Database, aggregated at the city level for consistency. Province-level data on hospital medical service, including charges and mortality, are collected from the China Health Statistical Yearbooks. Data on regional economic and social characteristics are obtained from the China City Statistical Yearbooks and the China Statistical Yearbooks. In addition, individual-level patient medical expense and evaluation data are collected from the China Family Panel Studies (CFPS), allowing us to examine how anti-corruption enforcement affects individual-level medical quality and cost.

3.3 Trade-Credit Data

Pharmaceutical-firm-level trade-credit data are obtained from the China Stock Market and Accounting Research database (CSMAR). We use accounts receivable scaled by sales to measure trade-credit provision and aggregate firm-level data to construct city-level trade-credit

measures. Other firm-level financial-statement and corporate-governance variables are also drawn from CSMAR.

3.4 Procurement Transaction Data

Hospital transaction-level procurement data are obtained from the China Government Procurement Database, which contains detailed information on procurement contracts between hospitals and pharmaceutical firms, including procurement items, prices, volumes, methods, and other contract terms. We use these transaction data to construct a hospital-level panel of pharmaceutical supplier selection. We further supplement supplier-quality information using official announcements from the National Medical Products Administration on innovative medical products and medical-product recalls.

4. Empirical Strategy and Identification

To identify the effect of hospital corruption investigations on healthcare resource allocation, medical costs, and outcomes across multiple dimensions, we employ a difference-in-differences (DiD) research design. The staggered timing of investigations across regions and years allows us to treat them as quasi-natural experiments. In our main specification, we compare changes in outcomes for cities with investigated hospital administrators (treated cities) against cities without investigated hospital administrators (control cities), before and after the investigation event.

Formally, we estimate:

$$Y_{it} = \alpha + \beta AntiCorrupt_{it} + \Gamma X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (1)$$

where Y_{it} denotes the outcome variable for city *i* in year *t*. $AntiCorrupt_{it}$ is an indicator equal to one for city-years after at least one local hospital official is announced to be under corruption investigation by the Commission for Discipline Inspection, and zero otherwise. X_{it} is a vector of city-level control variables. μ_i and λ_t denote city and year fixed effects, respectively. The coefficient of interest, β, captures the average treatment effect of anti-corruption campaign on the outcome variable. In province-level analyses, *i* denotes province rather than city. In

analyses using transaction-, hospital-, firm-, or individual-level data, we adapt the specification by using the corresponding unit of observation and including the relevant control variables and fixed effects.

Because some regional economic and institutional characteristics may themselves respond to anti-corruption enforcement, specifications that include contemporaneous controls may be subject to a bad-control problem. For example, local governments may adjust fiscal allocations to the healthcare sector after enforcement begins. We therefore treat the specification without contemporaneous controls as our preferred specification and report specifications with controls as robustness checks, interpreting the latter cautiously because some controls may lie on the causal path from enforcement to healthcare outcomes.

Our baseline city-level specification estimates the aggregate regional effect of enforcement on the local healthcare market. As discussed in Section 2.2, this effect combines the detection channel operating at directly investigated hospitals and the deterrence channel operating at non-investigated peer hospitals in the same local environment. The regional effect is the policy-relevant quantity for outcomes measured at the city or province level, including hospital beds, doctors, hospital entry, aggregate trade credit, and patient medical expenses. In transaction- and hospital-level analyses, where directly investigated hospitals and peer hospitals are observable, we decompose the regional effect by replacing *AntiCorrupt* with two indicators, *AntiCorrupt_Detect* and *AntiCorrupt_Deter*, which capture the direct effect at investigated hospitals and the spillover effect at peer hospitals, respectively. A parallel decomposition applies at the firm level, where *AntiCorrupt_SupDetect* and *AntiCorrupt_SupDeter* capture trade-credit responses by suppliers with prior contracting relationships with investigated hospitals and by suppliers linked to peer hospitals, respectively.

To mitigate concerns about pre-event trends and potential negative-weight bias under heterogeneous treatment effects (Goodman-Bacon, 2021), we further estimate dynamic treatment effects using several event-study estimators. We first estimate a conventional two-way fixed-effects event-study model using ordinary least squares (OLS). We then implement modern staggered-adoption estimators, including Callaway and Sant'Anna (2021), Sun and

Abraham (2021), and Cengiz, Dube, Lindner, and Zipperer (2019), which are designed to address treatment-effect heterogeneity across cohorts and over time. Standard errors are clustered at the city level in the city-level analyses and at the corresponding treatment level in other analyses.

5. Variables and Summary Statistics

We organize the dependent variables according to the analytical level at which each test is conducted. Appendix Table A1 provides detailed variable definitions. Continuous variables are winsorized at the 1st and 99th percentiles.

5.1 Dependent Variables

First, to measure the intensive margin of healthcare resource allocation, we use the number of hospital beds per hospital ($NumBed$) and the number of doctors per hospital ($NumDoc$) at the city level. We also use the number of hospital beds per capita ($NumBed2$) and the number of doctors per capita ($NumDoc2$) as alternative measures.

Second, to measure the extensive margin of healthcare resource allocation, we examine hospital entry and exit. Specifically, we use the number of hospitals per capita ($NumHos$), the natural logarithm of the average registered capital of newly established hospitals ($SizeEntry$), and the natural logarithm of the average registered capital of newly closed hospitals ($SizeExit$) at the city level.

Third, to examine patient financial burden and healthcare outcomes, we use both aggregate and individual-level measures. At the province level, we analyze the proportion of self-paid medical expenses in total medical expenses ($SelfExp$), the emergency mortality rate ($EmerDie$), and the observation-room mortality rate ($ObserDie$). At the individual level, using CFPS data, we examine the household's medical expenses scaled by income ($MediExp$), the natural logarithm of individual self-paid medical expenses ($SelfExpAmt$), and the proportion of individual self-paid medical expenses in total medical expenses ($SelfExpRatio$). We also examine individual satisfaction with local hospitals ($MediSatisf$) and individual

evaluation of local hospitals' medical quality ($MediEval$), both measured on five-point Likert scales.

Fourth, to test the trade-credit mechanism, we examine accounts receivable of pharmaceutical suppliers. The firm-level dependent variable $AccRe$ is accounts receivable scaled by total sales, which captures suppliers' provision of trade credit. We aggregate this measure at the pharmaceutical firm's city level to construct the city-level trade-credit-provision variable $AR_LocalSup$. We also aggregate trade credit from hospitals' recent pharmaceutical suppliers at the hospital-city level to construct the city-level trade-credit-receipt variable $AR_LocalHos$.

Finally, to examine procurement unit prices and changes in hospitals' pharmaceutical supplier base, we use transaction- and hospital-level measures from procurement data. At the transaction level, we use the unit price of procurement contracts ($UnitPrice$). At the hospital level, we construct several measures of supplier-base changes, including the number of new pharmaceutical suppliers ($NumNewSup$), the share of high-quality and low-quality new suppliers ($NewSupHQ$ and $NewSupLQ$), and the number of withdrawn suppliers ($NumEndSup$).

5.2 Control Variables

We include a comprehensive set of regional characteristics as control variables in robustness specifications. At the city level, we control for GDP growth ($GDPGrowth$), which captures local macroeconomic conditions; the natural logarithm of population ($Population$), which captures the scale of healthcare demand; and the share of value-added output from the primary industry ($PriInd$), which proxies for the degree of urbanization and local economic structure. We also control for the natural logarithm of local government public fiscal revenue ($PubRev$) and the local fiscal deficit ratio ($Deficit$), given the central role of public hospitals in China's healthcare system. We additionally include the provincial public healthcare expenditure as a share of total fiscal expenditure ($PubMediExp$) and the provincial

marketization index ($MarketIdx$), which captures variation in market development, private-sector participation, and institutional conditions.[5]

5.3 Summary Statistics

Table 1 provides descriptive statistics. At the city level, hospitals have, on average, about 178 beds and 99 doctors per hospital. The average number of hospitals is 0.35 per 10,000 residents, or approximately 3.5 hospitals per 100,000 residents. About 46% of city-year observations are exposed to a local hospital corruption investigation during the sample period. At the province level, the average proportion of self-paid medical expenses is 0.280, while the emergency and observation-room mortality rates average 8.9% and 18.2%, respectively.

At the individual level, household medical expenses account for 17.7% of household income on average, and the average individual self-paid share of total medical expenses is 43.3%. Satisfaction with local hospitals averages 3.16 out of 5, and perceived medical quality averages 3.14 out of 5. At the firm level, the average ratio of accounts receivable to sales is 0.57. At the transaction level, the mean log unit price is 8.98, corresponding to approximately RMB 7,900 per standardised unit. At the hospital level, the mean log of one plus the number of first-time pharmaceutical suppliers is 0.27.

[Please Insert Table 1 here]

6. Empirical Results

This section presents the main results on hospital resource allocation. Section 6.1 examines hospital investment and employment. Section 6.2 reports dynamic treatment effects, Section 6.3 reports robustness tests, Section 6.4 examines cross-sectional heterogeneity, and Section 6.5 examines hospital entry and exit. The motivation for examining these outcomes is that public hospitals in China have historically operated under financial constraints, while supplier kickbacks may have imposed additional costs on hospital operations through inflated procurement prices, distorted supplier selection, and rent extraction by procurement officials.

[5] City-level public healthcare expenditure is not consistently available for the full sample.

Anti-corruption enforcement (hereafter referred to as the campaign) targeting hospital officials may weaken the kickback wedge embedded in procurement relationships, thereby freeing resources for productive hospital use. It may also reduce relational entry barriers that had favored incumbent providers with established supplier and official connections. These mechanisms are consistent with prior evidence showing that hospital investment is sensitive to financing constraints and resource availability (Calem and Rizzo, 1995; Adelino, Lewellen, and Sundaram, 2015; Okeke, 2026). We test these implications below.

6.1 Hospital Investment and Employment

Table 2 presents the DiD estimates of the effect of anti-corruption enforcement on hospital investment and employment at the city level. The first two columns report results on hospital investment, proxied by the number of hospital beds per hospital. Column 1 includes city and year fixed effects but no time-varying controls. The coefficient estimate on $AntiCorrupt$ is positive and statistically significant at the 1% level ($\beta = 8.294, t = 2.671$). Column 2 adds the full vector of region-level controls, and the coefficient estimate remains positive and significant at the 5% level. Given the potential bad-control concern discussed in Section 4, we treat Column 1 as the preferred specification. The estimate implies that hospitals in treated cities add roughly eight beds per hospital after the campaign, equivalent to approximately 4.7% of the sample mean.

The last two columns report results on hospital employment, proxied by the number of doctors per hospital. Column 3 includes city and year fixed effects but no time-varying controls. The coefficient estimate on $AntiCorrupt$ is positive and significant at the 5% level ($\beta = 4.441, t = 2.299$). Column 4 adds the full vector of region-level controls, and the coefficient estimate remains positive and significant at the 10% level. Interpreting Column 3 as the preferred specification, the estimate implies that hospitals in treated cities add roughly four doctors per hospital after the campaign, equivalent to about 4.5% of the sample mean. Overall, the results indicate that anti-corruption enforcement is followed by meaningful increases in hospital physical capacity and clinical employment.

[Please Insert Table 2 here]

6.2 Dynamic Treatment Effects and Staggered-DiD Estimators

The baseline estimates reported in Tables 2 identify the average effect of local exposure to hospital anti-corruption enforcement. In this subsection, we examine the dynamic pattern of the effect and address two related concerns. First, the validity of the DiD design requires that treated and control cities exhibit no differential trends before the enforcement event. Second, because treatment timing is staggered across cities, conventional two-way fixed-effects (TWFE) estimators may produce biased estimates when treatment effects are heterogeneous across treatment cohorts or over event time, and may place negative weights on some treated observations (Goodman-Bacon, 2021).

To address these concerns, we estimate dynamic treatment effects using several event-study estimators. We first estimate a conventional TWFE event-study model using OLS. We then implement several modern staggered-adoption estimators, including Callaway and Sant'Anna (2021), Sun and Abraham (2021), and Cengiz, Dube, Lindner, and Zipperer (2019). These estimators are designed to accommodate treatment-effect heterogeneity across cohorts and over time. The coefficients are estimated relative to the year immediately preceding the first local hospital corruption investigation.

Figure 1 reports the dynamic estimates for two resource-allocation outcomes in Panels (a)-(b): hospital beds per hospital and doctors per hospital. Across the alternative estimators, the pre-treatment coefficients are close to zero and statistically insignificant, providing no evidence of differential pre-trends between treated and control cities before the campaign. After local exposure to enforcement begins, the coefficient estimates become positive and persistent for hospital beds and doctors. These dynamic effects support the interpretation that the hospital resource-allocation effects emerge after, rather than before, local hospital anti-corruption enforcement.

[Please Insert Figure 1 here]

6.3 Robustness Tests

We further conduct several additional robustness tests to examine whether the baseline resource-allocation results are sensitive to alternative outcome definitions, observable differences between treated and control cities, related enforcement actions, concurrent healthcare reforms, or administratively distinctive regions. Table 3 reports the results.

Alternative dependent variables

The baseline dependent variables in Table 2 measure hospital beds and doctors per hospital. One concern is that these results may partly reflect changes in the number of hospitals. To address this potential concern, we replace the dependent variables with population-scaled measures: hospital beds per capita (10,000 people) and doctors per capita. The coefficient estimates on $AntiCorrupt$ remain positive and statistically significant at the 5% level, indicating that treated cities experience increases in healthcare resources not only per hospital but also relative to local population size (Columns 1 and 2 of Table 3).

Entropy balancing

Treated and control cities may differ in observable pre-treatment characteristics. To improve comparability, we use entropy balancing to reweight the control group so that observable characteristics are more closely aligned with those of treated cities. The coefficient estimates on $AntiCorrupt$ remain positive and significant at the 1% level in the entropy-balanced sample, suggesting that the baseline results are not driven by observable differences between treated and control cities (Columns 3 and 4 of Table 3).

Controlling for investigations of medical regulatory authorities

Another concern is that the estimated effect of anti-corruption investigations may capture broader enforcement against healthcare regulators rather than enforcement targeting hospitals themselves. We therefore control for an analogous DiD indicator for investigations of local medical regulatory authorities, $AntiCorrupt_Reg$. Columns 5 and 6 of Tabel 3 show that the coefficient estimates on $AntiCorrupt$ remain positive and significant at least at the 5% level, indicating that the baseline results are not explained by enforcement actions targeting hospitals'

regulators. By contrast, the coefficient estimates of $AntiCorrupt_Reg$ are insignificantly different from zero.

Controlling for local corruption culture

Hospital investigations may also proxy for broader local corruption culture or general anti-corruption intensity. To address this concern, we control for local corruption culture, $LocalCorrupt$, measured by public-official corruption cases scaled by population. The coefficient estimates on $LocalCorrupt$ are positive and significant, indicating that cities with more recorded public-official corruption cases tend to have greater hospital capacity and employment after conditioning on city and year fixed effects. Importantly, the coefficient estimates on $AntiCorrupt$ remain positive and significant at least at the 5% level and are similar in magnitude to the baseline estimates, suggesting that the baseline results are not simply capturing broader local corruption culture or general anti-corruption enforcement intensity (Columns 7 and 8 of Table 3).

Controlling for centralized volume-based procurement

China's centralized volume-based procurement reform may affect hospital procurement prices, supplier relationships, and resource allocation. To address this potential confounder, we control for the staggered rollout of centralized volume-based procurement, $CVBP$. The coefficient estimates on $AntiCorrupt$ remain positive and significant at least at the 5% level, indicating that the baseline results are not driven by this procurement-policy reform (Columns 9 and 10 of Table 3).

Excluding directly administered municipalities

Finally, we examine whether the baseline results are driven by China's four directly administered municipalities: Beijing, Tianjin, Shanghai, and Chongqing. These municipalities have distinct administrative status, concentrated top-tier medical resources, greater political visibility, and potentially different enforcement intensity and hospital financing structures. After excluding these four municipalities, the coefficient estimates on $AntiCorrupt$ remain positive and significant at least at the 5% level, suggesting that the baseline results are not driven by these special administrative regions (Columns 11 and 12 of Table 3).

[Please insert Table 3 here]

Overall, the results in this section support the baseline finding that local exposure to hospital anti-corruption enforcement is followed by increased hospital investment and employment. The results are robust to alternative outcome definitions, reweighting based on observable characteristics, controls for related enforcement actions and concurrent procurement reforms, controls for local corruption culture, and the exclusion of administratively distinctive municipalities.

6.4 Heterogeneity Tests

We next examine whether the baseline resource-allocation effects vary across cities with different institutional, economic, and healthcare-market characteristics. These tests help assess whether the effects are stronger in settings where hospitals are more resource-constrained and/or where public hospitals play a larger role. Table 4 reports the results.

COVID-19 pandemic

The COVID-19 pandemic placed substantial pressure on hospitals and increased demand for medical resources. If the campaign relaxes hospital resource constraints, its effect could be more pronounced during the pandemic period, when hospital capacity was especially strained. However, the pandemic also created substantial adjustment frictions that may have limited treated hospitals' ability to expand measured capacity and employment in response to the campaign. In particular, pandemic-period expansion may have been constrained by staffing shortages, supply-chain disruptions, emergency allocation rules, and restrictions on normal hospital operations. Therefore, the incremental effect of anti-corruption enforcement need not be stronger during the COVID-19 period. To examine this issue, we interact $AntiCorrupt$ with an indicator for the COVID-19 period, $COVID$. Columns 1 and 2 of Table 4 show that the interaction terms are not statistically significant. Thus, we do not find evidence that the campaign's effect on hospital beds or doctors differs significantly between the COVID-19 period and other years.

Local economic development

The effects of anti-corruption enforcement are likely to be stronger in less-developed cities, where public hospitals tend to face tighter fiscal and financial constraints. In more-developed cities, local governments generally have stronger fiscal capacity, and public hospitals may have better access to financial and administrative resources. As a result, reducing procurement-related rent extraction should have a larger marginal effect on hospital resource allocation in less-developed cities. We therefore interact $AntiCorrupt$ with an indicator for cities with above-median GDP per capita in 2013, $HighGDP$. The results in Columns 3 and 4 of Table 4 support this prediction. The interaction terms are negative and significant, indicating that the capacity-expansion effects of the campaign are concentrated primarily in less-developed cities.

Initial hospital endowment

We next examine whether the campaign has stronger effects in cities with weaker initial hospital resource endowments. If anti-corruption enforcement relaxes hospital resource constraints, the marginal effect should be larger where baseline capacity is lower. We thus interact $AntiCorrupt$ with indicators for cities with above-median hospital beds per capita or doctors per capita in 2013, $HighBed$ and $HighDoc$. The results in Columns 5 to 8 of Table 4 generally support this prediction. The interaction terms with $HighBed$ and $HighDoc$ are all negative and are statistically significant in three of the four specifications, indicating that the campaign's capacity-expansion effects are more pronounced in cities with lower initial hospital resource endowment. Overall, the evidence suggests that anti-corruption enforcement has stronger resource-allocation effects where initial hospital capacity is weaker.

Public-sector involvement in healthcare

Because the campaign targets corruption involving public hospitals and public healthcare resources, its effects should be stronger where the public sector plays a larger role in local healthcare provision. We test this prediction by interacting $AntiCorrupt$ with indicators for cities with higher public-sector involvement in healthcare, measured by high public healthcare expenditure share or high public-hospital presence, $HighGovInv$ and $HighPubHos$. The results in Columns 9 to 12 of Table 4 support this prediction. The interaction terms are positive

and significant, indicating that the campaign's effect on hospital resource allocation is stronger in cities where public-sector healthcare is more important. This pattern is consistent with the interpretation that the enforcement shock operates primarily through public hospitals and public healthcare procurement.

[Please Insert Table 4 here]

To summarize, the heterogeneity tests reinforce the interpretation that anti-corruption enforcement improves hospital resource allocation by relaxing constraints and reducing procurement-related distortions. The effects are stronger in less-developed cities, cities with weaker initial hospital capacity, and cities with greater public-sector involvement in healthcare. These findings are consistent with the view that anti-corruption enforcement matters most where rent extraction and resource constraints are likely to be more consequential.

6.5 Hospital Entry and Exit

Appendix Table A4 presents the DiD estimates on hospital entry and exit. Because public hospitals are supported by local government funds and rarely exit the health sector, entry and exit are mainly observed among private hospitals. The first two columns report results for the number of hospitals per capita. Column 1 includes city and year fixed effects but no time-varying controls. The coefficient estimate on $AntiCorrupt$ is positive and significant at the 10% level ($\beta = 0.040, t = 1.924$). Column 2 adds the full vector of region-level controls, and the coefficient estimate remains positive and significant at the 10% level. Using Column 1 as the preferred specification, the estimate implies an increase of roughly 0.040 hospital per capita (10,000 people), equivalent to about 11.4% of the sample mean.

The last four columns examine the size of entering and exiting hospitals, proxied by registered capital. Columns 3 and 5 include city and year fixed effects but no time-varying controls. The coefficient estimate on $AntiCorrupt$ is negative and statistically significant at the 5% level for the size of newly established hospitals ($\beta = -0.208, t = -2.476$), while the coefficient for the size of exiting hospitals is statistically insignificant ($\beta = 0.181, t = 1.326$). Columns 4 and 6 add the full vector of region-level controls, and the coefficient estimates

remain similar. Interpreting Column 3 as the preferred specification, the estimate implies that the registered capital of newly established hospitals, on average, declines by 0.191 after the campaign, corresponding to about 3.6% of the sample mean. These results suggest that anti-corruption enforcement is followed by greater hospital entry, particularly by smaller entrants, consistent with a reduction in relational entry barriers in local healthcare markets.

Panels (a) and (b) of Appendix Figure A1 report the corresponding dynamic estimates for the number of hospitals and the size of newly established hospitals. The pre-treatment coefficients show no obvious differential trends, while the post-treatment coefficients increase persistently after enforcement begins for the number of hospitals. The coefficient estimates for the size of newly established hospitals become negative after enforcement, consistent with the entry of smaller hospitals. These dynamic patterns are consistent with the interpretation that the hospital entry effects emerge after, rather than before, local hospital anti-corruption enforcement.

7. Trade Credit as a Financing Mechanism

We next examine a financing mechanism through which anti-corruption enforcement may affect the documented resource-allocation outcomes. Our argument is that anti-corruption enforcement changes the supplier-hospital contracting relationship by making kickbacks less viable as a relational instrument in procurement. When illicit private payments to individual officials become more costly, pharmaceutical suppliers may preserve hospital relationships by competing through more favorable formal contracting terms, including trade credit.

This substitution matters because kickbacks and trade credit allocate benefits to different recipients. Kickbacks transfer private benefits to hospital officials and distort supplier selection. Trade credit, by contrast, provides liquidity benefits to hospitals by extending payment terms. An increase in trade-credit provision after enforcement is therefore consistent with a reallocation of supplier-side resources from illicit relational payments toward lawful supply-

chain financing.[6] By easing hospitals' working-capital constraints, this expansion in trade credit provides a plausible channel through which enforcement supports the documented hospital capacity expansion.

7.1 Aggregate City-Level Trade-Credit Responses

We first examine the trade-credit mechanism at the city level, which captures the aggregate responses of local pharmaceutical suppliers and local hospitals. We construct two measures: $AR_LocalSup$ captures trade credit provided by pharmaceutical firms located in the city, while $AR_LocalHos$ captures trade credit received by local hospitals from their pharmaceutical suppliers.

Table 5 reports the results. Columns 1 and 2 examine $AR_LocalSup$. The coefficient estimate on $AntiCorrupt$is positive and significant ($\beta = 0.040, t = 2.617$), indicating that local pharmaceutical firms on average provide more trade credit after the campaign. Relative to the sample mean, this estimate implies an increase of approximately 17%. Columns 3 and 4 examine $AR_LocalHos$. The coefficient estimate on $AntiCorrupt$ is again positive and significant ($\beta = 0.053, t = 2.023$), indicating that local hospitals receive more trade credit from their pharmaceutical suppliers after the campaign. Relative to the sample mean, the estimate implies an increase of approximately 14%.

Panels (a) and (b) of Figure 2 report the corresponding dynamic estimates for trade credit provided by local pharmaceutical suppliers and trade credit received by local hospitals. The pre-treatment coefficients show no obvious differential trends, while the post-treatment coefficients increase persistently after enforcement begins. These dynamic patterns are

[6] We do not argue that kickbacks and trade credit are equivalent instruments. Kickbacks are illicit private transfers to individual officials that distort procurement decisions, whereas trade credit is a lawful contractual concession that provides liquidity to the hospital institution. The substitution we examine operates at the level of supplier-hospital relationships: when enforcement constrains the use of informal payments as a means of sustaining procurement relationships, suppliers may shift toward formal financing terms that preserve commercial ties while relaxing hospitals' working-capital constraints.

consistent with the interpretation that trade credit expands following local exposure to hospital anti-corruption enforcement.

[Please Insert Table 5 and Figure 2 here]

7.2 Supplier-Level Trade-Credit Responses

We further examine the trade-credit mechanism at the pharmaceutical-firm level. The city-level tests capture aggregate local responses, but they do not directly show whether the increase in trade credit is concentrated among suppliers connected to treated (investigated) hospitals. The firm-level analysis allows us to use suppliers' prior contracting relationships with hospitals to distinguish between suppliers linked to directly investigated hospitals and suppliers linked to peer hospitals in the same local market.

We first examine suppliers' overall trade-credit provision using $AccRe$, defined as accounts receivable scaled by sales. Columns 1 and 2 of Table 6 show that the coefficient estimate on $AntiCorrupt$ is positive and significant ($\beta = 0.042, t = 2.259$), indicating that pharmaceutical firms located in treated cities on average increase trade-credit provision after the campaign. Relative to the sample mean, the estimate implies an increase of about 7%.

We then exploit procurement relationships to construct $AntiCorrupt_Sup$, which identifies suppliers with prior contracting relationships with hospitals exposed to local anti-corruption enforcement (i.e., hospitals located in treated cities). Columns 3 and 4 show that the coefficient estimate on $AntiCorrupt_Sup$ is positive and significant ($\beta = 0.110, t = 2.293$), indicating that suppliers with prior relationships with hospitals in treated local markets subsequently provide more trade credit after enforcement. Relative to the sample mean, the estimate implies an increase of about 19%.

Finally, we decompose $AntiCorrupt_Sup$into two indicators: $AntiCorrupt_SupDetect$ captures suppliers with prior contracting relationships with directly investigated hospitals, while $AntiCorrupt_SupDeter$ captures suppliers linked to peer hospitals in the same city. Columns 5 and 6 show that the increase in trade credit is concentrated among suppliers previously connected to directly investigated hospitals: the coefficient estimate on

$AntiCorrupt_SupDetect$ is positive and highly significant at the 1% level in both specifications. The coefficient estimate on $AntiCorrupt_SupDeter$ is also significantly positive at the 10% level, but becomes statistically insignificant once controls are added. These findings suggest that the trade-credit responses are strongest where enforcement directly disrupts prior supplier-hospital relationships.

[Please Insert Table 6 here]

In summary, the city-level and firm-level results support the trade-credit mechanism. Anti-corruption enforcement is followed by an increase in formal trade credit provided by pharmaceutical suppliers, especially among suppliers with prior relationships with directly investigated hospitals. This evidence is consistent with suppliers substituting away from illicit private payments toward lawful supply-chain financing, thereby relaxing hospitals' working-capital constraints and supporting the resource-allocation effects documented in Section 6.

8. Patient Outcomes, Procurement Prices, and Supplier-Base Changes

8.1 Patient Medical Expenses, Satisfaction, and Mortality

We next examine whether the resource-allocation and financing effects documented above reach patients. Tables 7 and 8 report DiD estimates on patient medical expenses, satisfaction, and mortality. Table 7 uses individual-level data from the China Family Panel Studies, covering the period from 2014 to 2022 at a biennial frequency. Table 8 uses province-level data from the China Health Statistical Yearbooks.

The first six columns of Table 7 examine patient medical expenses at the individual level. We use three measures: household medical expenses scaled by household income (*MediExp*), the natural logarithm of individual self-paid medical expenses (*SelfExpAmt*), and the share of individual self-paid medical expenses (*SelfExpRatio*). Across specifications with and without controls, the coefficient estimates on $AntiCorrupt$ are negative and statistically significant. In the preferred specifications, the coefficient is -0.017 for household medical expenses scaled by income ($t = -1.772$), -0.206 for individual self-paid medical expenses ($t = -2.874$), and -0.111 for the individual self-paid expense ratio ($t = -7.882$). These estimates indicate

that local exposure to hospital anti-corruption enforcement is associated with a meaningful reduction in patients' financial burden. In particular, the individual self-paid expense ratio declines by about 11 percentage points after the campaign, which is equivalent to about 26% of the sample mean.

The last four columns of Table 7 examine patient satisfaction and perceived healthcare quality. The coefficient estimates on $AntiCorrupt$ are positive and statistically significant for both individual satisfaction with local hospitals ($\beta = 0.086, t = 2.148$) and individual evaluation of local hospitals' medical quality ($\beta = 0.086, t = 2.000$). Relative to the sample means of 3.155 and 3.141, respectively, these estimates imply average increases of about 3%. These results suggest that the reduction in patient financial burden is accompanied by improved patient perceptions of local healthcare quality.

Table 8 provides aggregate province-level evidence. The first two columns examine the proportion of self-paid medical expenses in total medical expenses (*SelfExp*). Because this variable is available only through 2022, the sample ends in that year. The coefficient estimate on $AntiCorrupt$ is negative and statistically significant ($\beta = -0.013, t = -2.563$), implying a 1.3 percentage-point decline in the aggregate self-paid expense ratio, or approximately 4.6% relative to the sample mean. This province-level evidence is consistent with the individual-level result that patients bear a lower out-of-pocket burden after local exposure to enforcement.

The last four columns of Table 8 examine mortality outcomes at the province level, proxied by emergency mortality and observation-room mortality. The coefficient estimates on $AntiCorrupt$ are statistically insignificant. We interpret these results cautiously. Province-level mortality measures are coarse and aggregate across all hospitals within a province, which may attenuate variation in local enforcement timing across cities. Moreover, credible detection of mortality effects would require more granular patient- or hospital-level clinical outcome data, which are not available in our setting. Thus, the evidence supports improvements in patients'

financial burden and perceived healthcare quality, but does not allow us to draw strong conclusions about clinical mortality effects.[7]

[Please Insert Tables 7 and 8 here]

8.2 Procurement Prices and Supplier-Base Changes

We next examine whether the decline in patient medical expenses is accompanied by changes in hospital procurement prices and supplier relationships. Procurement prices provide a direct test of the kickback channel: if enforcement reduces rent extraction embedded in hospital procurement, hospitals should pay lower prices for comparable medical products. We use detailed transaction-level procurement data between hospitals and pharmaceutical firms and focus on the (natural logarithm of) unit transaction price, $UnitPrice$. To compare prices for similar purchases, we include hospital-supplier pair, procurement item, procurement method, and year fixed effects. This specification compares price changes within the same hospital-supplier-pair relationship, for the same medical product and procurement method, before and after local exposure to enforcement.

Columns 1 and 2 of Table 9 report the results. The coefficient estimate on $AntiCorrupt$ is negative and statistically significant ($\beta = -0.227, t = -2.505$), implying that procurement prices fall by about 20% on average after the campaign. We then decompose $AntiCorrupt$ into $AntiCorrupt_Detect$, which captures transactions at directly investigated hospitals, and $AntiCorrupt_Deter$, which captures transactions at peer hospitals in the same city. Columns 3 and 4 show that both coefficient estimates are negative and significant at least at the 10% level. This findings suggest that anti-corruption enforcement lowers procurement prices not only at directly investigated hospitals but also at peer hospitals, consistent with a local deterrence effect in the procurement market.

[Please Insert Table 9 here]

[7] Province-level mortality is reported as an aggregate across all hospitals in a province, which attenuates the city-level variation in enforcement timing that underlies our identification strategy. Credibly detecting mortality effects would require finer-grained outcome data, ideally at the hospital-month level, which are not available in our setting.

Finally, we investigate whether hospitals adjust their pharmaceutical supplier base after enforcement. Patient satisfaction may improve not only because patients pay less, but also because hospitals reorganize supplier relationships toward higher-quality suppliers. Because patient-level medication records are not available, we use transaction-level procurement data to construct a hospital-level panel dataset on pharmaceutical supplier changes. We focus on the number of new pharmaceutical suppliers (*NumNewSup*), the share of high-quality new suppliers (*NewSupHQ*), and the share of low-quality new suppliers (*NewSupLQ*). We classify a pharmaceutical firm as high quality if it has innovative medical products approved by the National Medical Products Administration, and as low quality if it has medical products recalled by the National Medical Products Administration.

Columns 1 to 6 of Table 10 report the results for new suppliers. The coefficient estimates on $AntiCorrupt_Detect$ are positive and statistically significant at the 1% level in Columns 1 and 2, indicating that directly investigated hospitals add more new pharmaceutical suppliers after enforcement. The coefficient of 0.461 implies an increase of approximately 59% in (one plus) the number of new pharmaceutical suppliers. The share of high-quality new suppliers increases by 0.4 percentage points, which is large relative to the baseline mean of 0.2%. The share of low-quality new suppliers declines, although the coefficient estimate is statistically insignificant. These results suggest that investigated hospitals actively reorganize their supplier base and are more likely to add higher-quality pharmaceutical suppliers after enforcement. By contrast, the coefficient estimates on $AntiCorrupt_Deter$ are statistically insignificant, suggesting limited evidence of supplier-base restructuring among peer hospitals.

We also examine supplier withdrawals. Columns 7 and 8 of Table 10 show that (one plus) the number of withdrawn pharmaceutical suppliers at investigated hospitals increases by about 17%. This result is consistent with the interpretation that enforcement disrupts existing supplier relationships and induces directly investigated hospitals to restructure their supplier base.

[Please Insert Table 10 here]

To summarize, the supplier-base evidence complements the procurement-price results. Anti-corruption enforcement is associated not only with lower procurement prices but also with

active reorganization of hospital-supplier relationships. The evidence is strongest for directly investigated hospitals, with more limited evidence of spillovers to peer hospitals.

9. Conclusion

This paper examines whether anti-corruption enforcement in a public-service sector improves resource allocation and welfare for the population it serves. Using the staggered onset of corruption investigations of Chinese hospital officials between 2013 and 2024, we show that enforcement has real effects on local healthcare markets. Treated cities experience increases in hospital physical capacity and clinical employment, adding roughly eight beds and four doctors per hospital. The number of hospitals also rises, with hospital entry concentrated among smaller institutions, consistent with a reduction in relational entry barriers that had previously favored incumbents. At the same time, procurement prices fall, hospitals reorganize their pharmaceutical supplier base, patients pay less out of pocket, and patient-reported satisfaction with local healthcare improves.

The evidence points to a supply-chain financing mechanism. Supplier kickbacks transfer private benefits to hospital insiders and distort procurement decisions. Once enforcement raises the expected cost of these illicit private payments, pharmaceutical suppliers shift toward lawful contractual concessions, particularly trade credit. We find that trade credit provided by pharmaceutical suppliers increases after enforcement, especially among suppliers with prior relationships with directly investigated hospitals. This pattern is consistent with substitution from illicit relational payments to lawful supply-chain financing, which relaxes hospitals' working-capital constraints and helps explain the hospital capacity expansion documented in the main results.

The findings contribute to three literatures. For the literature on anti-corruption enforcement, the study shows that enforcement can generate welfare-relevant effects beyond disciplining individual wrongdoers. In our setting, enforcement reallocates resources from private rents toward hospital capacity, procurement efficiency, and patient financial relief. For the literature on hospital financing and resource allocation, the results show that reducing rent

extraction can operate as a form of financial-constraint relaxation. For the trade-credit literature, the paper identifies a previously underexplored margin: trade credit as a lawful substitute for illicit private payments when external enforcement constrains corrupt relational contracting.

Although our empirical setting is China-specific, the mechanism we document has broader relevance. Many public-service sectors rely on procurement relationships in which suppliers interact repeatedly with officials who have discretion over purchasing, product selection, and payment timing. When illicit private payments become an important relational instrument, anti-corruption enforcement can do more than punish misconduct: it can reshape supplier competition and redirect resources toward the public-service institution. The magnitude of such effects will depend on institutional features, including the prevalence of procurement corruption, the strength of enforcement, and the availability of lawful contractual margins such as trade credit. Our evidence thus provides a framework for studying how anti-corruption enforcement affects resource allocation and welfare in other procurement-intensive public-service settings.

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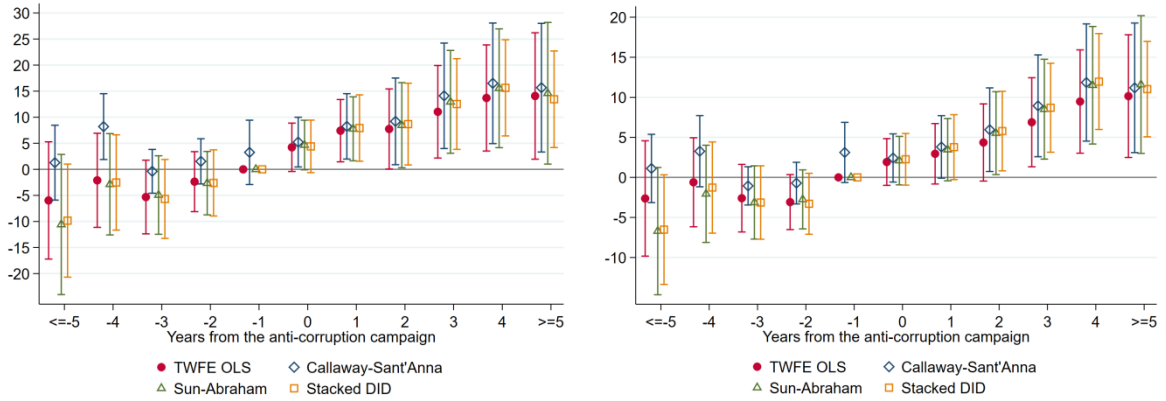
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Figure 1 The dynamic effect of anti-corruption campaign on hospitals' investment and employment

The figures show the coefficients and associated 95% confidence intervals from estimating the leads and lags regression using four different estimators: a dynamic version of the TWFE model, estimated using OLS (in red with dot markers); Callaway and Sant'Anna (2021) (in blue with diamond markers); Sun and Abraham (2021) (in green with triangle markers); Cengiz et al. (2019) (in yellow with square markers). The effects are relative to the year prior to the anti-corruption campaign on hospitals. In figures (a)-(b), the dependent variables are *NumBed* and *NumDoc* and no controls include. The sample is at the city-level, covering the period from 2013 to 2024. Standard errors are clustered by city.

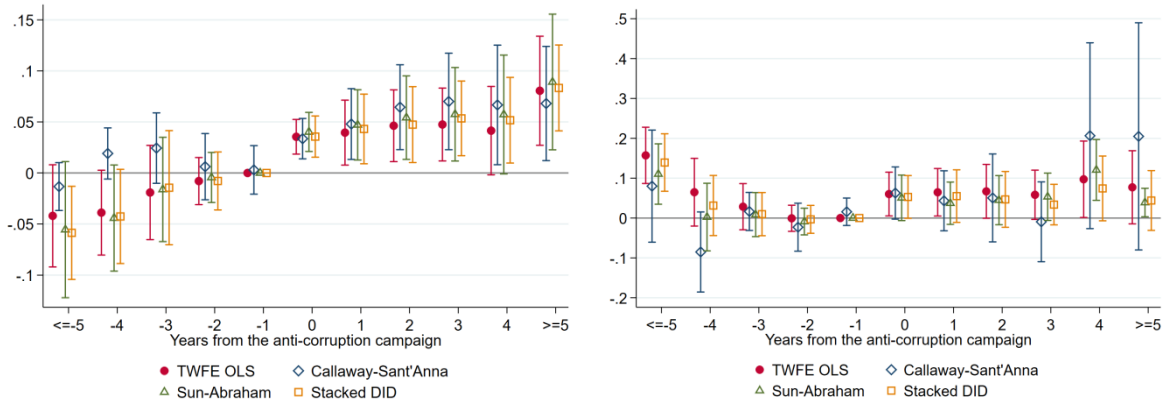


(a) Hospitals' investment

(b) Hospitals' employment

Figure 2 The dynamic effect of anti-corruption campaign on trade credit

The figures show the coefficients and associated 95% confidence intervals from estimating the leads and lags regression using four different estimators: a dynamic version of the TWFE model, estimated using OLS (in red with dot markers); Callaway and Sant'Anna (2021) (in blue with diamond markers); Sun and Abraham (2021) (in green with triangle markers); Cengiz et al. (2019) (in yellow with square markers). The effects are relative to the year prior to the anti-corruption campaign on hospitals. In figures (a)-(b), the dependent variables are *AR_LocalSup* and *AR_LocalHos* and no controls include. The sample is at the city-level, covering the period from 2013 to 2024. Standard errors are clustered by city.



(a) Trade credit provided by local pharmaceutical suppliers (b) Trade credit received by local hospitals

Table 1 Summary Statistics

This table presents summary statistics of the main variables used in the city-level, province-level, individual-level, firm-level, transaction-level, and hospital-level analysis. In Panel A, the sample is at the city level, covering the period from 2013 to 2024. In Panel B, the sample is at the province level, covering the period from 2013 to 2024. In Panel C, the sample is at the individual level, covering the period from 2014 to 2022 with a biennial frequency (i.e., 2014, 2016, 2018, 2020, and 2022). In Panel D, the sample is at the firm level, covering the period from 2013 Q1 to 2024 Q4. In Panel E, the sample is at the transaction level, covering the period from 2013 to 2024. In Panel F, the sample is at the hospital level, covering the period from 2013 to 2024. *GDPGrowth*, *Population*, *PriInd*, *PubRev*, and *Deficit* are measured at the city-level in Panel A and province-level in Panel B, although the variable names remain identical. All variables are defined in Appendix Table A1. All continuous variables are winsorized at the 1% level at both tails of their distribution.

Panel A: City-level Sample

Variables	N	Mean	Std. Dev.	Min	P25	Median	P75	Max
NumBed	3,356	177.641	75.702	15.821	118.075	179.172	231.350	361.973
NumDoc	3,359	98.606	50.819	8.000	57.476	95.775	133.375	232.247
NumHos	2,933	0.351	0.275	0.088	0.210	0.298	0.422	2.899
SizeEntry	3,363	5.320	1.613	0.000	4.480	5.536	6.372	8.709
SizeExit	3,363	1.513	2.282	0.000	0.000	0.000	3.434	7.601
AR_LocalSup	1,051	0.237	0.154	0.012	0.143	0.207	0.293	1.037
AR_LocalHos	858	0.374	0.155	0.116	0.294	0.351	0.417	1.348
AntiCorrupt	3,363	0.462	0.499	0.000	0.000	0.000	1.000	1.000
GDPGrowth	3,363	0.066	0.031	-0.038	0.046	0.070	0.085	0.140
Population	3,363	15.088	0.712	12.611	14.719	15.149	15.587	16.474
PriInd	3,363	0.121	0.078	0.003	0.062	0.109	0.165	0.394
PubRev	3,363	14.143	1.049	11.940	13.470	14.035	14.743	17.099
Deficit	3,363	3.108	2.052	1.027	1.734	2.491	3.748	13.313

Panel B: Province-level Sample

Variables	N	Mean	Std. Dev.	Min	P25	Median	P75	Max
SelfExp	294	0.280	0.065	0.056	0.253	0.286	0.317	0.414
EmerDie	352	0.089	0.054	0.030	0.040	0.080	0.130	0.260
ObserDie	351	0.182	0.365	0.010	0.040	0.070	0.170	2.540
GDPGrowth	352	1.066	0.025	0.998	1.048	1.068	1.082	1.123
Population	352	17.432	0.776	15.012	17.041	17.493	17.990	18.656
PriInd	352	9.324	5.106	0.200	6.191	9.077	12.218	23.398
PubRev	352	16.985	0.873	14.260	16.640	17.012	17.489	18.703
Deficit	352	2.609	1.673	1.102	1.654	2.234	2.781	10.533
PubMediExp	352	0.082	0.015	0.044	0.072	0.085	0.093	0.117
MarketIdx	352	8.146	1.981	2.694	6.867	8.493	9.427	11.494

Panel C: Individual-level Sample

Variables	N	Mean	Std. Dev.	Min	P25	Median	P75	Max
MediExp	62,994	0.177	0.502	0.000	0.004	0.029	0.112	3.750
SelfExpAmt	154,341	3.795	3.521	0.000	0.000	4.615	6.858	10.307
SelfExpRatio	145,760	0.433	0.426	0.000	0.000	0.400	0.900	1.000
MediSatisf	137,913	3.155	0.972	1.000	2.000	3.000	4.000	5.000
MediEval	137,848	3.141	0.972	1.000	3.000	3.000	4.000	5.000

Panel D: Firm-level Sample

Variables	N	Mean	Std. Dev.	Min	P25	Median	P75	Max
AccRe	11,382	0.570	0.602	0.023	0.216	0.379	0.691	3.819
Size	11,382	22.085	1.041	19.952	21.341	22.003	22.733	24.814
Cash	11,382	0.194	0.132	0.015	0.097	0.159	0.256	0.652
PPE	11,382	0.189	0.111	0.017	0.103	0.168	0.256	0.512
Netmargin	11,382	0.092	0.285	-1.910	0.042	0.112	0.200	0.550
Leverage	11,382	0.326	0.191	0.037	0.175	0.297	0.457	0.841
Age	11,382	3.381	1.084	0.000	2.708	3.611	4.290	4.787

InvTurn	11,382	5.198	6.726	0.227	1.825	3.448	6.031	51.834
SOE	11,382	0.227	0.419	0.000	0.000	0.000	0.000	1.000
Largest	11,382	0.323	0.131	0.096	0.223	0.303	0.404	0.678
BoardSize	11,382	2.120	0.180	1.609	1.946	2.197	2.197	2.639
IndDirector	11,382	0.373	0.048	0.333	0.333	0.333	0.429	0.500
Salary	11,382	14.904	0.738	13.236	14.396	14.845	15.403	17.037
Dual	11,382	0.356	0.479	0.000	0.000	0.000	1.000	1.000

Panel E: Transaction-level Sample

Variables	N	Mean	Std. Dev.	Min	P25	Median	P75	Max
UnitPrice	365,972	8.977	4.227	0.047	5.889	8.648	12.766	17.209

Panel F: Hospital-level Sample

Variables	N	Mean	Std. Dev.	Min	P25	Median	P75	Max
NumNewSup	159,325	0.267	0.685	0.000	0.000	0.000	0.000	5.746
NewSupHQ	27,713	0.002	0.031	0.000	0.000	0.000	0.000	1.000
NewSupLQ	27,713	0.003	0.032	0.000	0.000	0.000	0.000	1.000
NumEndSup	159,325	0.076	0.355	0.000	0.000	0.000	0.000	4.344

Table 2 Anti-corruption Campaign and Hospitals' Investment and Employment

This table presents the results of anti-corruption campaign on the number of hospital beds and doctors. The sample is at the city level, covering the period from 2013 to 2024. Number of sample changes across columns since there are singletons and missing values of control variables for certain city-year. *AntiCorrupt* is defined at the city level. *GDPGrowth*, *Population*, *PriInd*, *PubRev*, and *Deficit* are measured at the city level, while *PubMediExp* and *MarketIdx* are measured at the province level. All variables are defined in Appendix Table A1. The t-statistics reported in parentheses are based on standard errors clustered at the city level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	NumBed	*NumBed*	*NumDoc*	*NumDoc*
AntiCorrupt	8.294***	7.211**	4.441**	3.338*
	(2.671)	(2.325)	(2.299)	(1.728)
GDPGrowth		-81.691**		-74.347***
		(-1.984)		(-2.608)
Population		-43.744***		-14.947*
		(-3.025)		(-1.693)
PriInd		-35.841		-50.219
		(-0.554)		(-1.353)
PubRev		14.844*		12.142**
		(1.737)		(2.300)
Deficit		4.363		2.227*
		(1.492)		(1.725)
PubMediExp		-124.004		-50.331
		(-0.680)		(-0.407)
MarketIdx		-5.708*		-5.620***
		(-1.693)		(-2.935)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.805	0.809	0.824	0.827
N	3,356	3,356	3,359	3,359

Table 3 Robustness Tests

This table presents the results of robustness tests. In columns 1 and 2, we replace dependent variables with alternative ones with population scaling. In columns 3 and 4, we use entropy balancing method. In columns 5 and 6, we add *AntiCorrupt_Reg* into our model. In columns 7 and 8, we add *LocalCorrupt* into our model. In columns 9 and 10, we add *CVBP* into our model. In columns 11 and 12, we exclude four directly-administered municipalities. The sample is at the city level, covering the period from 2013 to 2024. *AntiCorrupt* is defined at the city level. Number of sample changes across columns since there are singletons and missing values of control variables for certain city-year. All variables are defined in Appendix Table A1. The t-statistics reported in parentheses are based on standard errors clustered at the city level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Alternative dependent variables		Entropy balancing		Controlling investigations on medical regulatory authority		Controlling local corruption culture	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	NumBed2	*NumDoc2*	*NumBed*	*NumDoc*	*NumBed*	*NumDoc*	*NumBed*	*NumDoc*
AntiCorrupt	1.251**	0.750**	14.928***	8.481***	8.805***	4.800**	8.199***	4.398**
	(2.474)	(2.243)	(2.949)	(3.316)	(2.792)	(2.405)	(2.645)	(2.276)
AntiCorrupt_Reg					-3.022	-2.126		
					(-0.663)	(-0.659)		
LocalCorrupt							0.195**	0.095**
							(2.442)	(2.371)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.909	0.908	0.801	0.825	0.805	0.824	0.806	0.824
N	2,930	2,933	3,356	3,359	3,356	3,359	3,356	3,359

	Controlling centralized drug procurement policy		Excluding four directly-administered municipalities	
	(9)	(10)	(11)	(12)
	NumBed	*NumDoc*	*NumBed*	*NumDoc*
AntiCorrupt	8.314***	4.454**	8.771***	4.710**
	(2.678)	(2.305)	(2.795)	(2.409)
CVBP	-14.076*	-8.913		
	(-1.746)	(-1.554)		
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.805	0.824	0.805	0.823
N	3,356	3,359	3,308	3,311

Table 4 Heterogeneity Tests

This table presents the results of heterogeneity tests. *COVID* is an indicator variable for COVID pandemic period (i.e., 2020-2022). Other moderating variables are measured in the baseline year 2013. *HighGDP* is a dummy variable equals one if the city's GDP per capita is above the median in 2013, and 0 otherwise. *HighBed* is a dummy variable equals one if the city's number of hospital beds per capita is above the median in 2013, and 0 otherwise. *HighDoc* is a dummy variable equals one if the city's number of doctors per capita is above the median in 2013, and 0 otherwise. *HighGovInv* is a dummy variable equals one if the city's government share of total health expenditure is above the median in 2013, and 0 otherwise. *HighPubHos* is a dummy variable equals one if the city's proportion of public hospitals is above the median in 2013, and 0 otherwise. *AntiCorrupt* is defined at the city level. Number of sample changes across columns since there are singletons and missing values of control variables for certain city-year. All variables are defined in Appendix Table A1. The t-statistics reported in parentheses are based on standard errors clustered at the city level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Moderator =	*COVID*		*HighGDP*		*HighBed*		*HighDoc*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	NumBed	*NumDoc*	*NumBed*	*NumDoc*	*NumBed*	*NumDoc*	*NumBed*	*NumDoc*
AntiCorrupt	7.535**	4.021**	17.060***	6.812***	16.218***	7.724***	15.361***	6.543***
	(2.332)	(1.988)	(4.212)	(2.778)	(4.039)	(3.170)	(4.055)	(2.770)
AntiCorrupt×*Moderator*	2.456	1.359	-20.715***	-5.612*	-18.837***	-7.816**	-17.026***	-5.072
	(0.868)	(0.808)	(-3.754)	(-1.655)	(-3.359)	(-2.341)	(-2.943)	(-1.469)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.805	0.824	0.808	0.824	0.808	0.825	0.807	0.824
N	3,356	3,359	3,356	3,359	3,356	3,359	3,356	3,359

Moderator =	*HighGovInv*		*HighPubHos*	
	(9)	(10)	(11)	(12)
	NumBed	*NumDoc*	*NumBed*	*NumDoc*
AntiCorrupt	-5.897	0.037	1.046	0.578
	(-1.431)	(0.015)	(0.251)	(0.234)
AntiCorrupt×*Moderator*	25.732***	7.997**	14.109**	7.530**
	(4.792)	(2.437)	(2.492)	(2.256)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.810	0.825	0.807	0.825
N	3,356	3,359	3,356	3,359

Table 5 Trade Credit Provided by Pharmaceutical Firms: Regional-level Evidence

This table presents the results of anti-corruption campaign on the accounts receivable. The sample is at the city level, covering the period from 2013 to 2024. Number of sample changes across columns since there are singletons and missing values of control variables for certain city-year. *AntiCorrupt* is defined at the city level. The control variables, *GDPGrowth*, *Population*, *PriInd*, *PubRev*, and *Deficit* are measured at the city level, while *PubMediExp* and *MarketIdx* are measured at the province level. All variables are defined in Appendix Table A1. The t-statistics reported in parentheses are based on standard errors clustered at the city level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	AR_LocalSup	*AR_LocalSup*	*AR_LocalHos*	*AR_LocalHos*
AntiCorrupt	0.040**	0.034**	0.053**	0.052**
	(2.617)	(2.425)	(2.023)	(1.978)
GDPGrowth		-0.526***		0.340
		(-2.850)		(0.669)
Population		0.106		-0.039
		(1.495)		(-0.910)
PriInd		-0.669		0.239
		(-1.308)		(0.323)
PubRev		-0.022		-0.045
		(-0.575)		(-0.436)
Deficit		-0.014		-0.025
		(-0.881)		(-1.296)
PubMediExp		0.018		-1.249
		(0.021)		(-0.971)
MarketIdx		-0.022*		0.041
		(-1.838)		(1.119)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.686	0.697	0.343	0.346
N	1,049	1,049	809	809

Table 6 Trade Credit Provided by Pharmaceutical Firms: Firm-level Evidence

This table presents the results of anti-corruption campaign on the accounts receivable. The sample is at the firm level, covering the period from 2013 Q1 to 2024 Q4. Number of sample changes across columns since there are singletons for certain city or firm-year-quarter. In column (1)-(2), *AntiCorrupt* is defined at the city level. In column (3)-(6), *AntiCorrupt_Sup*, *AntiCorrupt_SupDetect*, and *AntiCorrupt_SupDeter* are defined at the firm level. *GDPGrowth*, *Population*, *PriInd*, *PubRev*, and *Deficit* are measured at the city level, while *PubMediExp* and *MarketIdx* are measured at the province level. Other controls are at the firm level. All variables are defined in Appendix Table A1. The *t*-statistics reported in parentheses are based on standard errors clustered at the city level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	AccRe	*AccRe*	*AccRe*	*AccRe*	*AccRe*	*AccRe*
AntiCorrupt	0.042**	0.044**				
	(2.259)	(2.399)				
AntiCorrupt_Sup			0.110**	0.080*		
			(2.293)	(1.888)		
AntiCorrupt_SupDetect					0.101***	0.098***
					(2.779)	(2.962)
AntiCorrupt_SupDeter					0.104*	0.066
					(1.976)	(1.451)
Size		0.155***		0.183***		0.183***
		(3.302)		(3.680)		(3.675)
Cash		-0.321***		-0.325***		-0.326***
		(-6.632)		(-5.880)		(-5.857)
PPE		-0.251*		-0.153		-0.152
		(-1.874)		(-1.063)		(-1.058)
Netmargin		-0.476***		-0.474***		-0.474***
		(-5.735)		(-5.758)		(-5.760)
Leverage		-0.298***		-0.338***		-0.334***
		(-3.196)		(-3.240)		(-3.210)
Age		-0.005		0.003		0.004
		(-0.253)		(0.141)		(0.167)
InvTurn		-0.005		-0.006*		-0.006*
		(-1.578)		(-1.865)		(-1.887)
SOE		0.013		0.013		0.013
		(0.392)		(0.356)		(0.348)
Largest		0.017		0.083		0.079
		(0.088)		(0.380)		(0.364)
BoardSize		-0.173*		-0.152		-0.147
		(-1.873)		(-1.550)		(-1.505)
IndDirector		-0.167		-0.036		-0.033
		(-0.596)		(-0.112)		(-0.105)
Salary		-0.057**		-0.064**		-0.066**
		(-2.091)		(-2.023)		(-2.057)
Dual		-0.022		-0.021		-0.021
		(-0.945)		(-0.738)		(-0.740)
GDPGrowth		-0.654*		-0.856**		-0.842**
		(-1.875)		(-2.413)		(-2.364)
Population		-0.015		-0.019		-0.027
		(-0.159)		(-0.168)		(-0.239)
PriInd		0.660		0.628		0.634
		(0.674)		(0.526)		(0.532)
PubRev		-0.068		-0.085		-0.084
		(-1.264)		(-1.196)		(-1.169)
Deficit		-0.015		-0.013		-0.013

		(-0.762)		(-0.551)		(-0.552)
PubMediExp		-0.593		-1.099		-1.106
		(-0.971)		(-1.200)		(-1.212)
MarketIdx		-0.005		-0.010		-0.010
		(-0.293)		(-0.561)		(-0.516)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.688	0.715	0.690	0.718	0.690	0.718
N	11,382	11,382	10,266	10,266	10,266	10,266

Table 7 Anti-corruption Campaign and Patients' Welfare: Evidence from Individual Medical Expense and Satisfaction

This table presents the results of anti-corruption campaign on individual medical expense and medical perception. The sample is at the individual level, covering the period from 2014 to 2022 with a biennial frequency. Number of sample changes across columns since there are singletons and missing values of control variables for certain province-year. *AntiCorrupt* is defined at the province level. The control variables are all measured at the province-level. All variables are defined in Appendix Table A1. The t-statistics reported in parentheses are based on standard errors clustered at the province level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	MediExp	*MediExp*	*SelfExpAmt*	*SelfExpAmt*	*SelfExpRatio*	*SelfExpRatio*	*MediSatisf*	*MediSatisf*	*MediEval*	*MediEval*
AntiCorrupt	-0.017*	-0.019**	-0.206***	-0.203***	-0.111***	-0.114***	0.086**	0.078*	0.086*	0.085*
	(-1.772)	(-2.068)	(-2.874)	(-2.937)	(-7.882)	(-8.878)	(2.148)	(1.883)	(2.000)	(2.035)
GDPGrowth		0.029		-0.095		-0.010		0.286		0.515
		(0.565)		(-0.265)		(-0.195)		(1.241)		(1.546)
Population		0.048		0.077		-0.052		-0.125		0.140
		(0.853)		(0.122)		(-0.464)		(-0.558)		(0.481)
PriInd		-0.407		-3.473		-0.314		0.232		1.751
		(-1.103)		(-1.086)		(-0.569)		(0.123)		(0.764)
PubRev		-0.056**		-0.120		-0.044		-0.167**		-0.243*
		(-2.401)		(-0.286)		(-0.863)		(-2.609)		(-1.785)
Deficit		-0.005		0.009		-0.016		-0.051		-0.109
		(-0.374)		(0.056)		(-0.678)		(-0.907)		(-1.347)
PubMediExp		0.403		-4.272		0.504		-1.731		0.351
		(0.929)		(-1.266)		(0.624)		(-1.303)		(0.197)
MarketIdx		0.005		-0.009		-0.001		0.026		0.000
		(1.049)		(-0.141)		(-0.113)		(1.352)		(0.011)
Household FE	Yes	Yes	No	No	No	No	No	No	No	No
Individual FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.180	0.180	0.339	0.339	0.275	0.275	0.390	0.390	0.254	0.255
N	59,591	59,591	143,250	143,250	134,669	134,669	127,838	127,838	127,772	127,772

Table 8 Anti-corruption Campaign and Patients' Welfare: Evidence from Regional Medical Expense and Mortality

This table presents the results of anti-corruption campaign on the patients' medical expense and mortality. The sample is at the province level, covering the period from 2013 to 2024. The *SelfExp* variable is available through 2022 only. Number of sample changes across columns since there are singletons and missing values of control variables for certain province-year. *AntiCorrupt* is defined at the province level. The control variables are all measured at the province level. All variables are defined in Appendix Table A1. The t-statistics reported in parentheses are based on standard errors clustered at the province level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	SelfExp	*SelfExp*	*EmerDie*	*EmerDie*	*ObserDie*	*ObserDie*
AntiCorrupt	-0.013**	-0.009**	0.006	0.003	-0.044	-0.069
	(-2.563)	(-2.167)	(0.908)	(0.520)	(-0.657)	(-1.021)
GDPGrowth		-0.111		0.156		-1.191
		(-1.104)		(1.306)		(-1.141)
Population		0.132**		-0.008		-0.660*
		(2.421)		(-0.209)		(-1.881)
PriInd		-0.000		0.001		0.018***
		(-0.089)		(0.593)		(2.757)
PubRev		-0.019		-0.023		0.215*
		(-1.340)		(-1.570)		(1.870)
Deficit		-0.010		-0.005		0.000
		(-1.422)		(-0.636)		(0.007)
PubMediExp		-0.218		0.340		1.191
		(-1.008)		(0.888)		(0.866)
MarketIdx		-0.000		-0.003		0.011
		(-0.033)		(-0.424)		(0.580)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.946	0.951	0.859	0.860	0.843	0.847
N	294	294	352	352	351	351

Table 9 Procurement Contracting between Hospitals and Pharmaceutical Firms: Evidence from Unit Transaction Price

This table presents the results of anti-corruption campaign on unit price of procurement contracts. The sample is at the transaction level, covering the period from 2013 to 2024. Number of sample changes across columns since there are singletons and missing values of control variables for certain city-year. *AntiCorrupt* and *AntiCorrupt_Deter* are defined at the city level, and *AntiCorrupt_Detect* is defined at the hospital level. *GDPGrowth*, *Population*, *PriInd*, *PubRev*, and *Deficit* are measured at the city level, while *PubMediExp* and *MarketIdx* are measured at the province level. All variables are defined in Appendix Table A1. The t-statistics reported in parentheses are based on standard errors clustered at the city level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	UnitPrice	*UnitPrice*	*UnitPrice*	*UnitPrice*
AntiCorrupt	-0.227**	-0.225**		
	(-2.505)	(-2.523)		
AntiCorrupt_Detect			-0.320*	-0.306*
			(-1.663)	(-1.722)
AntiCorrupt_Deter			-0.222**	-0.221**
			(-2.422)	(-2.380)
GDPGrowth		1.446		1.447
		(0.434)		(0.434)
Population		-0.647		-0.650
		(-1.271)		(-1.272)
PriInd		-5.703		-5.691
		(-0.835)		(-0.833)
PubRev		0.845*		0.845*
		(1.793)		(1.793)
Deficit		0.338*		0.337*
		(1.657)		(1.654)
PubMediExp		2.259		2.265
		(0.055)		(0.055)
MarketIdx		-0.553*		-0.552*
		(-1.883)		(-1.881)
Hospital×Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Item FE	Yes	Yes	Yes	Yes
Method FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.803	0.803	0.803	0.803
N	208,945	208,945	208,945	208,945

Table 10 Procurement Contracting between Hospitals and Pharmaceutical Firms: Evidence from Supplier Selection

This table presents the results of anti-corruption campaign on hospital's supplier quality. The sample is at the hospital level, covering the period from 2013 to 2024. Number of sample changes across columns since there are singletons and missing values of control variables for certain city-year. *AntiCorrupt_Deter* is defined at the city level, and *AntiCorrupt_Detect* is defined at the hospital level. *GDPGrowth*, *Population*, *PriInd*, *PubRev*, and *Deficit* are measured at the city level, while *PubMediExp* and *MarketIdx* are measured at the province level. All variables are defined in Appendix Table A1. The t-statistics reported in parentheses are based on standard errors clustered at the city level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	NumNewSup	*NumNewSup*	*NewSupHQ*	*NewSupHQ*	*NewSupLQ*	*NewSupLQ*	*NumEndSup*	*NumEndSup*
AntiCorrupt_Detect	0.461***	0.492***	0.004*	0.004*	-0.001	-0.001	0.155**	0.182***
	(5.014)	(5.764)	(1.667)	(1.839)	(-0.070)	(-0.145)	(2.230)	(2.771)
AntiCorrupt_Deter	-0.022	-0.005	0.000	0.000	-0.002	-0.002*	-0.032	-0.023
	(-0.531)	(-0.110)	(0.152)	(0.103)	(-1.387)	(-1.937)	(-1.114)	(-0.705)
GDPGrowth		-0.022		0.012		-0.006		0.026
		(-0.044)		(0.774)		(-0.282)		(0.078)
Population		0.354*		0.000		0.002		0.317**
		(1.688)		(0.320)		(0.666)		(2.115)
PriInd		2.156***		0.003		0.016		1.556***
		(3.856)		(0.221)		(0.727)		(4.592)
PubRev		0.232**		-0.002		0.001		0.122**
		(2.317)		(-0.626)		(0.119)		(2.232)
Deficit		0.026*		-0.000		-0.002		0.023**
		(1.658)		(-0.429)		(-1.559)		(2.330)
PubMediExp		3.412		0.081		0.168*		4.454*
		(1.055)		(0.849)		(1.656)		(1.799)
MarketIdx		0.076***		-0.001		0.002*		0.023*
		(3.325)		(-1.049)		(1.840)		(1.697)
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.353	0.360	0.207	0.207	-0.009	-0.009	0.275	0.292
N	159,325	159,325	20,744	20,744	20,744	20,744	159,325	159,325

APPENDIX

Table A1 Variable Definition

Variable	Definition
Dependent variables	
NumBed	The number of hospital beds per hospital in the city.
NumDoc	The number of doctors per hospital in the city.
NumBed2	The number of hospital beds per capita (10,000 people) in the city.
NumDoc2	The number of doctors per capita (10,000 people) in the city.
NumHos	The number of hospitals per capita (10,000 people) in the city.
SizeEntry	The natural logarithm of one plus the average registered capital of newly-established hospitals in the city.
SizeExit	The natural logarithm of one plus the average registered capital of newly-closed hospitals in the city.
SelfExp	The proportion of self-paid medical expense by patients in the total medical expense in the province.
EmerDie	The emergency mortality rate of patients in the province.
ObserDie	The observation room mortality rate of patients in the province.
MediExp	The medical expense of the household scaled by its income.
SelfExpAmt	The natural logarithm of self-paid medical expense of the individual.
SelfExpRatio	The proportion of self-paid medical expense in the total medical expense of the individual.
MediSatisf	Individual satisfaction with local hospitals, where 1 represents very dissatisfied, 2 represents dissatisfied, 3 represents neutral, 4 represents satisfied, and 5 represents very satisfied.
MediEval	Individual evaluation for medical quality of local hospitals, where 1 represents very poor, 2 represents poor, 3 represents average, 4 represents good, and 5 represents very good.
AR_LocalSup	Total accounts receivable provided by pharmaceutical firms in the city scaled by total sales of these firms.
AR_LocalHos	Total accounts receivable received by hospitals in the city and provided by their previously-two-year (t and t-1)-contracting pharmaceutical suppliers scaled by total sales of these firms.
AccRe	Accounts receivable divided by total sales.
UnitPrice	The natural logarithm of the price per standardized unit (e.g., price per dose for pharmaceuticals) for each transaction.
NumNewSup	The natural logarithm of one plus the number of first-time pharmaceutical suppliers of the hospital.
NewSupHQ	The number of high-quality first-time pharmaceutical suppliers of the hospital divided by the number of total first-time pharmaceutical suppliers. A supplier is classified as high-quality one if it has innovative medical products approved by National Medical Products Administration.
NewSupLQ	The number of low-quality first-time pharmaceutical suppliers of the hospital divided by the number of total first-time pharmaceutical suppliers. A supplier is classified as low-quality one if it has medical products recalled by National Medical Products Administration.
NumEndSup	The natural logarithm of one plus the number of last-time former pharmaceutical suppliers of the hospital.

Variable	Definition
Independent variables	
AntiCorrupt	An indicator variable equal to one for the region-year following the first announcement of corruption investigations on the local hospital in that region by the central or local commissions for discipline inspection, and zero otherwise.
AntiCorrupt_Reg	An indicator variable equal to one for the region-year following the first announcement of corruption investigations on the local medical regulatory agency in that region by the central or local commissions for discipline inspection, and zero otherwise.
LocalCorrupt	The number of corruption cases of public officials convicted in each province divided by its population (in millions) in each year.
CVBP	An indicator variable equal to one for the region-year following the implementation of centralized drug procurement policy in that region, and zero otherwise.
AntiCorrupt_Sup	An indicator variable equal to one for the firm-quarters after any local hospital is declared to be under corruption investigations by the central or local commissions for discipline inspection and the pharmaceutical firm has previously-two-year (t and t-1)-contracting relations with the hospital, and zero otherwise.
AntiCorrupt_SupDetect	An indicator variable equal to one for the firm-quarters after the pharmaceutical firm's previously-two-year (t and t-1)-contracting client hospital experiences a corruption investigation by the central or local commissions for discipline inspection, and zero otherwise.
AntiCorrupt_SupDeter	An indicator variable equal to one for the firm-quarters after the pharmaceutical firm's previously-two-year (t and t-1)-contracting client hospital's peers in the same city experience corruption investigations by the central or local commissions for discipline inspection, and zero otherwise.
AntiCorrupt_Detect	An indicator variable equal to one for the hospital's transactions after it experiences a corruption investigation, and zero otherwise. (For transaction-level sample) An indicator variable equal to one for the hospital-year after it experiences a corruption investigation, and zero otherwise. (For hospital-level sample)
AntiCorrupt_Deter	An indicator variable equal to one for the hospital's transactions after its peers in the same city experience corruption investigations, and zero otherwise. (For transaction-level sample) An indicator variable equal to one for the hospital-year after its peers in the same city experience corruption investigations, and zero otherwise. (For hospital-level sample)
Regional-level control variables	
GDPGrowth	The GDP growth rate in the city (or province).
Population	The natural logarithm of the population in the city (or province).
PriInd	The proportion of the value-added output in primary industry (i.e., agriculture, forestry, animal husbandry, and fishery) in total GDP in the city (or province).
PubRev	The natural logarithm of the local government's public fiscal revenue (in billion RMB) in the city (or province).
Deficit	The public fiscal expenditure divided by the public fiscal revenue of the local government in the city (or province).
PubMediExp	The proportion of the public health expenditure in the total public fiscal expenditure of the local government in the province.
MarketIdx	The marketization index of each province.
Firm-level control variables	
Size	The logarithm of total assets.
Cash	The cash and cash equivalent divided by total assets.
PPE	The tangible fixed assets divided by total assets.

Variable	Definition
Netmargin	The net income before extraordinary items scaled by operating revenue.
Leverage	The total liabilities divided by total assets.
Age	The logarithm of one plus the difference in quarters between current quarter and the quarter the firm first listed.
InvTurn	The total sales divided by inventories.
SOE	An indicator variable that equals one if a firm's ultimate owner is the government and zero otherwise.
Largest	The ratio of the number of shares held by the largest shareholder to the total number of shares outstanding.
BoardSize	The logarithm of the number of directors on the board.
IndDirector	The ratio of the number of independent directors to the total number of directors.
Salary	The logarithm of the total compensation of the top three executives.
Dual	A dummy variable that equals one if a firm's chairman of board also serves as CEO in the current year, and zero otherwise.

Table A2 Examples of Hospital Officials Prosecuted for Taking Kickbacks from Pharmaceutical Suppliers

Announcement Date	Name	Position	Accusation and Sentence
2015/08	JIA Mou	President of Guangzhou Huangpu District Traditional Chinese Medicine Hospital	Between 2013 and 2014, taking advantage of his position, he provided assistance to suppliers during the hospital's procurement of medical equipment such as color ultrasound and anesthesia machines, accepting bribes totaling 1.063 million RMB. He was sentenced to six years and six months in prison.
2021/08	WU Wenxia	Director of Special Examination Department, Panzhihua Maternal and Child Health Hospital	In 2016, taking advantage of his role in setting procurement parameters, he tailored bidding conditions for medical equipment salespersons, helping a certain model of color ultrasound win the bid at 2.145 million RMB, and accepted 380,000 RMB in cash. Additionally, he accepted other bribes totaling 850,000 RMB. He was sentenced to four years and six months in prison.
2021/09	LIU Biao	Director of the Third Department of Cardiology, Lixin County People's Hospital	From October 2018 to January 2021, he accepted medical supplies kickbacks from SU Mou and GE Mou totaling over 2.31 million RMB (with an additional over 580,000 RMB being attempted bribery). The kickbacks were calculated as a percentage of the supply amount and paid after deducting expert consultation fees, etc.
2023/06	WANG Moumou	Director of Orthopedics and Traumatology Department, Dianjiang County Traditional Chinese Medicine Hospital	From July 2013 to April 2018, taking advantage of his authority over the procurement and use of high-value consumables, he extensively purchased supplies from medical equipment salesperson LUAN Mou and accepted kickbacks totaling over 1.12 million RMB. He was sentenced to four years in prison.
2023/07	LI Yulong	President and Party Secretary of Panzhihua Fourth People's Hospital	Between 2013 and 2021, taking advantage of his position, he specified technical parameters to ensure that five medical equipment companies successfully won bids in procurements. He accepted a total of 1.8 million RMB in cash kickbacks. He was sentenced to four years and six months in prison.

Table A3 The Distribution of Hospital Anti-corruption Cases by Province and by Year

This table presents distributions of the anti-corruption cases by province (Panel A) and by year (Panel B). The sample covers the period from 2013 to 2024.

Panel A: The distribution of the anti-corruption cases by province

Province	# of anti-corruption cases	% of all anti-corruption cases
Shanghai	2	0.13%
Yunnan	123	7.79%
Inner Mongolia	4	0.25%
Beijing	12	0.76%
Jilin	15	0.95%
Sichuan	165	10.46%
Tianjin	20	1.27%
Ningxia	14	0.89%
Anhui	151	9.57%
Shandong	42	2.66%
Shanxi	30	1.90%
Guangdong	83	5.26%
Guangxi	87	5.51%
Xinjiang	11	0.70%
Jiangsu	86	5.45%
Jiangxi	74	4.69%
Hebei	16	1.01%
Henan	52	3.30%
Zhejiang	45	2.85%
Hainan	86	5.45%
Hubei	89	5.64%
Hunan	81	5.13%
Gansu	20	1.27%
Fujian	15	0.95%
Tibet	22	1.39%
Guizhou	98	6.21%
Liaoning	25	1.58%
Chongqing	39	2.47%
Shaanxi	27	1.71%
Qinghai	4	0.25%
Heilongjiang	40	2.53%

Panel B: The distribution of the anti-corruption cases by year

Year	# of the anti-corruption cases	% of all anti-corruption cases
2013	4	0.25%
2014	42	2.66%
2015	96	6.08%
2016	63	3.99%
2017	83	5.26%
2018	132	8.37%
2019	144	9.13%
2020	102	6.46%
2021	185	11.72%
2022	189	11.98%
2023	310	19.65%
2024	228	14.45%

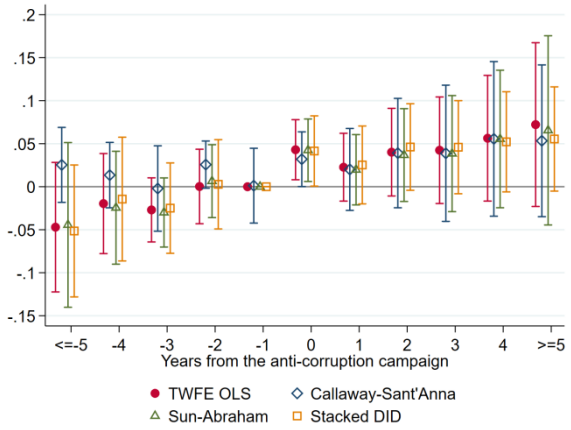
Table A4 Anti-corruption Campaign and Hospitals' Entry and Exit

This table presents the results of anti-corruption campaign on the number of hospitals and size of entered and exited hospitals. The sample is at the city level, covering the period from 2013 to 2024. Number of sample changes across columns since there are singletons and missing values of control variables for certain city-year. *AntiCorrupt* is defined at the city level. *GDPGrowth*, *Population*, *PriInd*, *PubRev*, and *Deficit* are measured at the city level, while *PubMediExp* and *MarketIdx* are measured at the province level. All variables are defined in Appendix Table A1. The t-statistics reported in parentheses are based on standard errors clustered at the city level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

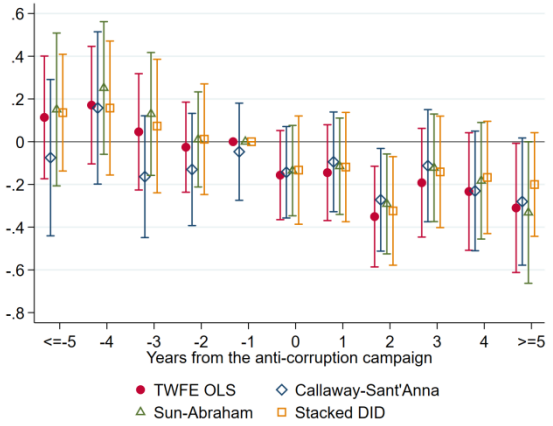
	(1)	(2)	(3)	(4)	(5)	(6)
	NumHos	*NumHos*	*SizeEntry*	*SizeEntry*	*SizeExit*	*SizeExit*
AntiCorrupt	0.040*	0.037*	-0.208**	-0.191**	0.181	0.206
	(1.924)	(1.766)	(-2.476)	(-2.258)	(1.326)	(1.549)
GDPGrowth		0.236		2.033		-5.273***
		(1.184)		(1.402)		(-3.005)
Population		-0.105		-0.092		0.562
		(-1.401)		(-0.334)		(1.023)
PriInd		-0.227		0.725		1.445
		(-0.521)		(0.566)		(0.666)
PubRev		-0.035		-0.135		0.268
		(-0.541)		(-0.610)		(0.886)
Deficit		-0.017		0.039		-0.052
		(-0.634)		(0.448)		(-0.652)
PubMediExp		1.442		3.970		33.899***
		(1.594)		(0.765)		(4.439)
MarketIdx		-0.052**		-0.069		0.205*
		(-2.323)		(-0.784)		(1.681)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.469	0.475	0.447	0.448	0.263	0.273
N	2,933	2,933	3,363	3,363	3,363	3,363

Figure A1 The dynamic effect of anti-corruption campaign

The figures show the coefficients and associated 95% confidence intervals from estimating the leads and lags regression using four different estimators: a dynamic version of the TWFE model, estimated using OLS (in red with dot markers); Callaway and Sant'Anna (2021) (in blue with diamond markers); Sun and Abraham (2021) (in green with triangle markers); Cengiz et al. (2019) (in yellow with square markers). The effects are relative to the year prior to the anti-corruption campaign on hospitals. In figures (a)-(b), the dependent variables are *NumHos* and *SizeEntry* and no controls include. The sample is at the city-level, covering the period from 2013 to 2024. Standard errors are clustered by city.



(a) Number of hospitals



(b) Size of newly-established hospitals