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AUDIT CENTRALIZATION AND AUDIT QUALITY:
EVIDENCE FROM CHINESE CITIES

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ABSTRACT

Audit design invokes a tradeoff between a monitor's local knowledge and their independence from influence. We study this tradeoff in the context of a pilot program in six Chinese provinces in 2016, in which provincial governments were given control over budgeting and personnel decisions for city audits. Using a difference-in-differences framework we show that, compared to non-pilot provinces, centralization increases detection of suspicious expenditures by over 50%. These improvements occur also in cities with auditors appointed pre-2016, suggesting that stronger incentives and greater resources rather than auditor selection explain the improvements. Consistent with this interpretation, we find that financial (but not human) resources devoted to city audits increase with centralization. Further results show that centralization's benefits are strongest in provinces that centralized audit office financing as part of the reform – particularly in poorer cities – suggesting a role for resources in improved performance.

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1 Introduction

Audits are a central feature of organizational monitoring in both the private and public sectors (Baber, 1983; DeFond and Zhang, 2014; Olken, 2007). There are myriad factors that could impact audit quality: auditor resources; independence; skill. While there is much work in accounting and economics on the effectiveness of such audits in improving public sector performance and governance (e.g., Avis et al., 2018; Bobonis et al., 2016; Saito and McIntosh, 2010; Shi, 2024), there is at most sparse research on the specific factors that lead to greater efficacy of public sector auditing (Hay and Cordery, 2018). Internationally recognized government auditing frameworks have long maintained that audit institutions can be objective and effective only if they are independent of the audited entity and insulated from external influence (INTOSAI, 2019a,b). This emphasis on independence underpins much of the research on government audits (e.g., Avis et al., 2018; Olken, 2007; Saito and McIntosh, 2010), despite the limited availability of direct and credible empirical evidence on its importance.

In this paper, we study how the control over auditor assignment and resources impact public audit effectiveness in uncovering suspicious expenditures, in the context of Chinese city governments.¹ Specifically, we examine how a pilot policy that shifted control over city audits to province-level governments, implemented in six province-level administrative divisions, impacted auditing outcomes. Further, taking advantage of variation in the reform’s timing and the appointment dates of government auditors, we can distinguish between selection versus incentives and resources of auditors affected by the pilot in explaining any improvements. Importantly from our perspective, the central government deliberately implemented the pilot in provinces across a range of geographies and levels of development, because it was done to evaluate the consequences of such reforms before a potential roll-out to the entire country, which is typical for policy implementation since 1980 in China (Wang and Yang, 2025).

The reform we study effectively centralized control over Chinese cities’ audit offices.² Historically, although these offices were formally under the dual oversight of city governments and provincial audit authorities, it is the former that had control over audit office funding and promotion of audit officials. In pilot provinces, the reform shifted control over personnel management, resource allocation, and project planning to provincial audit offices, significantly reducing the influence of city governments over how monitoring took place. The consequences are *ex ante* ambiguous, reflecting a fundamental tradeoff invoked by centralization more generally. Most obviously, centralizing personnel and project management takes away the conflict-of-interest inherent in auditing a government that is ultimately responsible for these auditors’ appointments (Dao et al., 2012; Vannutelli, 2022). However, this effect may be counterbalanced by the fact that well-meaning city officials may be particularly well-informed about the locally-managed city expenditures most in need of scrutiny, whereas provincial officials may set audit priorities that are

¹As distinct from, say, the U.S. model, audit of public expenditures by city governments in China is completely under the control of the Audit Bureau (or city audit office – we use the latter term throughout for simplicity and consistency), a city government department, so that auditors are themselves public employees. In the U.S., it is possible for accounting firms to audit government expenditures; in China, it is common for state-owned enterprises to hire accounting firms to audit their financial reports, but all public projects require government audits. Throughout when we refer to auditors, we refer to those employed by the government.

²To be precise, the administrative unit we study is the city, of which there are 333 in China. Note that cities are themselves often referred to as prefectures or municipal divisions. For readability, we simply refer to “cities” as the unit of analysis throughout.

less appropriate for local conditions (Hayek, 1945).³ There is another related tradeoff in the selection margin – local officials may be best-positioned to hire and motivate effective auditors (Holmström, 1979), but their choices may also be tainted by bias or corruption (Bardhan and Mookherjee, 2000; Jia and Nie, 2017).⁴

Using a difference-in-differences approach for the years 2013-2020, we show that city audits in pilot provinces uncover over 50 percent more suspicious expenditures post-reform. The endogenous responses of officials in reforming provinces would reduce *actual* malfeasance if they expect more or tougher audits post-reform, so this figure may represent a lower bound on audit improvements.

There are two primary concerns with the interpretation of our main result above. The first is related to possible endogenous selection of pilot provinces. If provinces with lower audit quality were more likely to be selected, our estimated effects on this subset of provinces would overstate the improvements that would occur if centralization were implemented across the country. However, as we detailed below, these pilot provinces are officially claimed to be selected for representativeness. More importantly, this argument is premised on the assumption that provinces with more serious audit problems would see larger improvements from centralization. We thus drop these weakly-governed provinces (Yunnan and Guizhou) from the treatment group, and obtain similar results. As a second approach to testing for endogenous assignment to weakly-governed provinces, we use additional data on audit efficacy as appraised by the National Audit Office (henceforth NAO) in the years preceding the reform, and we do not find any significant difference between pilot and non-pilot provinces.

The second concern is that the reform effects we document could be driven by attention from the central government. In particular, government officials in pilot provinces may exert more effort if they know they face increased scrutiny from the NAO, even if centralization itself has no effect. To assess this possibility, we collect detailed data on visits by NAO leaders to individual provinces as a measure of attention. We observe no change in the likelihood of a visit from NAO leadership in pilot provinces post-reform, nor do we find that NAO visits increase the estimated impact of centralization reform. Overall, we do not find any evidence consistent with the view that increased attention from the central government accounts for our main results.

The reform gave control over local chief auditor appointments to provincial authorities. To examine whether a change in auditor selection accounts in part for the improvements we observe, we may thus take advantage of variation in the timing of incumbent auditors’ appointment dates to examine whether

³Though in conversations with local auditors themselves, the informational advantage of city auditors was described as unclear, since provincial auditors may have a broader view, possibly due to higher ability, of the types of government projects that require closer scrutiny, and hence may be better positioned to select the projects most in need of audit.

⁴This is naturally a simplification of the full set of tradeoffs involved, as we discuss in more detail in Section 3. Others that auditors highlighted to us in interviews with them included: (1) The need for all relevant decisions to be approved by provincial authorities under centralization (including, for example, the hiring of an entry-level auditor), for which the provincial audit offices had insufficient capacity, so that some decisions were devolved to local offices. (2) Post-reform, many city auditors were assigned projects from the provincial audit office, which offloaded some of their assignments onto them. This shift increased local auditors’ workloads but also gave them exposure to more experienced and skilled provincial auditors. (3) Some local auditors complained about lack of flexibility in project selection post-reform; this is essentially a variant on the informational advantage argument in the main text, but one driven by need for approval for project audits from provincial authorities. (4) There may have been a reduction in redundant auditing that may have arisen due to lack of coordination pre-reform. This is again a variant on information and project selection. (5) According to city and county auditors we spoke to, post-centralization, the provincial audit office had the authority to move higher-performing auditors from city or county offices to the provincial office, leading to a possible local “brain drain.”

the reform’s effects differ based on whether the audits were overseen by bureaucrats appointed pre- versus post-2016. We observe very similar effects for both groups, consistent with a minimal role for auditor selection; also in line with this finding and interpretation, we do not observe any change in the observable characteristics of city chief auditors appointed in reforming provinces.

In four of the reforming provinces (Guangdong, Jiangsu, Zhejiang and Shandong), the pilot centralized personnel management but not the financing of city audit departments so that all financial support (including salaries) of city audit offices still came from city government budgets. Whereas centralized budgeting also led to pay equalization across city auditors (since all were considered civil servants employed by the provincial audit office), pay inequality could persist in these partial-reform provinces, with higher pay for auditors in relatively rich cities relative to poorer ones. Consistent with a role for financial resources in observed improvements, the increase in suspicious funds uncovered is approximately half as high in cities located in these “partial-reform” provinces (though this difference is not statistically significant). Interestingly, the impact of full reform (i.e., including control over financial resources) is much more pronounced in poorer cities which would plausibly have been most resource-constrained prior to the pilot; in richer cities, we observe very little difference between partial and full reform.

Overall, we see several important contributions from our work to the literature on auditing and oversight in economics and accounting. Most importantly, we speak directly to the role of auditor independence in determining audit efficacy. Research on the auditing of firms emphasizes such themes as the discretion in choice of auditor by the audited party and the inherent conflict-of-interest in audit fees being paid by the audited firm (see, e.g., Duflo et al., 2018, for a study of Indian environmental audits; and DeFond and Zhang, 2014 and Lennox and Wu, 2018 for an overview of accounting research on auditor identity and audit quality),⁵ the impact of auditor-firm social ties on audit quality (Guan et al., 2016; He et al., 2017), and the resultant problem of opinion-shopping (Lennox, 2000).⁶ Closely related to our study, Lennox et al. (2014) show that mandatory rotation of audit partners can improve audit quality. While the organizational reform we study is broader than the targeted interventions of this earlier work (incorporating, in particular, shifts in budgeting and promotion policies in addition to increased independence of local auditors), our setting also affords a number of advantages relative to prior work. Because we have variation in whether there is turnover in chief auditors depending on their tenure in 2016, we can distinguish the role of auditor selection (i.e., higher-ability auditors being selected as a result of centralization) from changes in economic and political incentives (where auditors remain the same). Furthermore, our study highlights the role of organizational structure change in improving audit quality.⁷

There is also a small but growing literature focused on government regulation of auditors (in particular

⁵There is evidence that audit partners whose compensation depends more on client fees provide lower-quality audits (Dekeyser et al., 2021). There is also empirical research which finds that transferring the power to hire and fire the auditor from managers to investors improve audit independence and audit quality (Dao et al., 2012; Mayhew and Pike, 2004). The accounting literature also stress the importance of independence for internal audits in the private sector (e.g., Abbott et al., 2016).

⁶More narrowly, we contribute to the burgeoning literature on accounting quality in China specifically. See Lennox and Wu (2022) for a summary of China-related accounting research.

⁷Our paper is also related to Nakhmurina (2024) who shows that state-level monitoring of city governance improves government quality, though our focus is on the effect of changes in the organizational form of public auditing agencies on audit quality.

the role played by the Public Company Accounting Oversight Board in the U.S.) and the consequences for audit quality (DeFond, 2010; Hendricks et al., 2022; Krishnan et al., 2024; Lennox and Pittman, 2010). While we are also interested in the political economy of auditing, our purpose is quite different from this literature, in that we focus on the political incentives of public sector auditors who themselves are public employees. Our study thus also links to the accounting literature on governmental auditors, which typically focuses on the supply of auditing (Baber, 1983), audit fee arrangements (Baber et al., 1987), and audit performance (Berry et al., 1987; Saito and McIntosh, 2010).

Given our focus on the organizational structure of public audit functions, our work is also related to the literature which examines the impact of audit firm organization on audit quality (Firth et al., 2012; Beck et al., 2019; Carson, 2009; Ege et al., 2020). Our finding that centralization can improve audit quality may be particularly relevant to this body of research.

The question of independence is less prominent in research on public sector audits, which focuses instead on the impact of audit outcomes (while most research in accounting on private sector audits focuses instead on the causes rather than the consequences of audit outcomes), largely taking auditor independence as given (see, e.g., Avis et al., 2018, for one of many studies of the impact of Brazilian city audits). One exception is Vannutelli (2022), which looks at the introduction of random audits to the oversight of Italian cities. This shift away from auditor appointment by mayors led to improved tax collection and reduced deficits, in line with national objectives. Our setting allows us to look more directly at fund misuse rather than changing governmental objectives, and to understand the role of incentives as distinct from auditor selection, whereas the latter is the primary focus of Vannutelli (2022).

Finally, the distinct features of the reform we analyze allow us to study in the same setting the potentially distinct roles of decentralizing personnel versus budgetary management. While this distinction has been well-noted in qualitative studies of political economy (Bardhan, 2022; Cai and Treisman, 2006), to our knowledge ours is the first to provide an empirical analysis of their separate effects. This is in large part because researchers in economics and accounting have not had measures of resources available to auditors; because we have such data on Chinese city audits, we are able to enrich our understanding of how financial resources affect audit outcomes.

2 Institutional Background

For the first thirty years following the establishment of the People’s Republic of China in 1949, there were no independent audit agencies. Instead, audit functions operated under the finance department within the context of a highly centralized planned economy. The Chinese Constitution, adopted in 1982, stipulated the creation of a central audit agency, established by the State Council, to “supervise through auditing the revenue and expenditure of all departments under the State Council and of the local governments at various levels, and the revenue and expenditure of all financial and monetary organizations, enterprises and institutions of the state” (Article 91 of the Constitution). The Constitution further required the creation of local audit units by governments at the county-level and higher.⁸ In subsequent decades, the

⁸In the Chinese administrative hierarchy, counties are a level below cities. County audit offices were also affected by the reform we describe here, with changes that essentially paralleled those that occurred in city-level offices. Since we do

National Audit Office (NAO) and local audit offices (including provincial, city, and county audit offices) were established as administrative departments within governments at all levels.

According to Article 109 of the Constitution and further codified in the 1994 Audit Law, audit offices at each level of government are responsible for and report their work to both local governments and higher-level audit offices. Taking the city audit offices that are our focus as an example, city governments had the authority to appoint city chief and vice-chief auditors, allocate the city audit office budget, recruit audit office staff, and so forth. Similarly, city audit offices audited city government expenditures and city SOEs. Provincial audit offices provided guidance to lower-level audit offices, formulated policies and plans to be implemented by city auditors, and also inspected the work of city offices to ensure their effectiveness (in addition to auditing provincial government expenditures and province-owned SOEs). Under this dual administrative system, city audit offices carried out audits on the overall level of revenue and expenditure, and also monitored each department within the city government, as well as other related public organizations and projects. Apart from audits by offices at the same administrative level, there was also “up-to-down” auditing by provincial audit offices (e.g., audits of projects spanning multiple cities), as discussed in Chu et al. (2021), though that is distinct from our focus here.

This initial administrative structure was a compromise that aimed to decentralize audit functions to local authorities with greater local expertise, while maintaining some higher-level oversight from the province. In practice, the system raised concerns over conflicts of interest that arise when auditors monitor governments that control them, with insufficient influence from province-level auditors to ensure independence (Xiao et al., 2016; Xiao, 2020).

These concerns led to calls for stronger control over subordinate audit offices by higher-level ones (Xiao, 2020). The resultant centralization plan was announced on December 8, 2015 by a directive from the Central Committee (the Communist Party of China’s highest governing body).⁹ The reform involved a pilot plan in six provinces: Jiangsu, Zhejiang, Shandong, Guangdong, Guizhou, and Yunnan.¹⁰ Their governments were required to centralize city audit functions before the end of July 2016. The pilot provinces were claimed to be selected to be representative of the China economy so it is a mix of higher (Jiangsu, Guangdong, Shandong, and Zhejiang) and lower (Guizhou and Yunnan) governance provinces.

There were three main components to the audit reform. The first pair relates to control over human resource practices. At a senior level, the pilot program centralized appointment of the city chief auditor (as well as county chief auditors), whose appointment and removal decisions were transferred to the Provincial Organization Department (and the provincial audit office, which was to be consulted by the Provincial Organization Department for these appointment or removal decisions). Thus, while city audit

not have data on county-level audit activity, our focus is on city audit offices.

⁹More precisely, the General Office of the Central Committee of the Communist Party of China and the General Office of the State Council issued the *Framework Opinion on Several Major Issues Concerning Improving the Audit System* on December 8, 2015, which provides a general guidance on reforming the audit system and also lists 7 provinces (and central administered municipalities) as pilot provinces (see https://www.gov.cn/zhengce/2015-12/08/content_5021393.htm for full text, accessed on December 2, 2025). The *Pilot Plan for the Reform of Personnel, Finance, and Property Management in Local Audit Offices* was then released at the same time, which specifies further details of the reform (see <https://sjc.tongji.edu.cn/info/1068/1212.htm> for full text, accessed on December 2, 2025).

¹⁰Chongqing was also included in the pilot. However, because it is a municipality administered directly by the central government (i.e., without multiple cities under its jurisdiction), it is dropped from our analysis, which is conducted at the city-year level.

offices remained local government departments, after the reform, appointment and approval of their leaders was handled at the province-level.

Second, the reform centralized audit planning. This gave the provincial audit office agenda-setting power over which projects to audit; furthermore, any audit projects proposed by city offices required review and approval by the provincial audit office before being executed. Ex post, provincial audit offices also were tasked with evaluating subordinate audit offices to ensure accountability. As a further element to centralized oversight and planning of audit activities, provincial audit offices were required to establish a uniform audit information system.

Finally, the reform centralized budgeting of city audit offices, so that audit expenses (including salaries) of city audit offices would be provided by the provincial audit office (which in turn is backed by the provincial finance department), whereas pre-reform these resources were provided by city governments. However, the December 8, 2015 directive also explicitly specified that provinces could centralize budgeting only if local conditions allowed it; otherwise, budgeting and financial resources were to continue to be provided by local governments.¹¹ The language of the directive left considerable discretion in the hands of provincial authorities. According to Xiao (2020), only two (Yunnan and Guizhou) of the six provinces in the pilot also centralized budgeting. Note that budget centralization was encoded in the initial audit reform documents in Guangdong and Shandong, but Xiao (2020) reports that it was never implemented in practice. We confirmed this to be the case in interviews with provincial and city auditors in these provinces. We thus call the other four “partial-reform” provinces, where city-level auditors still relied on city-level governments for budgeting, while their promotion was managed by the provincial audit office after the reform.

One further feature of the reform warrants particular attention. Based on our conversations with senior auditors in both full-reform and partial-reform provinces, the main reason behind the decision to partially reform was, in their view, compensation inequality across different cities within a given province, which would be reduced under budget centralization. Provinces that implemented full reform have relatively low average incomes but also relatively low income dispersion across cities. In full-reform provinces, the average income of urban residents was 53,321 RMB (about 7,600 USD) in 2015 with an average within-province coefficient of variation (of city-level income) of 0.088. In contrast, partial-reform provinces are relatively rich, but also have high between-city income differences, with average incomes of urban residents of 69,750 RMB (about 9,900 USD) in 2015 with an average within-province coefficient of variation (of city-level income) of 0.141. Budget centralization (including salaries) in these high-income provinces would necessitate a cut in total compensation for auditors in richer cities, which meant that the government anticipated significant resistance in these relatively influential cities. Naturally, this leads to heterogeneity across full- versus partial-reform provinces, though as we show below, including controls for initial income times year fixed effects does not impact our results, suggesting that this heterogeneity is unlikely to account for our main findings.¹²

¹¹Specifically, Article 3 of the directive states that “The budget and assets of local audit institutions shall be centrally managed by the relevant provincial departments. They may also be entrusted to relevant city and county departments...based on actual conditions.”

¹²Of secondary relevance, a further reason for take-up of partial versus full reform involves differences in fiscal systems. For cities in full-reform provinces, local fiscal revenue accounts for a small proportion of the total budgets of local governments,

We summarize the administrative structure and relationships among governments and audit offices in each level both before the reform (Figure 1a) and after the reform (Figure 1b). Overall, provincial audit offices (on behalf of provincial governments) were given far greater authority over city audit offices after the reform. In full-reform provinces, city governments were left with relatively little power to set city audit policies, while in partial-reform provinces city governments still held control over budgeting decisions (and the accompanying power that comes with fiscal control).

We conclude this section with a discussion of reform implementation in China in general, and with respect to the specific pilot program we study here.

At least in principle, any pilot reform in China is meant to be done in a manner that is reflective of its likely impact when rolled out to the entire country. This view is encapsulated in a directive from the China’s leader at the time of this reform, Xi Jinping, who instructed the group entrusted with leading reform efforts that the entire purpose of pilot programs is to generate an “overall view of a reform” in preparation for full-scale rollout across the whole country. The implication is that pilot locations need to be representative, otherwise the lessons one might draw from them may not be applicable to locations that have not yet adopted a given set of reforms.¹³ This has implications for the extent to which any findings from the pilot program we study here can be generalized to the country overall.

Turning to the specifics of the audit reform we study here, this idea of broad applicability was invoked in journalistic accounts of the pilot policy: “General Secretary Xi Jinping pointed out that “pilot programs are an important task of reform and an even more important method of reform.” The purpose of pilot programs is to explore the paths and forms of reform implementation, providing replicable and scalable experience and methods for improving the quality and expanding the scope of reform. The core purpose of pilot programs is to identify problems, explore solutions, and provide a realistic basis for macroeconomic decision-making.”¹⁴ The pilot provinces, as noted above, encompass a broad range of attributes, consistent with the official position on reform.

3 Conceptual Framework

Audits are a common feature of government monitoring systems for ensuring accountability and responsible management of public funds. The 1977 Lima Declaration (INTOSAI, 2019a) emphasizes the importance of establishing audit institutions with independence guaranteed by law. The 2007 Mexico

since these are relatively poor provinces that rely to a greater extent on transfers from higher-level governments (and also municipal bond issues that are managed and overseen by provincial finance departments since city governments cannot directly issue municipal bonds by law). It is thus much easier for the provincial governments in provinces that rely heavily on transfers to manage the budgeting of audits, as the provincial finance department can simply reduce its fiscal transfers to city-level finance departments. In contrast, in rich provinces like Guangdong, Zhejiang, Jiangsu and Shandong, the funds flow in the opposite direction: a county would transfer tax revenues to a city, which would then transfer these funds to the provincial finance department. Centralization of audit office budgeting would create a budget shortfall in these well-off provinces, since the provincial government would not have sufficient funds to cover the incremental expense of financing county and city auditing, without requesting further fiscal transfers from city finance departments, which was politically difficult to implement. The data provide at least suggestive support for this view: in our sample period, local fiscal income accounts for 29% of total revenues in full-reform provinces, while this figure is 59% in partial-reform provinces.

¹³See, e.g., https://www.chinadaily.com.cn/interface/toutiao/1138561/2015-6-10/cd_20922439.html, last accessed December 2, 2025.

¹⁴See https://www.thepaper.cn/newsDetail_forward_13481736, last accessed December 2, 2025.

Declaration on SAI Independence (INTOSAI, 2019b) further highlights that audit independence is rooted in autonomy over personnel, finance, and administration. Organizational design is a central element to ensuring such independence, and centralization of audit functions that provides greater autonomy of audit agencies over the governmental bodies they monitor aligns with the theoretical arguments laid out in these international frameworks and principles.

Ex ante, the effect of audit centralization on audit quality is ambiguous. There are several reasons that it could lead to improvements. First, centralizing personnel and financial resources reduces city auditors' reliance on city governments, thus increasing their independence from the authorities whose expenditures they are tasked with monitoring. Pre-reform, city governments controlled both the financial resources and political promotions for auditors from a city's audit office, thus creating an obvious incentive for auditors to be lax in their oversight of the local government and its departments (including state-owned firms controlled by the city government). After personnel and project management were centralized to provincial governments, local auditors are less likely to be subject to capture by local officials (Bardhan and Mookherjee, 2000, 2006; Jia and Nie, 2017), and this reduced conflict-of-interest may improve audit quality (Dao et al., 2012; Kong and Liu, 2024; Vannutelli, 2022). Second, centralization leads to better coordination among different city audit offices and also with the provincial audit office, which in turn could reduce redundant audits and also improve audit quality from information and knowledge sharing (Lipscomb and Mobarak, 2016; Liu et al., 2022). A third reason makes a distinct prediction about the particular cities where we should see the greatest improvements from audit centralization. In particular, centralization of financial resources may hold particular benefits for audit offices in poor cities which are either understaffed or underfunded due to financial constraints. These constraints, which would not be as severe in richer cities with better-funded government agencies, would limit audit efficacy. Prior studies have shown that an audit team's human capital is closely related to audit quality (Lennox and Wu, 2018; Nagy et al., 2023). The shift to provincial control over city audit office staffing potentially had particular benefits for poorer cities, as the recruitment of audit staff at the province level and the allocation of staff across cities was done by the provincial audit office. This helps ensure previously understaffed city audit offices (typically those with financial constraints in poor cities) maintain a basic audit team, and it also helps city audit offices with insufficient funding to secure financial resources. (By a symmetric argument, budget centralization could lead to a decline in performance in richer cities.) Such a resource channel is especially salient in our setting, because partial-reform provinces did not centralize city audit offices' financial resources while full-reform ones did. This variation allows us to further examine whether provinces that centralized both personnel and financial resources experienced larger improvements in audit quality (and whether these improvements were larger in poor cities).

There are also reasons that centralization could decrease audit quality. First, standard theory suggests that decentralization in general enables local officials to better understand and respond to local needs and shocks, thereby improving the timeliness and relevance of decision-making while reducing efficiency losses caused by information asymmetry (Hayek, 1945; Tiebout, 1956). After centralization, decisions were centralized at the provincial audit office, thus making city audit offices more distant administratively from local decision-making and information channels. This may have made it harder to identify the most problematic projects or to detect subtle irregularities which requires detailed knowledge of local

conditions. The second cost of centralization is the loss of flexibility, which may worsen the adaptability of the organization’s decisions to local conditions (Alonso et al., 2008). When city audits are centralized to the provincial level, they are more likely to follow uniform provincial templates rather than being fully adapted to local risk profiles and governance problems. Third, centralization may reduce motivation. Because provincial leaders are physically further away from city auditors, individual efforts on local audit projects are less observable to those evaluating performance, thus lowering the return to exerting effort (Holmström, 1979). Fourth, centralization may have led to a “brain drain” from local audit offices, which was a very practical concern with the particular reform we study. After the centralization, the provincial audit office could have chosen to emphasize the audits of provincial rather than city projects and, given their control over personnel allocations, may have moved the highest-quality auditors to provincial auditing. Finally, in practice, provincial offices may have insufficient managerial capacity to manage such a large audit team encompassing all cities within the same province (even if they have full access to local information), leading to weakened management of auditors and also inefficient job assignments.

Overall, the preceding discussion highlights that the impact of centralization on audit quality is ambiguous, and depends on the relative importance of a range of factors that stem from core theories of delegation, monitoring, and organizational design. Our primary purpose is to explore whether these features collectively lead to improved audit outcomes. Further, by exploring heterogeneity in the nature of the reform and its effects across different cities, we may begin to adjudicate among the relevance of these various factors in explaining how centralization impacts performance.

4 Data

Our sample consists of Chinese cities during 2013 to 2020.¹⁵ The sample period is dictated by two main considerations. First, we wish to avoid the confounding effects of the many reforms that accompanied Xi Jinping’s selection as the General Secretary in 2013 (though we note in practice that our results look very similar if we include data from 2006 onward, which is the earliest that comparable audit data are available). Second, post-2020 data are confounded by the effects of Covid.

We begin by identifying pilot provinces from the CPC’s (2015) File 58. According to this directive, there are seven pilot provincial administrative regions, including Jiangsu, Zhejiang, Shandong, Guangdong, Chongqing, Guizhou, and Yunnan (though recall that Chongqing, as a central administered municipality without multiple cities under its jurisdiction, is omitted from the analysis). The implementation of the reform described in the previous section began in July 2016, based on policy releases from these provinces.¹⁶ These provincial implementation plans directly affected all cities within the province, so all city-level audit offices have the same reform date. We treat 2016 and later as post-reform years.

¹⁵As a reminder, the unit we consider is the city, of which there are 333 in China, which are themselves administrative subdivisions within each of China’s provinces.

¹⁶See <https://china.huanqiu.com/article/9CaKrK0kUO> for the reform year and month information in Guangdong; see <http://www.wssj.com.cn/news/nbsj/2017-06-30/1897.html> for Yunnan; see <https://www.audit.gov.cn/n4/n19/c86718/content.html> for Jiangsu; see https://zjnews.zjol.com.cn/gdxw/ycxw_zxtf/201608/t20160802_1818609.shtml for Zhejiang; see <https://www.audit.gov.cn/oldweb/n4/n19/c86947/content.html> for Shandong; and see <https://www.audit.gov.cn/n4/n19/c86425/content.html> for Guizhou. All links last accessed December 2, 2025.

Pilot is a (time-invariant) indicator variable that takes on a value of one for each of the six provinces in our sample where the pilot reform was implemented, and *Post2016* denotes years 2016 and later, when the centralization reform was implemented.

Our primary data analysis looks at city audit outcomes, which we obtain from the China Audit Yearbooks. The National Audit Office compiles these yearbooks, which provide comprehensive data on national and local audit operations in China. The outcome of primary interest is (the natural log of) suspicious fiscal funds uncovered by a city audit office, $\log(\text{Susp Funds})$, which directly reflects a city audit office’s effectiveness in uncovering fiscal problems, whether due to improved efficiency or effort. Similar measures are used in other works on government auditing (e.g., Chu et al., 2021; Ferraz and Finan, 2011; Olken, 2007).

We also split suspicious funds into two elements: the number of audit projects completed by a city audit office that year (again taking the natural log, $\log(\text{Num Audits})$), and the quantity of suspicious funds per audit project ($\log(\text{Susp Funds}/\text{Num Audits})$). Data on the number of audit projects is also collected from the China Audit Yearbooks, and captures the workload completed by a city audit office.

We include as city-level controls the natural logarithm of GDP per capita ($\log(\text{GDP PC})$), population ($\log(\text{Population})$), and city government revenue ($\log(\text{Gov Rev})$); the ratio of city government expenditure to revenue (*Gov Balance*); the ratio of industrial output to GDP (*Industrial Ratio*); the FDI to GDP ratio (FDI/GDP) which is available only up to the end of 2019; and average number of years of education of local residents at the province level (*Avg Education*). These provincial and city economic statistics are from the China Statistical Yearbooks and China City Statistical Yearbooks.

We exclude Beijing, Shanghai, Tianjin, and Chongqing from our analysis, as they are the four municipalities that have provincial-level administrative status, and so do not have cities within their jurisdiction.

For our later analyses in which we look at the roles of auditor selection versus incentives, we collect the identities of city chief auditors from the official websites of individual audit offices and China Audit Yearbooks. Based on these names, we then hand-collect background information on each city chief auditor, using a range of sources, including the China Audit Yearbooks, official websites of each city audit office, and, as needed, the website *Baidu Encyclopedia*. Based on city chief auditors’ appointment dates, we distinguish between auditors appointed before (*Old Auditor*) versus after (*New Auditor*) the reform. We also use these background characteristics to generate the following covariates: indicator variables for whether the chief auditor is stationed in their home city (*Born Local*), whether they have local working experience (*Local Work Exp*), and whether they have work experience in either the central or provincial government, e.g., working in the National Audit Office or a provincial audit office, (*Central/Prov Work Exp*); education level (*Education Level*, taking on values of 1 for some college or less, 2 for undergraduate degree, 3 for master’s, and 4 for doctorate); and indicator variables for an educational background in economics, finance, or accounting (*Education Expert*), and working experience in government audit (*Audit Expertise*).

In the later analyses in which we look at the role of resources, we obtain the number of city audit staff from the China Audit Yearbooks, and detailed information on city audit budgets from city audit budget reports available on the official websites of individual audit offices and city governments.¹⁷

¹⁷See <http://www.audit.gov.cn/> for a complete list of city audit office websites in each provincial audit office website.

Based on these data, we construct variables (all in natural logs) including number of city office staff ($\log(Staff)$), city audit budget per employee ($\log(Budget\ PC)$), city fixed (or non-project based) budget per employee ($\log(Base\ Budget\ PC)$), and city variable (or project based) budget per employee ($\log(Project\ Budget\ PC)$). We make two observations about the breakdown of budgets into the base and project-based components. First, base expenditures are far higher, representing about 74 percent of overall budgets. Second, since we focus below on auditor compensation, we also observe that salaries and benefits are main components of base budgets.

Also from the China Audit Yearbooks, we compile proxies of the extent to which the National Audit Office (NAO) focused attention on the audit activities of particular provinces. Specifically, we create two (highly correlated) measures of such interest: the number of site visits from NAO leaders (including the Auditor General, Deputy Auditors General, Discipline Inspector General, Chief Auditor, and the Director of the Office of the Inter-Ministerial Joint Conference on Economic Responsibility Audit Work) to a province; and the subset of these visits that directly involve visits to local audit offices (at the province, city, and county levels). These data are derived from the daily records of NAO leaders' public activities fully released in the China Audit Yearbooks.

5 Research Design

Our source of identification is the “assignment” of provinces to participate in the audit centralization pilot, in order to study how such centralization impacts audit efficacy. Our primary approach is a difference-in-differences design, implemented as a two-way fixed-effects model with city and year fixed effects. This absorbs any time-invariant differences across provinces or the cities within them (e.g., quality of governance; income) or region-invariant differences over time (e.g., secular shifts in the quality of auditing).

That is, our basic estimating equation is given by:

$$AuditOutcome_{ct} = \beta_1 * Pilot_{c(p)} * Post2016_t + \beta * X_c + \gamma_c + \mu_t + \varepsilon_{ct} \quad (1)$$

for city c in province p and year t . $Pilot$ is a (time-invariant) indicator variable that takes on a value of one for each of the six provinces in which the pilot reform was implemented, and $Post2016$ denotes years 2016 and later, when the centralization reform was implemented. All specifications include city fixed effects γ_c and year fixed effects μ_t ; note that the city and year fixed effects absorb the direct effects of $Pilot$ and $Post2016$. The outcome variable, $AuditOutcome$, captures various audit outcomes that we consider in our analysis. Our primary outcome of interest is $\log(Susp\ Funds_{ct})$, the natural logarithm of suspicious fiscal expenditures uncovered by the audit office in city c in year t . We also decompose this value into suspicious funds uncovered per audit ($\log(Susp\ Funds/Num\ Audits)$) and number of audits completed by the city audit office ($\log(Num\ Audits)$). Standard errors in all specifications are clustered by province, as that is the level of assignment for the pilot reform. Following Chu et al. (2021), we

See https://www.gov.cn/home/2023-03/29/content_5748954.htm for a complete list of city government websites in each provincial government website. Last accessed December 2, 2025.

include as city-level controls the log values of GDP per capita, population, and city government revenue; the ratio of city government expenditure to revenue; the ratio of industrial output to GDP; the FDI to GDP ratio, and average education (at the province level). We use data from the baseline year of 2013; we also show that our results are robust to including time trends in the effects of these variables, allowing for their effect to differ post-reform, or allowing for the effect of baseline conditions to vary by year. The last of these is our preferred specification. We also use (time-varying) city-year controls, though we see this as suffering from a potential bad control problem, which in our case may have even more serious concerns, as recovered suspicious funds are recorded as government revenues.

In interpreting β_1 as the effect of audit centralization on audit outcomes, we are assuming that there are no omitted factors that are correlated with the (province-level) assignment to the pilot program and measures of audit quality. Probing threats to this identification is the main focus of this section.

We see the primary concern as the non-random assignment of provinces to the pilot program. Before describing the approaches we take to mitigating such concerns, we refer to the institutional background discussion above, where we noted that the central government’s explicit purpose in designing pilot policies is to evaluate what the effects may be if a reform is extended to the entire country. It is notable that the pilot we study here includes a range of provinces, both coastal and inland, rich and poor, which suggests some attempt at capturing responsiveness to the reform in a diverse set of institutional environments.

Naturally, there can be a gap between stated government policies and those enacted in practice. The most natural source of non-random assignment is that the government selects provinces based on the weakness of their local (city) audit offices, which have the greatest potential benefit from centralizing management to the province-level. We probe this possibility in a number of ways. First, we use the standard approach of assessing whether the outcome variable is balanced across pilot and non-pilot provinces, and also whether there are parallel trends prior to the reform in cities of pilot and non-pilot provinces. The existence of comparable trends would suggest that the difference-in-difference estimator is not simply capturing differential trends across the two groups, but rather the impact of the intervention itself (regardless of the reasons for initial assignment).

While the presence of parallel trends would be reassuring, there remain concerns over the interpretation of β_1 as the treatment effect of centralization on audit outcomes. Consider, in particular, our main outcome variable, $\log(SuspFunds_{ct})$. Suspicious funds is the product of two objects: *Problem Projects* $\times$ *Detection Rate*. Let us suppose that pilot assignment is made in part based on the central government’s assessment of provinces with the weakest detection rates, in an effort to improve them. It is still possible for there to be “pre-treatment” balance across pilot and non-pilot cities in suspicious fund detection, since low detection rates could plausibly be associated with higher levels of problematic public expenditure, with the product being indistinguishable from better-governed provinces. This could in turn affect how one might interpret the impact of centralization were it rolled out to the entire country (i.e., the average treatment effect may differ from the treatment-on-the-treated impact that we measure in our analyses below). In particular, suppose that there are diminishing returns to the impact of the reform – in poorly-governed provinces with very low detection rates (and high levels of problematic expenditures), we may see a bigger improvement than in better-governed provinces if the reform were extended to the whole country. To address this particular concern, we present analyses that

limit the set of pilot provinces to those with strong government institutions. If the preceding argument were to limit the efficacy of broader reforms, we would expect a smaller coefficient on β_1 , since we limit our treatment group to those where *ex ante* we anticipate that the impact on cities in these provinces (which already have effective oversight, at least in relative terms) would be more modest.

As an alternative approach to assessing the empirical importance of this concern, we collected data on audit efficacy as appraised by the National Audit Office in the years preceding the reform, which serves as a rough proxy of the central government’s assessment of local audit capabilities. Specifically, the primary means through which the NAO formally acknowledges strong performance by local audit offices is by recognition of “excellent audit projects” based on criteria discussed in further detail in Section 6.3. We expect that these proxies for audit quality would be relatively low in pilot provinces (whether or not one accounts for other covariates) if targeting under-performing audit offices were a primary consideration in assignment.¹⁸

The second primary threat to identification is that provinces assigned to the pilot program may see themselves as under particular scrutiny by the NAO, which was tasked with the implementation and supervision of the pilot program we study. This suggests a direct measure of attention: the number of site visits by NAO leaders from Beijing to a province. If the NAO was particularly attentive to the pilot reform we study, its leaders should make more frequent visits to pilot provinces post-reform. Their presence would be a symbolic statement about the pilot’s importance and, substantively, their presence could increase scrutiny of local audit activities. In both cases, we expect visits to induce greater audit effort.

To test whether such increased scrutiny appears in practice, we revisit our main specification in Equation (1), using measures of such visits as the outcome variable to assess whether there are any changes post-reform in pilot provinces. In this case, we interpret a null result on β_1 as evidence against increased concern of central government scrutiny induced by the pilot. Putting aside the frequency of such visits, their impact could also differ after the pilot’s implementation. We therefore also augment the specification in Equation (1) to include the interaction term $Pilot * Post2016 * Visit$, which captures whether the reform had a larger impact at times when a province may have been perceived as being under greater scrutiny by the NAO. We interpret a null result on this third-order term as evidence against increased scrutiny playing a role in any effect of the pilot on audit outcomes.

While we cannot fully rule out these alternative interpretations, we believe that the tests outlined above provide a credible approach to evaluating their likely relevance.

6 Results

6.1 Summary Statistics

Before proceeding to our regression analyses, we provide summary statistics on the main variables we use. Summary statistics for the city-year variables we use in our analysis appear in Table 1a, while the

¹⁸We present several further results to assess the likelihood that this is a major concern, but defer a discussion of these additional tests to Section 6.3.

variables we use for base year controls (with minimal time-series variation) appear in Table 1b, with their values from 2013; for ease of reference throughout, we provide definitions for all variables in Appendix Table A1. There are 2650 observations comprised of 333 cities with, subject to a handful of exceptions, 8 years of data.¹⁹ Given that there are 27 provinces in our sample, this works out to an average of 12 cities per province; the most populous province, Guangdong, has 21 cities, while Tibet has only 7. Table 1b also provides the average of *Pilot*, which is 0.26, indicating that the pilot provinces have marginally more cities per province than non-pilot ones.

In Table 1c, we compare the city attributes of observations with *Full Pilot*=1 versus *Partial Pilot*=1 versus *Pilot* = 0 at the start of our sample period, in 2013. We note that cities in full-reform provinces are the least developed, cities in control provinces are in the middle, and cities in partial-reform provinces are the most developed in terms of GDP per capita, FDI to GDP ratio, and average number of years of education. As we noted earlier, these differences underscore the importance of accounting for initial conditions (and their interactions with year fixed effects) in estimating the impact of the audit reform.

6.2 The impact of centralization on audit outcomes

In Table 2, we present our main results on the value of suspicious funds based on Equation (1). In column (1) we show results with only city and year fixed effects. Given the log scale, the coefficient on the interaction term *Pilot* * *Post2016* implies a 45 percent increase in suspicious funds uncovered post-reform. In the remaining columns we present results with the various controls described in Section 5. Column (2) includes city controls from the base year 2013 interacted with a time trend, column (3) includes base-year controls interacted with *Post2016*, and column (4) adds base-year controls interacted with year fixed effects.²⁰ The point estimate on *Pilot* * *Post2016* is largely unchanged. In column (5) we include city-year time-varying controls (which reduces the sample size from 2650 to 2318 since city-level FDI data are unavailable from 2020 onward); the coefficient on the interaction term falls by approximately a quarter to 0.28 (significant at the 5 percent level). Note, however, that as we explained earlier, these may be seen as bad controls given that city-level fiscal outcomes may be endogenous to the auditing regime – indeed, this may be a mechanical result, as recovered suspicious funds are part of government revenues.

We provide an event plot in Figure 2 to show the evolution of suspicious funds around the reform date of 2016 for pilot provinces relative to others, based on a specification similar to column (4) above. We see a distinct and sustained increase that coincides with $t = 0$ (i.e., 2016). We also note that the coefficients for pre-reform periods are statistically indistinguishable from zero, so that the parallel trend assumption required for a standard difference-in-difference test very likely holds in our setting. Online Appendix Table OA1 presents the dynamic effects over the window of $[-3, +4]$ where the omitted category is -1, i.e., one year before the reform.

¹⁹The city of Danzhou was only established in 2015, and is thus missing data from 2013 and 2014; the city of Laiwu was abolished at the end of 2018 and so is also missing two observations. Finally, audit results are missing from the China Audit Yearbooks for all 7 cities in Tibet in 2013; these data are also missing for a single year for each of three cities: Shannan (2020), Tacheng (2016), and Tongling (2015) for reasons we could not determine. Our results are robust to dropping all of these cities from the analysis in order to create a balanced panel.

²⁰In all cases, the direct effects of the base year controls are absorbed by the city fixed effects.

In Table 3, we explore whether this increase in suspicious funds uncovered results from an expansion in the number of audit projects or from an increase in the amount uncovered per audit project. To conserve space, we present fewer specifications for the results that follow: (1) the sparsest version with only city and year fixed effects; (2) base-year controls interacted with year fixed effects; (3) city-year controls (though the results for other specifications are near-identical to the second of these). Focusing first on the log of the number of audit projects, we find no change in columns (1)-(3). This suggests that centralization did not lead to more suspicious funds uncovered simply because of, say, a shift to a more comprehensive audit policy, in which (less well-informed) provincial audit offices audit *all* public projects post-reform. As expected given that the number of audits is essentially unchanged, we thus find in columns (4)-(6) an increase in suspicious funds uncovered per audit, with the magnitude comparable to that of overall suspicious funds uncovered.

To focus on more severe governance lapses, we use the log of the number of projects in which auditors found such severe violations that the project was referred to the judiciary or discipline inspection commissions for further investigation. These results appear in Appendix Table A2, based on the specification in Equation (1). We do not find any relative change in severe violations in pilot cities.

Overall, these results suggest that audit centralization led to improved audit outcomes. In the sections that follow we explore the robustness, potential interpretations, and underlying mechanisms for these primary findings.

6.3 Robustness and interpretation of the main results

6.3.1 Endogenous selection of pilot provinces

As discussed in Section 5, there are two main threats to interpreting our results as reflecting a causal impact of audit centralization on audit outcomes. First and foremost, there are concerns that stem from the non-random assignment to the pilot program. Of particular concern, if provinces were selected on the basis of low-functioning audit institutions, we may expect that this subset of provinces would overstate the improvements that would occur if centralization were implemented in an average province. That is, the treatment-on-the-treated estimate may overstate the average treatment effect.

There are several patterns in the data that argue against this being a first-order concern. First, as noted in Section 2, the regions assigned to the pilot were in a mix of higher and lower governance provinces. Specifically, Jiangsu, Guangdong, Shandong, and Zhejiang are relatively rich and well-governed provinces, while Guizhou and Yunnan are less well-developed and with less well-functioning government institutions.²¹ Thus, the actual selection of pilot provinces appears to be in line with the government’s claims that it aimed for representativeness to understand what the effect of reform would be if it were rolled out to the entire country.

Second, the premise of the above argument is that provinces with more serious audit problems see larger improvements from centralization; if this were the case, we would observe smaller improvements

²¹For example, according to the China Marketization Index, which is widely used in the accounting and economics literatures (e.g., Guan et al. (2016), see <https://cmi.ssap.com.cn>), the province’s rankings for the government quality sub-index in 2015 were: 3 for Guangdong, 4 for Jiangsu, 6 for Zhejiang, 8 for Shandong, 24 for Yunnan, and 26 for Guizhou.

associated with the pilot in relatively well-governed provinces. One approach is therefore to drop the more weakly governed provinces like Guizhou and Yunnan, leaving us with the subset of provinces with relatively strong governance (and thus starting from positions of stronger oversight). The result, which is similar to our main results in Table 2, appears in column (1) of Appendix Table A3 (see similar results in specifications with different sets of controls in Online Appendix Table OA2).

It is also instructive to consider the details of the particular reform we study here, as one of the pilot provinces, Guangdong (itself a very high-governance province) was not included in the initial list of pilot provinces. However, its leadership asked to be included in the first batch of reformers, because the province sees itself at the vanguard of reform more generally. Of course, this invokes a distinct sort of endogenous selection (and related to one we consider below of a “spotlight” on reformers), but it is somewhat distinct from what we see as the primary threat to interpretation described above. In column (2) of Appendix Table A3, we therefore repeat our main analyses including *only* cities in Guangdong as the treated group.²² (Since it is also the most populous province in China, its many cities – 21 in total – still allow for a plausible statistical test of the impact of the reform.) We again find that the point estimates on $Pilot * Post2016$ are similar to those in our main results. An alternative approach to ensuring a comparison of more similar cities is simply to try to better match pilot cities to their closest “neighbor” in non-pilot provinces based on pre-reform (2013) covariates, using propensity score matching. The result appears in column (3) of Appendix Table A3 (see the full set of propensity score matched results in Online Appendix Tables OA4a and OA4b), and again generates point estimate that is quite similar to those provided in our main results in Table 2.

It is possible that even provinces that are relatively high-functioning on average have weak audit institutions. In our final test, we therefore use additional data on audit efficacy as appraised by the National Audit Office in the years preceding the reform, which serves as a more direct proxy for audit efficacy prior to the reform. Specifically, the primary means through which the NAO formally acknowledges strong performance of local audit offices is by recognizing of “excellent audit projects” based on the extent to which (1) audit processes have complied with audit laws and regulations; (2) audit findings are backed by solid evidence, and dealt with using appropriate penalties and practical recommendations; (3) audits have revealed significant criminal cases, and (4) audit results have generated significant social impact.²³ This assignment is considered both an honor and reward for audit effort, and plays a role in audit team promotion. The data we use for this test are derived from the “excellent audit projects” lists available in the China Audit Yearbooks. These results, presented in Appendix Table A4, show no difference between “treated” and control city audit offices, which is as close as is feasible to testing directly for the concern about pre-pilot differences in audit efficacy.

In summary, while we cannot rule out that our results are driven in part by endogenous treatment assignment, we do not, in any of our approaches to assessing these concerns, find any evidence suggesting they play a dominant role in the patterns we document.

²²(See similar results in specifications with different sets of controls in Online Appendix Table OA3)

²³See <https://law.esnai.com/do.aspx?action=show&lawid=110756>, last accessed December 2, 2025.

6.3.2 Central government attention on pilot provinces

As described in Section 5, even if assignment were random, the response of local audit offices may not be reflective of what we would observe if the program were extended to the whole country. In particular, if administrators in local audit offices feel that they are the focus of increased scrutiny from the National Audit Office, they may be particularly scrupulous in their audit duties during the pilot. We therefore examine whether a direct measure of attention differentially changes in pilot provinces after the implementation of the reform. If the reform led to greater scrutiny from NAO officials, we would expect their visits to pilot provinces to increase. These results appear in the first two columns of Table 4. For both measures, we observe no change in the likelihood of a visit from NAO leadership in pilot provinces after the reform began. In columns (3) and (4) of Table 4 we augment our main regression specification in Equation (1) to include the interaction term $Pilot * Post2016 * Visit$ which, recall, captures whether the reform had a larger impact at times when a province may have been perceived as being under greater scrutiny by the NAO. We find that the coefficient on this higher-order term is small and statistically insignificant for both visit measures.

To the extent that our proxies are a credible reflection of central government attention on city audit offices, the evidence above is inconsistent with increased focus from the central government on pilot provinces – rather than centralization – driving our results.

6.3.3 Additional robustness checks

In this subsection we provide several additional robustness checks for our main findings.

Are cities in any individual pilot province driving the results? In Figure 3, we provide a “leave-out” analysis in which we provide the estimated coefficient on $Pilot \times Post2016$, excluding one pilot province at a time. The point estimates are very consistently signed and of qualitatively similar magnitudes to those we report in our main results, indicating that no individual province drives the patterns we observe in Section 6.2.

Are the results sensitive to the level of clustering? Given that “assignment” is at the province level, we use province-level clustering in our main analyses, which should provide more conservative estimates. To ensure that our results are not sensitive to this assumption, we take two approaches. First, we show the results using city-level clustering. Second, given the modest number of province clusters, we also provide results using wild bootstrap standard errors, as proposed by Cameron et al. (2008). These results appear in columns (4) and (5) of Appendix Table A3, where we show that our results are robust to both alternative choices of modeling standard errors.

Are the results picking up the effects of correlated reforms? One may be concerned that some other policy shock in 2016 could have differentially impacted cities in treated versus control provinces, or might overlap with the policy change we study here. We take several approaches to assessing this concern. First, we conducted a careful search of major economic and political reforms that might be centered around our “treatment” date, primarily based on reporting by Xinhua News, which lists all major economic and political reforms in China over the past 40 years.²⁴ We do not find any reform in 2015 or 2016 that uses

²⁴See http://www.xinhuanet.com/politics/2018-12/17/c_1123861055_4.htm for the link to the list of major reforms in

the same pilot provinces nor any reform that directly affects city-level audit outcomes. On the latter point, it is important to emphasize that our outcome of interest – suspicious funds – is somewhat specific to audit reform, and would not be expected to be affected by broader or differently-targeted reforms. (Consistent with this, we do not find any differential change on GDP per capita, for example, if we use it as an outcome in regressions following Equation (1)). Finally, it is possible that audit-focused reforms – too minor to be mentioned in Xinhua News but still consequential for audit office functioning – might also be correlated with the pilot reform we study. We thus read through *China Audit Yearbook 2017*, which includes major changes to China’s audit system in 2016, and we do not find any event other than the centralization reform we focus on here that could (differentially) affect city-level audit outcomes in our treated versus control provinces. Overall, while we cannot fully rule out the possibility of correlated policy shifts, we did not find any evidence which would suggest this is a major concern.

6.4 Discussion of potential mechanisms

We now turn to examining a pair of channels through which centralization may have improved audit outcomes: auditor selection and increased financial resources.

6.4.1 The role of auditor selection

The most straightforward approach to capture the extent to which the observed changes are driven by auditor turnover versus incentives is to add auditor fixed effects to Equation (1), so that the coefficient on $Pilot * Post2016$ captures the relative change in suspicious funds uncovered for a given chief auditor before versus after the reform. We present these results in Table 5. The estimates are, if anything, larger than those we observe in our main results in Table 2, suggesting that our findings are driven by the change in incentives and resources rather than personnel turnover. In the final column, we distinguish between auditors appointed before versus after the reform, estimating a model (naturally without auditor fixed effects, which would be over-identified) that includes $Pilot * Post2016 * New Auditor$ and $Pilot * Post2016 * Old Auditor$ as separate regressors. Again consistent with a minimal role for auditor selection resulting from the reform, the coefficients on both terms are quite similar (and statistically indistinguishable).

Also consistent with the interpretation that our main results are not driven by auditor selection, in Table 6 we find no difference in the attributes of chief auditors appointed before or after the reform in pilot provinces relative to unaffected ones. In particular, we show variants on Equation (1) with a range of outcomes that reflect chief auditors’ independence and competence. Also note that since these are auditor-level attributes, we use only one observation per auditor from their appointment year. Specifically, independence proxies include indicator variables for whether the chief auditor is stationed in their hometown, whether they have local working experience, and whether they have central or provincial working experience (e.g., working in the National Audit Office or a provincial audit office). Measures of auditor competence include education level (taking on values of 1 for some college or less, 2 for undergraduate degree, 3 for master’s, and 4 for doctorate); educational background in economics (e.g., public

China 2012-2018, which includes the period of reform we study here. Last accessed December 2, 2025

economics), finance (e.g., public finance), or accounting (including auditing); and working experience in government audit. The coefficients on $Pilot * Post2016 * New Auditor$ in specifications predicting these attributes are of varying signs and in no case approaches statistical significance.

Overall, we find no evidence that any shift in auditor selection played a role in driving the improvements we observe – incumbent auditors experienced similar improvements, and new auditors’ characteristics were no different from those hired prior to the reform.

6.4.2 The role of centralized budgeting

We next consider whether the other major feature of the reform – control over financial resources – played a significant role.

We begin by exploring whether, given the stronger financial footing of provincial governments, the reform was associated with an increase in auditing resources, focusing again on the baseline specification in Equation (1). Our outcome variables include (all values in logs): number of city audit office staff; city audit budget per employee; city “base” (non-project) budget per employee; and project budget per employee. As a reminder, base expenditures are about 74 percent of budgets, and encompass all personnel expenses, including salaries. Results with these outcomes appear in Table 7. Focusing on staffing, while there is a positive coefficient of moderate magnitude on $Pilot * Post2016$, it does not approach statistical significance ($p = 0.101$). We observe a more robust pattern for expenditure per audit employee in column (2): the coefficient on $Pilot * Post2016$ implies an increase of 13 percent in total expenditure in pilot provinces. Interestingly, we observe that the effect comes entirely from non-project expenditures (column (3)), with an increase of 15 percent, while we observe no change for project budgeting (column (4)). These results suggest that it is not necessarily outlays on project spending per se that leads to improved audit outcomes.

As an alternative approach to examining the role of centralized resources, we compare the effect of centralization in partial- versus full-reform provinces. Recall that there were two primary elements to centralization: personnel management and budgeting, but only a subset of provinces implemented both elements of the reform. In “partial-reform” provinces, only the former was centralized, whereas in what we term “full-reform” provinces both personnel and budgeting were centralized. We posit that, if budgeting played a major role in improvements from centralization, then we would expect a smaller impact in partial-reform provinces. We thus generate two variables, *Partial Pilot* and *Full Pilot*, which are indicators for whether a province was a partial- or full-reform province. In column (1) of Table 8, we present a specification following Equation (1), but including $Partial Pilot * Post2016$ and $Full Pilot * Post2016$ as covariates instead of $Pilot * Post2016$. Consistent with financial resources playing at least some role, the coefficient on $Full Pilot * Post2016$ is approximately 60 percent larger than that of $Partial Pilot * Post2016$, though this difference is not statistically significant ($p = 0.18$).

The budgetary implications of full centralization potentially differed widely across cities within a province. As we discussed in Section 2, budget centralization led not just to more resources (as suggested by the above results), but also a differential distribution of resources across richer cities (which already had the resources to provide sufficient funding for their audit offices prior to the reform) versus poorer

ones. We thus consider the change in both audit inputs and outputs as a function of whether a city is above or below the median GDP per capita in a province at the base year of 2013, for full- versus partial-reform provinces. We do so by augmenting the specification from column (1) with the interaction terms $Partial\ Pilot * Post2016 * Rich$ and $Full\ Pilot * Post2016 * Rich$, where $Rich$ is an indicator variable denoting above-median-income cities. (We emphasize that $Rich$ is a city-level variable that has within-province variation, which is what we exploit in this triple-difference test.) These results appear in column (2). Interestingly, we observe a sharp difference between richer and poorer cities in the effect of full versus partial reform. Focusing first on the terms $Partial\ Pilot * Post2016$ and $Full\ Pilot * Post2016$, these capture the impacts of full versus partial centralization in poorer cities (since $Rich = 0$). Notably, there is a substantially larger gap between the effects of full versus partial centralization. By contrast, for richer cities (which had sufficient resources prior to centralization) the difference is far more muted, as reflected in the large negative coefficient on $Full\ Pilot * Post2016 * Rich$ (significant at the 1 percent level). Column (3) further controls for province-by-year fixed effects, so that the identifying variation comes exclusively from within-province differences in city-level income. In this even more stringent specification, we find that in full-reform provinces there is a relatively large impact of centralization on audit outcomes in relatively poor cities; we find no such difference for partial reformers.

In columns (4) to (6) we repeat the same specifications as in columns (1) to (3), but with $\log(Budget\ PC)$ as the dependent variable. As expected, the budget increase is larger for full-reform provinces (column (4)). More interestingly, this masks very substantial heterogeneity for richer versus poorer cities. Focusing on poorer cities (i.e., the terms $Partial\ Pilot * Post2016$ and $Full\ Pilot * Post2016$), there is essentially no increase in resources in partial-reform cities, while there is a very large increase in full-reform ones in column (5). For rich cities, there is a modest increase for both, but no difference in partial-versus full-reform provinces. Also of relevance, we may see in column (5) that the effect of reform on budgets even in richer cities is positive (the sum of the coefficients on $Full\ Reform * Post2016$ and $Full\ Reform * Post2016 * Rich$), though not statistically different from zero, indicating that – contrary to some concerns expressed by officials in well-resourced locales – financial resources increased even in well-off cities. Column (6) shows that the rich versus poor difference in budgeting in full-reform provinces survives the inclusion of province-by-year fixed effects.

Overall, we interpret the evidence in this subsection as indicating a role for increased resources that came with budget centralization, but that these benefits accrued much more to poorer cities which, without transfers from the provincial government, were resource-constrained in implementing effective audits. However, the fact that, even in partial-reform provinces, we observe a substantial impact from centralization, suggests at least some role of political incentives – since appointment decisions were removed from city governments, local auditors may have had stronger incentives post-reform to rigorously audit local projects.

We conclude by noting that the results in this subsection rely on third-order interactions that effectively partition the data into a small number of “treated” cells – while there are numerous cities in each province, there are only four partial-reform provinces and two full-reform provinces. Hence, these findings should necessarily be interpreted with particular caution.

7 Conclusion

In this paper, we study the impact of a program that centralized city audits in six Chinese provinces in 2016. In a subset of these provinces, only personnel management was centralized, while others experienced both personnel and financial centralization. Using a difference-in-differences framework we show that centralization greatly improved audit outcomes, increasing detection of suspicious expenditures by over 50%. We find little role for auditor selection in driving these results, while analyses comparing partial- versus full-reform provinces indicate an important role for financial centralization in driving our results.

Broadly speaking, our findings highlight the importance in distinguishing among *types* of centralization and their consequences for public sector performance. Focused more specifically on auditing, our findings suggest that resources and incentives, rather than auditor identity, are crucial for driving audit outcomes.

The inconsistency in reforms across provinces that is part of our identification also prompts a number of further questions that warrant further research. In particular, despite the potential benefits of centralization, resistance to reform in relatively prosperous cities appears to have prevented wider adoption. This is what allows us to identify the uneven benefits of reform as a function of initial conditions (rich versus poor cities) and reform characteristics (full versus partial reform). Yet it raises the question of whether and how policies that improve public monitoring can be implemented, despite bureaucratic resistance. We leave this and other questions for future research.

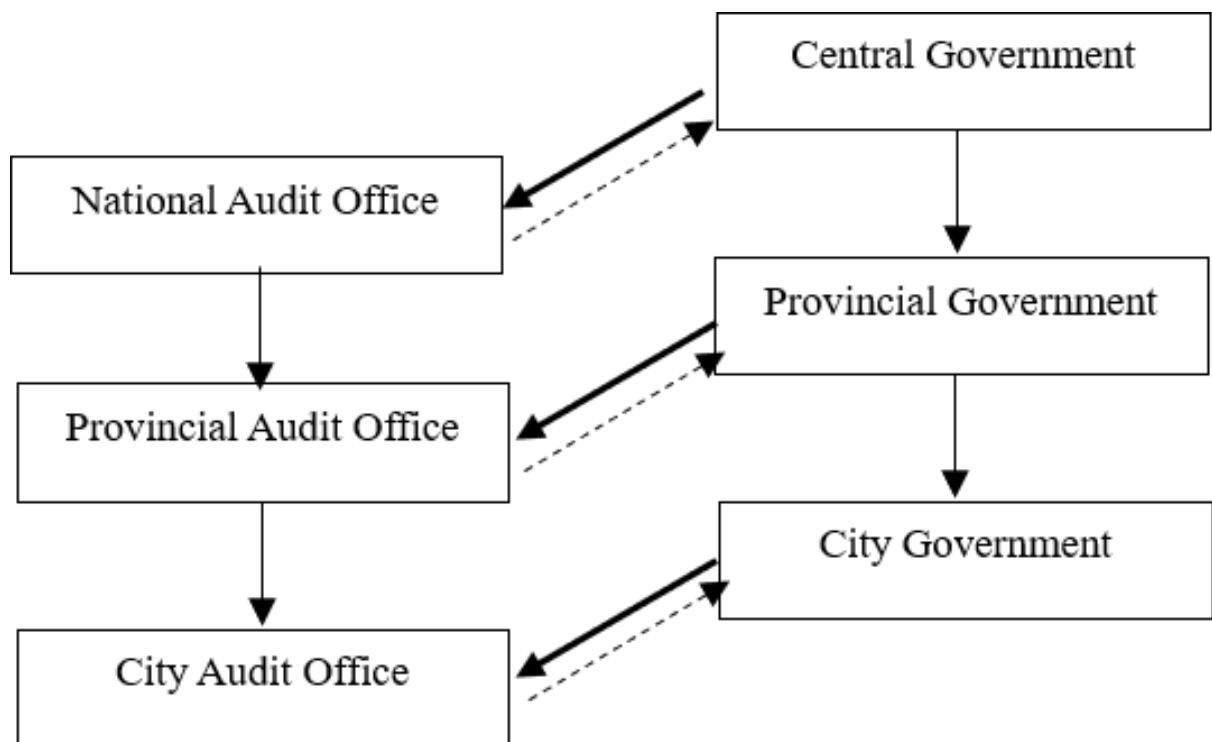
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Notes: The figure shows the administrative structure and relationships among governments and audit offices at each administrative level before the centralization reform. The solid line captures administrative control and the dashed line is for audit function.

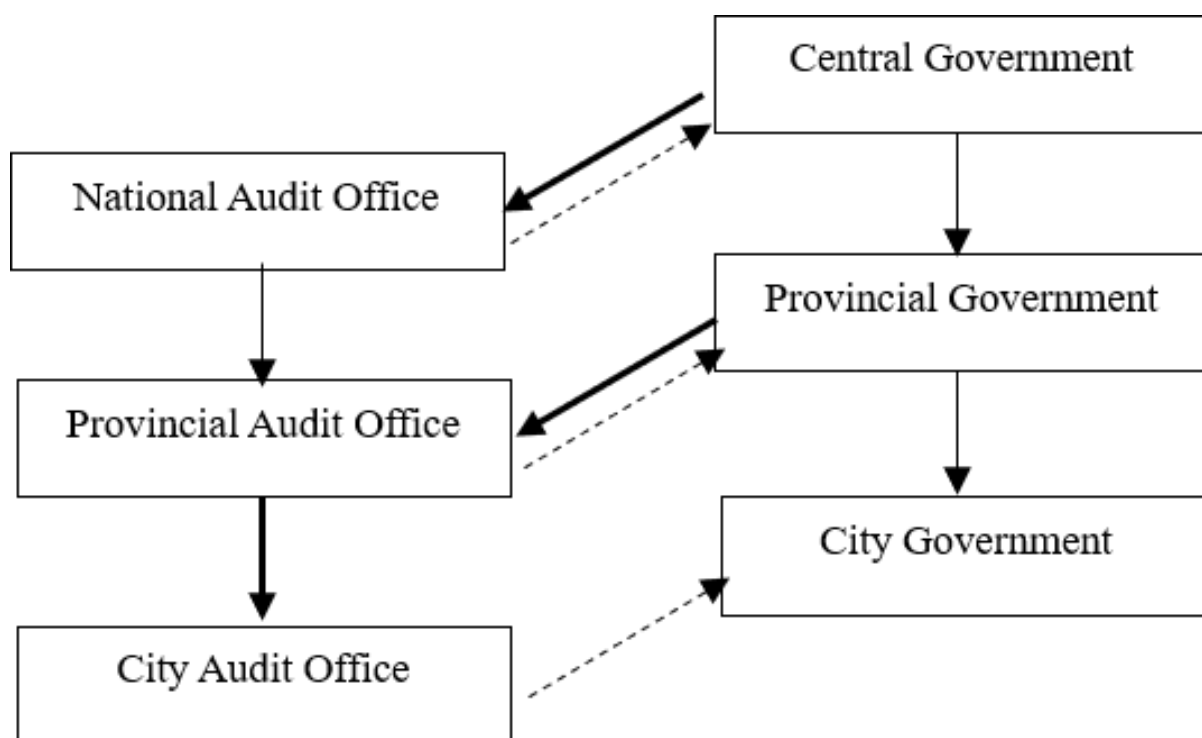


Figure 1b: The Structure of Audit Organization after the Reform

Notes: The figure shows the administrative structure and relationships among governments and audit offices at each administrative level after the centralization reform. The solid line captures administrative control and the dashed line is for audit function.

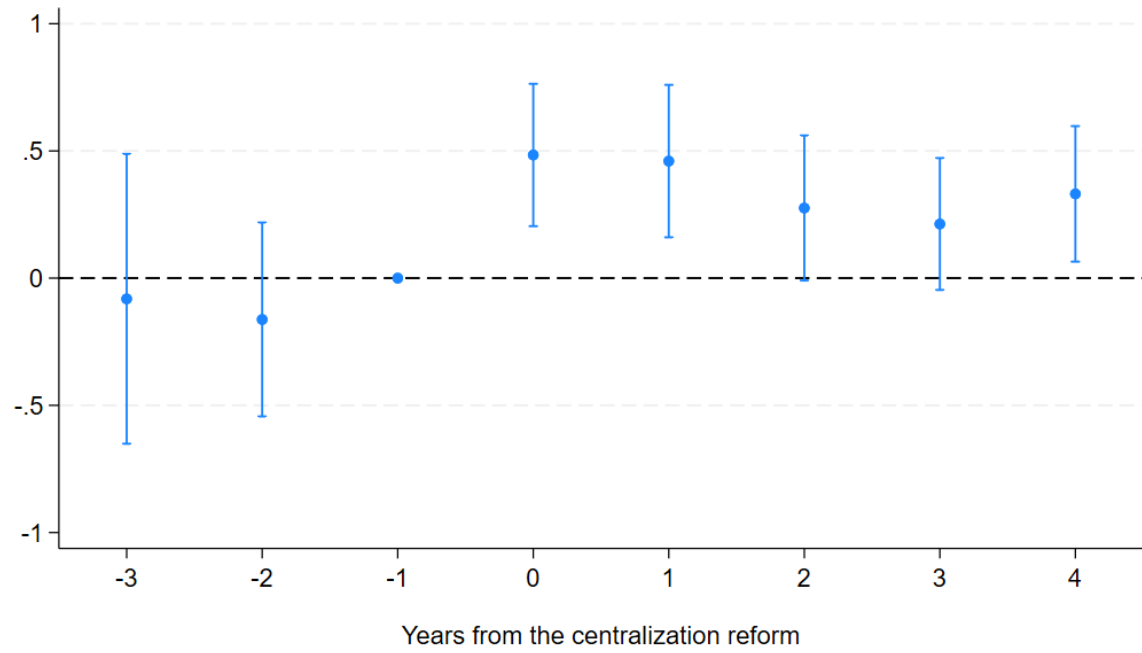


Figure 2: The Dynamic Effect on $\log(Susp Fund)$

Notes: The figure shows the coefficients and associated 90% confidence intervals from estimating the leads and lags regression, where the dependent variable is $\log(Susp Fund)$, and controls include Year indicator variables interacted with $\log(GDP PC)$, $Industrial Ratio$, $\log(Population)$, $\log(Gov Rev)$, $Gov Balance$, FDI/GDP , and $Avg Education$ in 2013. All effects are relative to the year prior to the reform (i.e., 2015). Standard errors are clustered by province.

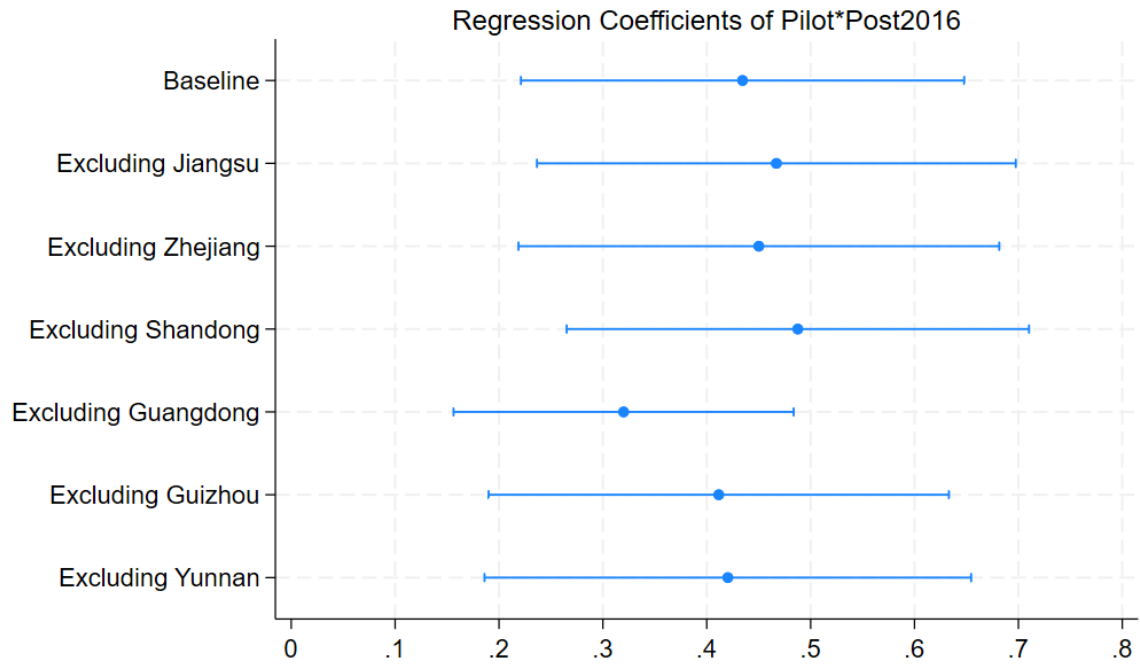


Figure 3: The Leave-Out Analysis on $\log(\text{Susp Funds})$

Notes: The figure shows the coefficients and associated 90% confidence intervals from estimating the regression of leave-out analysis, where excluding one pilot province at a time. The dependent variable is $\log(\text{Susp Fund})$, and controls include Year indicator variables interacted with $\log(\text{GDP PC})$, Industrial Ratio , $\log(\text{Population})$, $\log(\text{Gov Rev})$, Gov Balance , FDI/GDP , and Avg Education in 2013. Standard errors are clustered by province.

Table 1a: Summary Statistics of City-Year Variables

Variables	Observations	Mean	StdDev	P25	Median	P75
<i>log(Susp Funds)</i>	2650	12.762	1.576	11.836	12.876	13.784
<i>log(Susp Funds/Num Audits)</i>	2650	8.746	1.516	7.844	8.865	9.761
<i>log(Num Audits)</i>	2650	4.017	0.654	3.638	3.989	4.394
<i>Post2016</i>	2650	0.627	0.484	0.000	1.000	1.000
<i>Pilot</i>	2650	0.262	0.440	0.000	0.000	1.000
<i>Full Pilot</i>	2650	0.075	0.264	0.000	0.000	0.000
<i>Partial Pilot</i>	2650	0.186	0.390	0.000	0.000	0.000
<i>New Auditor</i>	2650	0.226	0.418	0.000	0.000	0.000
<i>Born Local</i>	2650	0.345	0.476	0.000	0.000	1.000
<i>Local Work Exp</i>	2650	0.992	0.087	1.000	1.000	1.000
<i>Central/Prov Work Exp</i>	2650	0.007	0.082	0.000	0.000	0.000
<i>Education Level</i>	2650	2.224	0.590	2.000	2.000	3.000
<i>Education Expert</i>	2650	0.215	0.411	0.000	0.000	0.000
<i>Audit Expertise</i>	2650	0.171	0.377	0.000	0.000	0.000
<i>log(Staff)</i>	2650	4.011	0.466	3.738	3.989	4.290
<i>log(Budget PC)</i>	1954	3.337	0.397	3.075	3.311	3.554
<i>log(Base Budget PC)</i>	1904	2.990	0.406	2.738	2.962	3.214
<i>log(Project Budget PC)</i>	1783	1.954	0.889	1.529	2.047	2.514
<i>log(GDP PC)</i>	2646	10.616	0.745	10.168	10.575	11.079
<i>Industrial Ratio</i>	2646	0.435	0.114	0.363	0.443	0.510
<i>log(Population)</i>	2648	5.690	0.846	5.259	5.821	6.291
<i>log(Gov Rev)</i>	2646	13.806	1.124	13.143	13.804	14.431
<i>Gov Balance</i>	2646	3.844	4.228	1.821	2.730	4.158
<i>FDI/GDP</i>	2318	0.016	0.029	0.001	0.008	0.022
<i>Avg Education</i>	2650	8.837	1.118	8.129	9.016	9.562

Notes: The sample covers city-year panel observations from 2013 to 2020. See Appendix Table A1 for detailed definitions of the variables.

Table 1b: Summary Statistics of Baseline (2013) City Statistics

Variables	Observations	Mean	StdDev	P25	Median	P75
<i>Pilot</i>	333	0.261	0.440	0.000	0.000	1.000
<i>Full Pilot</i>	333	0.075	0.264	0.000	0.000	0.000
<i>Partial Pilot</i>	333	0.186	0.390	0.000	0.000	0.000
<i>log(GDP PC)</i>	333	10.509	0.682	10.031	10.424	10.926
<i>Industrial Ratio</i>	333	0.495	0.112	0.432	0.506	0.560
<i>log(Population)</i>	333	5.672	0.848	5.239	5.818	6.288
<i>log(Gov Rev)</i>	333	13.610	1.113	13.026	13.614	14.252
<i>Gov Balance</i>	333	3.323	3.158	1.638	2.357	3.557
<i>FDI/GDP</i>	333	0.016	0.017	0.002	0.011	0.023
<i>Avg Education</i>	333	9.867	0.834	9.587	10.096	10.251

Notes: The sample covers cross-sectional city observations in 2013. See Appendix Table A1 for detailed definitions of the variables.

Table 1c: Comparison of Baseline (2013) City Statistics, by Pilot Status

Variables	Pilot		Full Pilot		Partial Pilot		Non Pilot		Pilot vs. Non		Full vs. Partial	
	Mean	StdDev	Mean	StdDev	Mean	StdDev	Mean	StdDev	Difference	t-statistic	Difference	t-statistic
<i>log(GDP PC)</i>	10.706	0.813	9.967	0.443	11.003	0.736	10.440	0.616	0.266	1.300	-1.037	-11.949
<i>Industrial Ratio</i>	0.476	0.078	0.417	0.083	0.499	0.062	0.501	0.121	-0.026	-1.190	-0.082	-5.853
<i>log(Population)</i>	5.950	0.640	5.629	0.749	6.079	0.545	5.574	0.890	0.376	2.040	-0.450	-1.905
<i>log(Gov Rev)</i>	14.205	1.038	13.352	0.857	14.548	0.902	13.399	1.062	0.805	2.590	-1.196	-3.761
<i>Gov Balance</i>	2.136	1.305	3.498	1.620	1.587	0.551	3.743	3.498	-1.607	-2.558	1.911	5.293
<i>FDI/GDP</i>	0.017	0.015	0.006	0.008	0.021	0.016	0.015	0.018	0.001	0.263	-0.015	-3.736
<i>Avg Education</i>	9.856	0.454	9.201	0.231	10.120	0.152	9.871	0.933	-0.015	-0.057	-0.919	-4.816

Notes: Pilot indicates the group of each reform province. Full Pilot indicates the group of each full-reform province. Partial Pilot indicates the group of each partial-reform province. Non Pilot indicates the group of each non-reform province. See Appendix Table A1 for detailed definitions of the variables. T-statistics are calculated based on a regression of each outcome, clustering at the province level.

Table 2: The Relationship Between Audit Centralization and Audit Quality

Dependent Variable	(1)	(2)	(3)	(4)	(5)
	<i>log(Susp Funds)</i>				
<i>Pilot * Post2016</i>	0.369** (0.135)	0.410*** (0.145)	0.394*** (0.132)	0.434*** (0.125)	0.287** (0.136)
Year FEs	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes
Controls2013*Time trend		Yes			
Controls2013*Post2016			Yes		
Controls2013*Year dummy				Yes	
Controls					Yes
Observations	2650	2650	2650	2650	2318
R-Squared	0.659	0.665	0.663	0.677	0.675
Mean of Dep. Var.	12.762	12.762	12.762	12.762	12.724

Notes: Standard errors in parentheses are clustered by province. The sample covers the period from 2013 to 2020. The dependent variable in columns (1)-(5) is *log(Susp Funds)*, which is the natural logarithm of suspicious fiscal funds uncovered by a city audit office. *Pilot* is an indicator variable denoting pilot provinces and *Post2016* denotes years 2016 and later after the centralization reform was implemented. Controls in column (2) include Time trend interacted with *log(GDP PC)*, *Industrial Ratio*, *log(Population)*, *log(Gov Rev)*, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. Controls in column (3) include *Post2016* interacted with *log(GDP PC)*, *Industrial Ratio*, *log(Population)*, *log(Gov Rev)*, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. Controls in column (4) include Year indicator variables interacted with *log(GDP PC)*, *Industrial Ratio*, *log(Population)*, *log(Gov Rev)*, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. Controls in column (5) include *log(GDP PC)*, *Industrial Ratio*, *log(Population)*, *log(Gov Rev)*, *Gov Balance*, *FDI/GDP*, and *Avg Education*. The coefficients and standard errors of the control variables are suppressed to conserve space. See Appendix Table A1 for detailed definitions of the control variables.

Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Number of Audit Projects versus Suspicious Funds per Project

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(\text{Num Audits})$			$\log(\text{Susp Funds}/\text{Num Audits})$		
<i>Pilot * Post2016</i>	0.039 (0.095)	0.023 (0.107)	0.057 (0.093)	0.330** (0.132)	0.412*** (0.101)	0.230* (0.133)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls2013*Year dummy		Yes			Yes	
Controls			Yes			Yes
Observations	2650	2650	2318	2650	2650	2318
R-Squared	0.612	0.635	0.640	0.615	0.633	0.636
Mean of Dep. Var.	4.017	4.017	4.036	8.746	8.746	8.688

Notes: Standard errors in parentheses are clustered by province. The sample covers the period from 2013 to 2020. The dependent variable in columns (1)-(3) is $\log(\text{Num Audits})$, which is the natural logarithm of the number of audit projects. The dependent variable in columns (4)-(6) is $\log(\text{Susp Funds}/\text{Num Audits})$, which is the natural logarithm of suspicious fiscal funds per audit project. *Pilot* is an indicator variable denoting pilot provinces and *Post2016* denotes years 2016 and later after the centralization reform was implemented. Controls in columns (2) and (5) include Year indicator variables interacted with $\log(\text{GDP PC})$, *Industrial Ratio*, $\log(\text{Population})$, $\log(\text{Gov Rev})$, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. Controls in columns (3) and (6) include $\log(\text{GDP PC})$, *Industrial Ratio*, $\log(\text{Population})$, $\log(\text{Gov Rev})$, *Gov Balance*, *FDI/GDP*, and *Avg Education*. The coefficients and standard errors of the control variables are suppressed to conserve space. See Appendix Table A1 for detailed definitions of the control variables.

Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Attention from the Central Government

Dependent Variable	(1) <i>Visit</i>	(2) <i>Visit Audit</i>	(3) <i>log(Susp Funds)</i>	(4) <i>log(Susp Funds)</i>
<i>Pilot * Post2016</i>	-0.068 (0.090)	0.107 (0.082)	0.466*** (0.132)	0.418** (0.154)
<i>Pilot * Post2016 * Visit</i>			-0.043 (0.120)	
<i>Pilot * Post2016 * Visit Audit</i>				0.048 (0.127)
<i>Visit</i>			-0.092 (0.084)	
<i>Visit Audit</i>				-0.125 (0.084)
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes
Controls2013*Year dummy	Yes	Yes	Yes	Yes
Observations	2650	2650	2650	2650
R-Squared	0.418	0.535	0.677	0.677
Mean of Dep. Var.	0.655	0.355	12.762	12.762

Notes: Standard errors in parentheses are clustered by province. The sample covers the period from 2013 to 2020. The dependent variable in column (1) is *Visit*, which is an indicator variable denoting a site visit to a province (including its cities and counties) by leaders of the National Audit Office. The dependent variable in column (2) is *Visit Audit*, which is an indicator variable denoting a site visit to the audit offices of a province (including its cities and counties) by leaders of the National Audit Office. The dependent variable in columns (3)-(4) is *log(Susp Funds)*, which denotes the natural logarithm of suspicious fiscal funds uncovered by a city audit office. *Pilot* is an indicator variable denoting pilot provinces and *Post2016* denotes years 2016 and later after the centralization reform was implemented. Controls in columns (1)-(4) include Year indicator variables interacted with *log(GDP PC)*, *Industrial Ratio*, *log(Population)*, *log(Gov Rev)*, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. The coefficients and standard errors of the control variables are suppressed to conserve space. See Appendix Table A1 for detailed definitions of the control variables.

Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: The Role of Auditor Selection

Dependent Variable	(1)	(2)	(3)	(4)
		$\log(Susp Funds)$		
<i>Pilot * Post2016</i>	0.505*** (0.175)	0.542*** (0.149)	0.447** (0.176)	
<i>Pilot * Post2016 * New Auditor</i>				0.378** (0.141)
<i>Pilot * Post2016 * Old Auditor</i>				0.456*** (0.134)
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes
Auditor FEs	Yes	Yes	Yes	
Controls2013*Year dummy		Yes		Yes
Controls			Yes	
Observations	2539	2539	2159	2650
R-Squared	0.735	0.747	0.733	0.677
Mean of Dep. Var.	12.770	12.770	12.727	12.762

Notes: Standard errors in parentheses are clustered by province. The sample covers the period from 2013 to 2020. The dependent variable in columns (1)-(4) is $\log(Susp Funds)$, which is the natural logarithm of suspicious fiscal funds uncovered by a city audit office. *Pilot* is an indicator variable denoting pilot provinces and *Post2016* denotes years 2016 and later after the centralization reform was implemented. *New Auditor* is an indicator variable denoting city auditors appointed after the centralization reform. *Old Auditor* is an indicator variable denoting city auditors appointed before the centralization reform. Controls in columns (2) and (4) include Year indicator variables interacted with $\log(GDP PC)$, *Industrial Ratio*, $\log(Population)$, $\log(Gov Rev)$, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. Controls in column (3) include $\log(GDP PC)$, *Industrial Ratio*, $\log(Population)$, $\log(Gov Rev)$, *Gov Balance*, *FDI/GDP*, and *Avg Education*. The coefficients and standard errors of the control variables are suppressed to conserve space. See Appendix Table A1 for detailed definitions of the control variables.

Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Comparison of New and Old Auditors

Dependent Variable	(1) <i>Born Local</i>	(2) <i>Local Work Exp</i>	(3) <i>Central/Prov Work Exp</i>	(4) <i>Education Level</i>	(5) <i>Education Expert</i>	(6) <i>Audit Expertise</i>
<i>Pilot * Post2016 * New Auditor</i>	0.144 (0.108)	-0.001 (0.018)	0.007 (0.020)	0.175 (0.145)	-0.095 (0.092)	0.039 (0.099)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls2013*Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Observations	678	678	678	678	678	678
R-Squared	0.539	0.569	0.593	0.592	0.565	0.512
Mean of Dep. Var.	0.341	0.991	0.010	2.242	0.220	0.159

Notes: Standard errors in parentheses are clustered by province. The sample covers the period from 2013 to 2020 with the first year of each auditor. The dependent variable in column (1) is *Born Local*, which is an indicator variable denoting the city auditor is stationed in his home city. The dependent variable in column (2) is *Local Work Exp*, which is an indicator variable denoting the city auditor has local working experience at the city level. The dependent variable in column (3) is *Central/Prov Work Exp*, which is an indicator variable denoting the city auditor has central or provincial working experience. The dependent variable in column (4) is *Education Level*, which denotes the city auditor's education level (taking on values of 1 for some college or less, 2 for undergraduate degree, 3 for master's, and 4 for doctorate). The dependent variable in column (5) is *Education Expert*, which is an indicator variable denoting the city auditor's education background in economics, finance, or accounting. The dependent variable in column (6) is *Audit Expertise*, which is an indicator variable denoting the city auditor has working experience in government audit. *Pilot* is an indicator variable denoting pilot provinces and *Post2016* denotes years 2016 and later after the centralization reform was implemented. *New Auditor* is an indicator variable denoting city auditors appointed after the centralization reform. Controls in columns (1)-(6) include Year indicator variables interacted with *log(GDP PC)*, *Industrial Ratio*, *log(Population)*, *log(Gov Rev)*, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. The coefficients and standard errors of the control variables are suppressed to conserve space. See Appendix Table A1 for detailed definitions of the control variables.

Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: The Role of Centralized Budgeting

Dependent Variable	(1)	(2)	(3)	(4)
	$\log(Staff)$	$\log(Budget\ PC)$	$\log(Base\ Budget\ PC)$	$\log(Project\ Budget\ PC)$
<i>Pilot * Post2016</i>	0.059 (0.035)	0.123* (0.067)	0.139** (0.057)	0.049 (0.111)
Year FEs	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes
Controls2013*Year dummy	Yes	Yes	Yes	Yes
Observations	1950	1950	1901	1776
R-Squared	0.949	0.755	0.779	0.682
Mean of Dep. Var.	4.042	3.337	2.990	1.955

Notes: Standard errors in parentheses are clustered by province. The sample covers the period from 2013 to 2020 with no missing value of $\log(Budget\ PC)$. The dependent variable in column (1) is $\log(Staff)$, which is the natural logarithm of the number of city audit staff. The dependent variable in column (2) is $\log(Budget\ PC)$, which is the natural logarithm of the amount of city audit budget (in expenditure caliber) divided by the number of city audit staff. The dependent variable in column (3) is $\log(Base\ Budget\ PC)$, which is the natural logarithm of the amount of city audit budget for basic expenditure divided by the number of city audit staff. The dependent variable in column (4) is $\log(Project\ Budget\ PC)$, which is the natural logarithm of the amount of city audit budget for project expenditure divided by the number of city audit staff. *Pilot* is an indicator variable denoting pilot provinces and *Post2016* denotes years 2016 and later after the centralization reform was implemented. Controls in columns (1)-(4) include Year indicator variables interacted with $\log(GDP\ PC)$, *Industrial Ratio*, $\log(Population)$, $\log(Gov\ Rev)$, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. The coefficients and standard errors of the control variables are suppressed to conserve space. See Appendix Table A1 for detailed definitions of the control variables.

Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Full versus Partial Reform, and Heterogeneity by Development

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
	$\log(Susp Funds)$			$\log(Budget PC)$		
<i>Partial Pilot * Post2016</i>	0.373** (0.148)	0.406** (0.160)		0.098 (0.077)	0.029 (0.077)	
<i>Full Pilot * Post2016</i>	0.595*** (0.150)	0.721*** (0.167)		0.205** (0.091)	0.280*** (0.059)	
<i>Partial Pilot * Post2016 * Rich</i>		-0.086 (0.154)	-0.122 (0.173)		0.133** (0.062)	0.044 (0.051)
<i>Full Pilot * Post2016 * Rich</i>		-0.236*** (0.064)	-0.238*** (0.073)		-0.159* (0.080)	-0.200*** (0.054)
Year FEs	Yes	Yes	No	Yes	Yes	No
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year FEs	No	No	Yes	No	No	Yes
Controls2013*Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2650	2650	2650	1950	1950	1941
R-Squared	0.677	0.677	0.721	0.755	0.756	0.819
Mean of Dep. Var.	12.762	12.762	12.762	3.337	3.337	3.338

Notes: Standard errors in parentheses are clustered by province. The sample covers the period from 2013 to 2020. The dependent variable in columns (1)-(3) is $\log(Susp Funds)$, which denotes the natural logarithm of suspicious fiscal funds uncovered by a city audit office. The dependent variable in columns (4)-(6) is $\log(Budget PC)$, which denotes the natural logarithm of amount of city audit budget (in expenditure caliber) divided by the number of city audit staff. *Partial Pilot* is an indicator variable denoting pilot provinces where the reform included personnel centralization but not budget centralization and *Full Pilot* denotes pilot provinces where the reform included both personnel and budget centralization. *Post2016* denotes years 2016 and later after the centralization reform was implemented. *Rich* is an indicator variable denoting city is more developed in GDP per capita than median in its province. Controls in columns (1)-(6) include Year indicator variables interacted with $\log(GDP PC)$, *Industrial Ratio*, $\log(Population)$, $\log(Gov Rev)$, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. The coefficients and standard errors of the control variables are suppressed to conserve space. See Appendix Table A1 for detailed definitions of the control variables.

Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A1: Variable Definitions

Variables	Definitions
Dependent Variables	
$\log(\text{Susp Funds})$	Natural logarithm of suspicious fiscal funds uncovered by a city audit office.
$\log(\text{Susp Funds}/\text{Num Audits})$	Natural logarithm of suspicious fiscal funds per audit project.
$\log(\text{Num Audits})$	Natural logarithm of the number of audit projects.
Independent Variables	
<i>Post2016</i>	An indicator for years 2016 and later.
<i>Pilot</i>	An indicator for each reform province.
<i>Full Pilot</i>	An indicator for each full-reform province.
<i>Partial Pilot</i>	An indicator for each partial-reform province.
Mechanism Variables	
<i>New Auditor</i>	An indicator for the city auditor appointed after the centralization reform.
<i>Old Auditor</i>	An indicator for the city auditor appointed before the centralization reform.
<i>Born Local</i>	An indicator for the city auditor stationed in his home city.
<i>Local Work Exp</i>	An indicator for the city auditor's city-level local working experience.
<i>Central/Prov Work Exp</i>	An indicator for the city auditor's central or provincial working experience.
<i>Education Level</i>	The city auditor's education level, with 1 for some college or less, 2 for undergraduate degree, 3 for master's, and 4 for doctorate.
<i>Education Expert</i>	An indicator for the city auditor's education background in economics, finance, or accounting.
<i>Audit Expertise</i>	An indicator for the city auditor's working experience in government audit.
$\log(\text{Staff})$	Natural logarithm of the number of city audit staff.
$\log(\text{Budget PC})$	Natural logarithm of city audit budget per audit staff.
$\log(\text{Base Budget PC})$	Natural logarithm of city audit basic budget per audit staff.
$\log(\text{Project Budget PC})$	Natural logarithm of city audit project budget per audit staff.
<i>Rich</i>	An indicator for the city with above-median GDP per capita in its province.
Control Variables	
$\log(\text{GDP PC})$	Natural logarithm of city GDP per capita.
<i>Industrial Ratio</i>	Ratio of city industrial output to GDP.
$\log(\text{Population})$	Natural logarithm of city population.
$\log(\text{Gov Rev})$	Natural logarithm of city fiscal revenue.
<i>Gov Balance</i>	Ratio of city fiscal expenditure to revenue.
<i>FDI/GDP</i>	City foreign direct investment scaled by GDP.
<i>Avg Education</i>	Average number of years of education of local residents at the province level.
Other Variables	
<i>Severity Ratio</i>	Ratio of severe suspicious fiscal funds to all suspicious fiscal funds that need to be rectified.
$\log(\text{Transferred Cases})$	Natural logarithm of one plus the number of audited cases transferred to discipline inspection commissions, judicial authorities, or other departments.
<i>Excellent Audits</i>	Number of excellent city audit projects as appraised by the National Audit Office.
<i>Visit</i>	An indicator for a site visit to a province (including its cities and counties) by leaders of the National Audit Office.
<i>Visit Audit</i>	An indicator for a site visit to the audit offices of a province (including its cities and counties) by leaders of the National Audit Office.

Table A2: The Severity of Violations Uncovered by Audit Projects

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Severity Ratio</i>			<i>log(Transferred Cases)</i>		
<i>Pilot * Post2016</i>	0.032 (0.036)	0.046 (0.043)	0.054* (0.031)	-0.021 (0.232)	0.007 (0.226)	-0.144 (0.211)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls2013*Year dummy		Yes			Yes	
Controls			Yes			Yes
Observations	1613	1613	1613	2650	2650	2318
R-Squared	0.398	0.410	0.404	0.600	0.616	0.635
Mean of Dep. Var.	0.519	0.519	0.519	2.053	2.053	2.020

Notes: Standard errors in parentheses are clustered by province. The sample covers the period from 2013 to 2020, while only to 2017 for *Severity Ratio* due to data availability. The dependent variable in columns (1)-(3) is *Severity Ratio*, which denotes the ratio of severe suspicious fiscal funds (misappropriated funds need to return to previous usage and outstanding funds need to turn over to the treasury) to all suspicious fiscal funds that need to be rectified. The dependent variable in columns (4)-(6) is *log(Transferred Cases)*, which denotes the natural logarithm of one plus the number of audited cases transferred to discipline inspection commissions, judicial authorities, or other relevant departments. *Pilot* is an indicator variable denoting pilot provinces and *Post2016* denotes years 2016 and later after the centralization reform was implemented. Controls in columns (2) and (5) include Year indicator variables interacted with *log(GDP PC)*, *Industrial Ratio*, *log(Population)*, *log(Gov Rev)*, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. Controls in columns (3) and (6) include *log(GDP PC)*, *Industrial Ratio*, *log(Population)*, *log(Gov Rev)*, *Gov Balance*, *FDI/GDP*, and *Avg Education*. The coefficients and standard errors of the control variables are suppressed to conserve space. See Appendix Table A1 for detailed definitions of the control variables.

Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A3: Robustness Tests

Dependent Variable	(1)	(2)	(3)	(4)	(5)
	<i>log(Susp Funds)</i>				
<i>Pilot * Post2016</i>	0.375** (0.147)	0.679*** (0.115)	0.443*** (0.151)	0.434*** (0.123)	0.434*** (0.134)
Year FEs	Yes	Yes	Yes	Yes	Yes
City FEs	Yes	Yes	Yes	Yes	Yes
Controls2013*Year dummy	Yes	Yes	Yes	Yes	Yes
Observations	2450	2124	1016	2650	2650
R-Squared	0.682	0.647	0.659	0.677	0.677
Mean of Dep. Var.	12.773	12.577	13.206	12.762	12.762

Notes: Standard errors in parentheses in columns (1)-(3) are clustered by province and in column (4) by city. Column (5) provides wild-bootstrap standard errors clustered by province. The sample covers the period from 2013 to 2020. Column (1) excludes cities in Guizhou and Yunnan; column (2) excludes cities in Jiangsu, Zhejiang, Shandong, Guizhou, and Yunnan; column (3) provides propensity score matched result using one-by-one nearest neighbor matching with controls in 2013. The dependent variable in columns (1)-(5) is $\log(Susp Funds)$, which denotes the natural logarithm of suspicious fiscal funds uncovered by a city audit office. *Pilot* is an indicator variable denoting pilot provinces and *Post2016* denotes years 2016 and later after the centralization reform was implemented. Controls in columns (1)-(5) include Year indicator variables interacted with $\log(GDP PC)$, *Industrial Ratio*, $\log(Population)$, $\log(Gov Rev)$, *Gov Balance*, *FDI/GDP*, and *Avg Education* in 2013. The coefficients and standard errors of the control variables are suppressed to conserve space. See Appendix Table A1 for detailed definitions of the control variables.

Significance: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A4: Comparison of Audit Efficacy before the Reform

Variable	Pilot		Non Pilot		Pilot vs. Non	
	Mean	StdDev	Mean	StdDev	Difference	t-statistic
<i>Excellent Audits</i>	0.157	0.395	0.139	0.354	0.018	0.672

Notes: Pilot indicates the group of each reform province. Non Pilot indicates the group of each non-reform province. *Excellent Audits* denotes the number of excellent city audit projects as appraised by the National Audit Office. T-statistics are calculated based on a regression of each outcome, clustering at the province level.