The dark side of audit market competition☆

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ABSTRACT

This paper examines the relation between audit market competition and audit quality. We use the staggered introduction of bullet trains in different Chinese cities as shocks to travel time between audit clients and prospective audit firms, which increases the threat of competition for incumbent audit firms. The inception of bullet train connectivity leads to a 4.5 percentage point (pp) increase in the probability of GAAP violations and a 1.7 pp decrease in the probability of modified audit opinions for clients headquartered in connected cities. Bullet train connectivity is also followed by a 1.6 pp decrease in income-decreasing audit adjustments but no change in income-increasing audit adjustments. The negative relation between bullet train connectivity and audit quality is 1) stronger when bullet trains put greater competitive pressure on incumbent auditors and 2) weaker when clients demand high audit quality. Our paper provides plausibly causal evidence that competition lowers audit quality.

1. Introduction

Understanding how audit market competition affects audit quality is of significant interest to academics, practitioners, and regulators, with important capital market and policy implications. From a theoretical perspective, it is unclear what effect audit market competition has on equilibrium audit quality. On the one hand, greater competition can foster audit process innovation and reduce auditor complacency, such that audit procedures are more rigorous and financial statements strictly adhere to GAAP (e.g., Government Accountability Office [GAO], 2008). Further, to the extent audit clients bear the costs of a low-quality audit (e.g., misstatement) and demand high-quality audits as a result, competition can strengthen auditors' reputational incentives to supply high audit quality. On the other hand, competition can lower audit quality if it leads auditors to focus on appeasing clients by reducing professional skepticism and allowing clients excessive financial reporting discretion. Prior studies examine the relation between audit market concentration, the typical proxy for competition, and audit quality and/or fees, but find mixed evidence. Thus, there is a lack of consensus among academics and practitioners on whether competition increases or decreases audit quality (see DeFond and Zhang (2014) for a review).

A primary reason for the mixed evidence on the relation between audit market competition and audit quality is that existing proxies for both constructs are subject to measurement error and have theoretically ambiguous interpretations. For example, several studies proxy for competition using market concentration (where more concentrated markets are interpreted as being less competitive) but the industrial organizations literature shows that the relation between concentration and competition is theoretically unclear (Stiglitz, 1987; Sutton, 1991; also see Dedman and Lennox, 2009). In addition, if companies select audit firms based on the audit quality they supply then audit market concentration/competition could reflect client preferences and characteristics, raising endogeneity concerns in the absence of exogenous variation in audit market concentration/competition (Causholli et al., 2010; Ferguson et al., 2018). Finally, several studies proxy for audit quality using discretionary or abnormal accruals, calculated in a variety of ways. Recent research finds that these proxies are subject to significant measurement error and inferential problems, especially when such proxies are used as dependent variables (Chen et al., 2018).

In this paper, we examine whether audit market competition improves or worsens equilibrium audit quality using plausibly exogenous changes in audit market competitiveness. We use the staggered introduction of bullet trains across prefectural cities in China between 2008 and 2017 as shocks to travel time between the companies located in a city and the offices of both their incumbent audit firms and competing audit firms. Our analyses focus on companies headquartered in prefectural cities because these are smaller cities that are home to few audit firm offices, and have low audit market competition. By its very nature, a significant portion of the work that goes into the production of an audit is performed at the audit client's office, typically the company's headquarters (PCAOB Release No. 2010-004).[1] As a result, the time auditors spend traveling to and from the client's office directly affects the cost of conducting an audit. Since audit firms typically do not have local offices in most prefectural cities in China, companies headquartered in these cities had little choice with respect to auditor selection. The introduction of bullet trains put companies headquartered in the average prefectural city within a two-hour train ride of five provincial cities, and (as a result) most of the forty audit firms authorized to audit listed companies in China. We argue that the introduction of a bullet train in a prefectural city provides companies located in the city with a significantly larger pool of potential audit firms to choose from. Thus, incumbent auditors are likely to face a significantly greater threat of competition from audit firms located in nearby cities once their client's city is connected to the bullet train network.

The Chinese setting not only provides plausibly exogenous variation in audit market competitiveness, but also allows us to measure audit quality using proxies relatively less susceptible to measurement error (DeFond et al., 2020; Lennox and Wu, 2022). Specifically, we proxy for audit quality using 1) the likelihood that a company's financial statements violate GAAP, 2) the likelihood that an auditor issues a modified audit opinion (MAO), and 3) adjustments made to pre-audit earnings during year-end audits, which is based on confidential data obtained from the Chinese Ministry of Finance (MOF). The ability to access audit adjustment data is especially useful because it helps us isolate the effect of an audit on earnings, controlling for all time-varying and time-invariant factors that affect audited earnings via their effect on pre-audit earnings.

Using data from 2007 to 2017 and a generalized difference-in-differences design, we find that the introduction of bullet trains leads to 1) a 4.5 percentage point (pp) increase in the probability that a company headquartered in that city violates GAAP, 2) a 1.7 pp decrease in the probability that a company headquartered in that city receives a MAO, and 3) a 1.6 pp decrease in income-decreasing adjustments to pre-audit earnings (but no change in income-increasing adjustments; untabulated). These results are consistent with the hypothesis that competition increases auditors' focus on client retention such that auditors allow clients greater reporting discretion and reduce professional skepticism, lowering audit quality. We find no evidence of a pre-treatment trend in audit quality before the inception of bullet train operations in a city, supporting the validity of the parallel-trends assumption and a causal interpretation of relation.

We conduct two sets of cross-sectional analyses. First, we exploit cross-sectional variation in the impact of bullet train connectivity on changes in audit market competition using 1) the number of competing audit firms located within a 0.5−2.5 h bullet train ride from the city, 2) the frequency of bullet trains passing through the city in a day, and 3) the location of the incumbent auditor. We find that the negative relation between bullet train connectivity and audit quality is stronger when bullet trains impose greater competitive pressure on the incumbent audit firm.

Second, we examine how cross-sectional variation in clients' ex ante preference for audit quality affects the relation between bullet train connectivity and audit quality. The ex-ante pairing of clients and auditors is not random but rather the outcome of a two-sided matching game where clients select auditors based on audit quality supplied and fees charged (Johnson and Lys, 1990), and auditors select clients based on the audit risks they impose and the fees they are willing to pay (Shu, 2000; Johnstone and Bedard, 2004). By increasing audit market competition, bullet train connectivity increases the client's bargaining power relative to the auditor. We identify clients that are more likely to demand high audit quality using the clients' 1) pre-treatment auditor choice (big-10 versus non-big-10), 2) pre-treatment reporting transparency, and 3) pre-treatment analyst coverage. We find that the negative effect of competition on audit quality is weaker when clients demand high-quality audits ex ante.

There are at least two important concerns with our setting and identification approach. First, the decision to connect a city to the bullet train network could be correlated with city growth rates, political connections, etc. If increases in growth

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[1] We refer to firms performing audits as "auditors" or "audit firms" and their clients as "audit clients" or "companies."

rates adversely affect audit quality and the introduction of a bullet train route is correlated with changes in growth rates, then our inferences could be incorrect. We conduct an instrumental-variable test and a placebo test to mitigate the concern that factors correlated with the introduction of bullet trains in a city confound our results.[2] Specifically, we instrument for whether/when a city is connected to the bullet train network using the slope or elevation of the city. Prior research finds that sloped terrains significantly increase engineering difficulties and construction costs, increasing the time taken to connect such cities to the bullet train network (Saiz, 2010; Dong et al., 2020). We use the average slope within each city's administration area as an instrument for when a city is connected to the bullet train network, and show that our inferences are unchanged.

In our placebo test, we examine whether the introduction of a bullet train affects audit quality for companies located in provincial cities. Since provincial cities are home to several audit firm offices, bullet train connectivity of these cities does not provide companies located in these cities with many additional audit firms to choose from. As a result, the introduction of a bullet train route in a provincial city is less likely to increase audit market competition, and thus we predict no changes in audit quality. However, if the introduction of a bullet train route captures an omitted variable that affects our audit quality proxies (e.g., changes in city growth rates), then we should continue to find that new bullet train routes in provincial cities affect our audit quality proxies. Consistent with our prediction, we find that new bullet train routes in provincial cities are not associated with audit quality changes for companies located in these cities.

A second threat to our inference is that bullet train connectivity could affect not only the behavior of auditors but also that of audit clients and their stakeholders. For example, bullet train connectivity could reduce information acquisition costs for investors, analysts, and regulators located in nearby cities, thereby increasing their ability to monitor companies located in prefectural cities. Such increases in monitoring by a company's stakeholders could confound the interpretation of our results. We conduct two tests to address this concern. First, we test and find that bullet train connectivity is not associated with changes in the quality of *pre-audit* earnings (we back out pre-audit earnings using our data on audit adjustments). Second, we test and find that bullet train connectivity is not associated with changes in the probability that a company receives a sanction for its non-GAAP disclosures (that are outside an auditor's purview). These results suggest that the documented association between bullet train connectivity and audit quality is likely due to changes in auditor behavior rather than that of the company's other stakeholders. We conduct several additional analyses and robustness tests to corroborate our inference (see section 7).

This paper contributes to the literature by using a plausibly exogenous shock to audit market competition that helps us test the causal relation between competition and audit quality. Academic and policy interest in the relation between competition and audit quality dates back many years (see section 2 for a detailed discussion of the related literature). However, previous attempts to examine this relation typically require interpreting concentration ratios as proxies for competition, even though several studies cast doubt on the relation between market concentration and competition. The benefit of our experimental design is that it not only side-steps the need to explicitly measure audit market competition, but also helps us address reverse causality and other endogeneity concerns orthogonal to the measurement of competition. For example, a negative relation between competition and audit quality could arise because 1) a subset of auditors supply higher quality audits than others, which pushes lower quality auditors out of the market and reduces audit market competition; or because 2) competition impairs auditor independence and thus leads to lower audit quality (as we propose). To our knowledge, the empirical methods employed in prior studies do not mitigate such concerns (see Causholli et al. (2010) and Ferguson et al. (2018) for discussions of endogeneity issues).

Before proceeding, we caveat that our inferences are based on analyses of companies and auditors in China, where the regulatory and institutional environment is weaker than that in the U.S. Thus, the results of this paper might not generalize to other settings. Regardless, the evidence in this paper is important given the increasing role of China in the global economy (see Lennox and Wu (2022) for a discussion of the importance of China-related accounting research).

2. Prior research and hypothesis development

Over the past few decades, there has been a rise in audit market concentration across the world. The increase in audit market concentration has raised the concern of regulators in several countries about whether the audit market is sufficiently competitive to incentivize auditors to supply high-quality audits (e.g., GAO, 2003, 2008; The American Assembly, 2005; Oxera, 2006, 2007; U.S. Department of Treasury, 2008; U.K. House of Lords, 2010; European Commission, 2011). Regulators' concerns about competition are mostly based on the idea that companies, especially larger ones, have a very small set of audit firms that they can choose from. The lack of choice gives audit firms significant market power. However, economic theory shows that lesser choice does not necessarily imply less competition (Stiglitz, 1987; Sutton, 1991). For example, Sutton (1991) shows that competition can lead to greater market concentration if competition forces inefficient firms to exit the market, leaving fewer but more efficient firms in the market.

Even putting aside the arguments made by regulators and the ambiguity around the interpretation of market concentration, the theoretical relation between competition and audit quality is ex ante unclear. Clients are not homogenous in their

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[2] In our main tests, we control for province × year and industry × year fixed effects to mitigate such concerns. Our cross-sectional tests and the absence of a pre-treatment trend in audit quality also help address these concerns.

demand for audit quality, and audit firms are not homogenous in their supply of audit quality (Johnson and Lys, 1990; Wang et al., 2008; Cook et al., 2020). Rather, companies and audit firms match to each other based on their preferences for audit quality and audit fees. Changes in audit market competition lead to changes in the relative bargaining power of companies and auditors such that equilibrium audit quality can increase or decrease based on the preference of the party with greater bargaining power.

On the one hand, competition can incentivize auditors to improve audit quality if a company's stakeholders demand high audit quality. Since companies, investors, and managers bear part of the cost of a low quality audit (Palmrose et al., 2004), and enjoy the capital market benefits associated with high audit quality (Blackwell et al., 1998; Minnis, 2011; Kausar et al., 2016; Shroff, 2020), they have market-based incentives to select higher quality auditors. Consistent with companies preferring high audit quality, prior research finds that an increase (decrease) in an auditor's reputation for supplying high audit quality leads to an increase (decrease) in their market share (Skinner and Srinivasan, 2012; Aobdia and Shroff, 2017). Thus, it is plausible that an increase in audit market competition increases auditors' incentives to improve audit quality.

Further, competition can strengthen auditors' incentives to innovate and improve efficiency. In competitive markets, audit firms will be forced to find ways to differentiate themselves from competitors, leading to innovation and greater efficiency (Shleifer and Vishny, 1997). In the mid-2000s, audit process innovation came in the form of employing paperless workflows and technology (CCH, 2008). Today, audit process innovation entails using technology to dig deeper into company data and reveal more about the company's risks, financial reporting controls and operating environment (Forbes, 2018). Absent competition, incentives to engage in costly innovation activity are diminished, which can lower (or not improve) audit quality.

On the other hand, greater competition can lead to a decrease in audit quality if clients use their increased bargaining power to demand greater financial reporting discretion. Specifically, competition can lead auditors to focus their effort on keeping clients happy and retaining them while decreasing any conflict that might arise when auditors express professional skepticism towards company-prepared financial statements. Disagreements over accrual accounting estimates and asset/liability values are fairly typical in the auditing process, and can create conflict between auditors and their clients (Hanlon and Shroff, 2022). If competition gives clients access to a larger pool of audit firms to select from, auditors facing an increased risk of client turnover might respond by allowing clients greater latitude with respect to their accrual estimates in an effort to reduce auditor-client tension. Since auditing has attributes of a credence good, where it is hard to evaluate audit quality not just for investors but also for boards of directors and company management, a reduction in audit quality and auditor professional skepticism could go unobserved by the company stakeholders, barring a restatement or fraud. Thus, it is plausible that greater audit market competition leads to a decrease in audit quality.

Prior research examines the relation between competition and audit quality but finds mixed evidence. Boone et al. (2012) find that greater audit market concentration is associated with lower audit quality (i.e., higher probability of meeting or beating earnings estimates). In contrast, Kallapur et al. (2010) find that higher audit market concentration is associated with higher audit quality (measured using discretionary accruals and the Dechow and Dichev (2002) accruals quality proxy). Newton et al. (2013, 2016) find that higher audit market concentration is associated with a lower likelihood of restatements and a lower incidence of internal control opinion shopping. Using a cross-country setting, Francis et al. (2013) find that in countries where there is greater market concentration within the Big-4 group (i.e., unequal market shares within the Big-4), clients exhibit lower audit quality, measured using total accruals, abnormal accruals, loss avoidance, and timely loss recognition. However, in countries where the Big-4 group dominates and has greater aggregate market share relative to the non-Big-4 group, such Big-4 dominance is associated with higher audit quality. Huang et al. (2016) find that, in the Chinese setting, audit market concentration improves audit quality indirectly through increased audit fees, and this positive indirect effect offsets the negative direct effect of concentration on audit quality.

Prior literature typically requires readers to interpret high market concentration as lower competition. However, as discussed earlier, higher concentration could imply higher or lower competition, making it hard to interpret the evidence in prior research (and the authors of prior studies are careful to state that higher concentration does not imply less competition). In fact, Ferguson et al. (2018) find that the primary driver of audit market concentration is the growth of the largest public companies and the associated need for audit firm scale. They go on to show that the rate of audit switching and the extent of fee discounting increase over time, and interpret their evidence as suggesting that the audit market remains competitive in spite of greater concentration.

Most recently, a working paper by Ciconte and Kitto (2020) measures competition with the persistence of abnormal profits in an audit market using confidential PCAOB data. They find evidence suggesting that competition can harm audit quality. However, their paper does not address endogeneity concerns due to reverse causality (i.e., the possibility that audit quality affects competition). Overall, the collective evidence on whether competition improves or worsens audit quality remains unclear. Thus, we examine the null hypothesis that audit market competition is unrelated to audit quality.

H: Changes in audit market competition are unrelated to changes in equilibrium audit quality.

3. The Chinese bullet train setting

3.1. Background on bullet trains and the competing means of transportation

Bullet trains are railway lines running at an average speed of 250 km per hour (kmph) or more, or passenger-dedicated intercity lines running at an average speed of 200 kmph or more. In contrast, the average speed of conventional trains in China was below 60 kmph in the 1990s. While conventional train speeds had been raised several times over the years, the highest speed did not exceed 150 kmph (Zheng and Kahn, 2013). In 2004, the Chinese State Council approved the Mid-to-Long Term Railway Network Plan and then updated it in 2008, laying out the high-speed railway development plan through 2020. As of the end of 2017, there were 95 bullet train lines in operation in China, with a total mileage of 27,322 km. The bullet train network dramatically increased the ease with which people could commute across cities, especially from smaller prefectural cities to megacities (Dong et al., 2020).

To understand whether the bullet train setting is suitable to test our research question, we first provide insight into how it affects the way people travel. A World Bank report by Amos et al. (2010) suggests that bullet trains deliver a competitive advantage over airlines for journeys of less than (approximately) 500 miles, particularly between city pairs where airports are located far from city centers. Roads are considered more suitable than trains for journeys less than 100 miles. Thus, bullet trains are expected to deliver the most benefit relative to alternative transportation forms for journeys between 100 and 500 miles. As such, there are many pairs of cities (e.g., Beijing and Shijiazhuang, Shanghai and Hefei) that are too far apart to drive and too close to fly.

Survey evidence supports the above argument. Specifically, Jianbin (2011), Wu et al. (2013), and Ollivier et al. (2014) conduct passenger surveys and interviews, and find that bullet trains mainly compete with conventional railways for short-haul trips and with air travel for longer trips. Survey evidence also suggests that a large fraction of the bullet train passengers travel for business. In addition, bullet trains reduce travel uncertainty, improve connections to inner-city transportation systems, and allow passengers to use their mobile phones, thereby reducing the opportunity cost of the time spent commuting (Chen et al., 2022). Data obtained from the Railway yearbook and the Ministry of Transport's annual report indicate that bullet trains are estimated to have transported two billion (670 million) passengers in 2018 (2013). For comparison, airlines are estimated to have transported 612 (354) million passengers in 2018 (2013). These data suggest that bullet trains have had a marked effect on the way in which people commute between cities.

3.2. Bullet train connectivity and audit market competition

Our premise is that bullet train connectivity increases audit market competition in connected cities. Specifically, bullet trains make it economically feasible for a larger pool of audit firms to compete for the audit business of clients located in cities with few audit firms. Since labor is one of the primary costs of audit production and travel time to and from a client's office increases this cost, bullet trains can significantly lower the cost of conducting an out-of-town audit. As a result, when a city becomes connected to the bullet train network, incumbent auditors of companies located in the city are likely to face stiffer competition from audit firms that do not have a local presence in the city but are located within a 0.5−2.5-h bullet train ride from (or 100−500 mile radius of) the company − i.e., distances that are too far to drive and too close to fly.

Above, we assume that an audit requires audit firm personnel to be physically present in their clients' offices for a significant portion of the audit. Performing an audit requires auditors to access their clients' accounting records, such as invoices, contracts, general and subsidiary ledgers, journal entries, adjustments to the financial statements that are not reflected in formal journal entries, etc. In addition, audit procedures include physical inspections of records and assets, and observation of activities by auditors (e.g., inventory counting by the company's personnel). In fact, audit firm personnel are typically provided working space in their client's office during the course of an audit.[3] Thus, we believe the assumption that audits are typically done in-person is justified and descriptive of practice, especially pre-COVID.

We also assume that very few audit firms have offices in smaller, prefecture-level cities, constraining the auditor choice for companies located in these cities. To support this assumption, Table 1, Panel A provides descriptive data about the number of audit offices located in the different types of Chinese cities. Mainland China is composed of 31 provinces and 338 cities (as of 2018). Each province has a capital city, which is the economic and transportation center of the province. In addition to the 31 capital cities, five cities (Dalian, Ningbo, Qingdao, Shenzhen, and Xiamen) are considered special economic zones and exhibit high economic development relative to other cities. The remaining 302 cities in each province are prefecture-level cities. We refer to the 31 capital cities and the 5 special economic zones as provincial cities.[4] Most audit firms establish branches in one or more of the provincial cities because of the high density of clients in these cities. Further, the largest customers of most audit firms are typically state-owned enterprises and building good relationships with provincial governments helps audit firms acquire and retain such clients. Proximity to the capital city helps build relationships with provincial governments. As

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[3] We recognize that this assumption might be weakened post-COVID due to improvements work-from-home technologies. However, our analyses pre-date COVID. In addition, our interest lies in examining the effect of competition on audit quality, and changes in travel time is simply a proxy for changes in audit market competition. It is not clear that COVID affects the economic incentives that shape the relation between competition and audit quality.

[4] Beijing, Shanghai, Tianjin, and Chongqing are municipalities that are of equal rank as provinces. We refer to these four cities as provincial cities.

Table 1
City-level descriptive statistics.

Panel A: Distribution of companies and audit firms across provincial and prefectural cities in 2018

Variable (as of Dec. 2018)	City	Publicly listed companies		Audit firms	
	Aggregate	Aggregate	Average	Aggregate	Average
Provincial cities with listed companies & audit offices	36	2072	57.6	649	18.0
Provincial cities with listed companies & no audit offices	0	0	0.0	0	0.0
Provincial cities without listed companies & audit offices	0	0	0.0	0	0.0
All provincial cities	**36**	**2072**	**57.6**	**649**	**18.0**
Prefectural cities with listed companies & audit offices	51	757	14.8	113	2.2
Prefectural cities with listed companies & no audit offices	186	666	3.6	0	0.0
Prefectural cities without listed companies & audit offices	65	0	0.0	0	0.0
All prefectural cities	**302**	**1423**	**4.7**	**113**	**0.4**

Panel B: Distribution of audit firms located in or nearby prefectural cities in 2018

Variables	Mean	SD	P25	P50	P75	N
# AUDIT FIRMS IN PREFECTURAL CITIES	0.5	1.6	0.0	0.0	0.0	237
# AUDIT FIRMS WITHIN 100 MILES OF PREFECTURAL CITIES	10.4	13.0	0.0	2.0	24.0	237
# AUDIT FIRMS WITHIN 500 MILES OF PREFECTURAL CITIES	33.1	10.3	35.0	38.0	39.0	237

Notes: This table presents several descriptive statistics related to the number of audit firms in or near each provincial and prefectural city in China. We classify the following 36 cities having provincial status: Beijing, Changchun, Changsha, Chengdu, Chongqing, Dalian, Fuzhou, Guangzhou, Guiyang, Ha'erbin, Haikou, Hangzhou, Hefei, Huhehaote, Jinan, Kunming, Lanzhou, Lasa, Nanchang, Nanjing, Nanning, Ningbo, Qingdao, Shanghai, Shenyang, Shenzhen, Shijiazhuang, Taiyuan, Tianjin, Wuhan, Wulumuqi, Xiamen, Xi'an, Xining, Yinchuan, and Zhengzhou. Panel A presents data on all provincial and prefectural cities in China as of 2018, and Panel B presents data (as of 2018) for the 237 prefectural cities with at least one publicly listed company headquartered in the city.

such, audit markets in provincial cities are highly competitive and thus we exclude companies headquartered in these cities from our analyses (except for the placebo test in section 7).

Of the 302 prefectural cities in China, 65 do not have any publicly listed company headquarters and thus are excluded from our analyses. Our analyses focus on 237 prefectural cities that are home to at least one publicly listed company. Table 1, Panel B shows that the average (median) prefectural city in our sample is home to 0.5 (zero) audit firms. Further, the average (median) city is within a 100-mile radius of ten (two) audit firms but within a 500-mile radius of 33 (38) of the 40 audit firms permitted to audit listed companies in China. These data suggest that companies headquartered in prefectural cities have a limited pool of audit firms to choose from if their choice is restricted to audit firms located in the same city or those within a 100-mile radius. However, companies headquartered in prefectural cities are within a 500-mile radius of most of the 40 firms permitted to audit Chinese listed companies. Insofar as bullet train connectivity reduces a company's cost of hiring auditors located in a different city, such connectivity is likely to increase the threat of competition for incumbent auditors.

To support our premise that bullet train connectivity increases audit market competition, we examine how bullet trains affect audit market characteristics at the city-level. If bullet train connectivity makes it cost-effective to hire auditors located in nearby cities (e.g., within a 100-to-500 mile radius of the company), we predict that the inception of bullet train connectivity will: 1) increase the number of unique audit firms serving clients in connected cities, 2) reduce audit market concentration in connected cities, and 3) increase the distance between companies and their auditors in connected cities. We estimate regressions of the following form to test our prediction:

$$y_{c,t} = \beta_1\ BULLET\ TRAIN\ OPEN_{c,t} + \alpha_c + \alpha_p \times \alpha_t + \gamma'X + \varepsilon_{i,t} \tag{1}$$

where *c*, *t*, and *p* index cities, years, and provinces, respectively. *BULLET TRAIN OPEN* is an indicator variable that equals one for city-years connected to the bullet train network. *X* is a vector of time-varying city-level control variables. $y_{c,t}$ is one of three variables: 1) the natural logarithm of the number of unique audit firms that have at least one client in the city (*LN(#AUDITORS)*), 2) the Herfindahl-Hirschman index (*HHI*) for the audit market in the city, or 3) the natural logarithm of the median change in distance between clients and auditors in a city (*LN(ΔDISTANCE)*).

Table 2, Panels A and B present the sample selection procedure and descriptive statistics for the variables used in our analysis, respectively. Table 2, Panel C presents the number of prefectural cities connected to the bullet train each year during our sample period (2007−2017), the number of unique audit firms with a client in the average city each year, and the audit market HHI in the average city each year. Panel C shows that 144 out of 237 prefectural cities were connected to the bullet train network between 2008 and 2017; none were connected until 2007 and 93 were yet to be connected as of 2017.

Table 2, Panel D presents the regression results and shows that the coefficient for *BULLET TRAIN OPEN* is positive and significant when the dependent variable is *LN(#AUDITORS)* (coef. = 0.09; t-stat. = 2.15), negative and significant when the dependent variable is *HHI* (coef. = -0.038; t-stat. = −1.85), and positive and significant when the dependent variable is *LN(ΔDISTANCE)* (coef. = 0.339; t-stat. = 3.13). These results suggest that bullet train connectivity leads to a 9% increase in the number of unique audit firms with a client in the city, a 3.8% reduction in audit market *HHI*, and a 33.9% increase in the median

Table 2
Effect of bullet train connectivity on audit markets characteristics in prefectural cities.

Panel A: Sample selection

No.	Sample composition	City-year level
(1)	City-year observations from 2007 to 2017	2820
(2)	*Less:* Provincial cities	(396)
(3)	*Less:* Observations missing data to construct control variables	(476)
	Final sample of city-years available for analyses	1948

Panel B: Descriptive statistics

Variables	Mean	SD	P25	P50	P75	N
#AUDITORS	2.620	2.080	1.000	2.000	3.000	1948
LN(#AUDITORS)	0.710	0.690	0.000	0.690	1.100	1948
HHI	0.630	0.320	0.330	0.500	1.000	1948
ΔDISTANCE	12.950	45.950	0.000	0.000	0.000	1948
LN(ΔDISTANCE)	0.560	1.500	0.000	0.000	0.000	1948
CITY GDP GR*	0.110	0.050	0.080	0.100	0.140	1948
CITY POPULATION GR*	0.060	0.050	0.020	0.050	0.090	1948
CITY WAGE GR*	0.130	0.070	0.090	0.120	0.160	1948

Panel C: Bullet train connectivity and city-level audit market characteristics by year

Year	Prefectural City		
	# cities opening first bullet train	Ave. number of audit firms with at least one client in city	Ave. audit market HHI (city-level)
2007	0	1.86	0.734
2008	4	1.97	0.714
2009	13	1.84	0.732
2010	12	2.15	0.691
2011	14	2.32	0.671
2012	16	2.59	0.635
2013	17	2.67	0.633
2014	31	2.74	0.624
2015	22	2.93	0.607
2016	10	3.08	0.593
2017	5	3.20	0.566
Sum	144	Not meaningful	Not meaningful

Panel D: Regression analyses of bullet train connectivity and city-level audit market characteristics

Dependent Variable:	LN(#AUDITORS)	HHI	LN(ΔDISTANCE)
	(1)	(2)	(3)
	Coefficient	Coefficient	Coefficient
	t-Statistic	t-Statistic	t-Statistic
BULLET TRAIN OPEN	0.090**	−0.038*	0.339***
	(2.15)	(−1.85)	(3.13)
CITY GDP GR	−0.248	0.168	−0.611
	(−0.90)	(1.22)	(−0.53)
CITY POPULATION GR	0.009	0.030	−0.963
	(0.03)	(0.23)	(−0.74)
CITY WAGE GR	0.146	−0.072	0.249
	(1.44)	(−1.27)	(0.49)
City Indicators	Included	Included	Included
Province × Year Indicators	Included	Included	Included
R-Squared	90.6%	87.2%	41.2%
No. of Observations	1948	1948	1948

Panel E: Dynamic analyses of bullet train connectivity and city-level audit market characteristics

Dependent Variable:	LN(#AUDITORS)	HHI	LN(ΔDISTANCE)
	(1)	(2)	(3)
	Coefficient	Coefficient	Coefficient
	t-Statistic	t-Statistic	t-Statistic
BULLET TRAIN OPEN [-4]	−0.102*	0.034	0.231
	(−1.80)	(1.17)	(1.24)
BULLET TRAIN OPEN [-3]	0.008	−0.027	0.096
	(0.19)	(−1.24)	(0.49)
BULLET TRAIN OPEN [-2]	0.013	−0.026	−0.206

(*continued on next page*)

Table 2 (*continued*)

Panel E: Dynamic analyses of bullet train connectivity and city-level audit market characteristics

Dependent Variable:	LN(#AUDITORS)	HHI	LN(ΔDISTANCE)
	(1)	(2)	(3)
	Coefficient	Coefficient	Coefficient
	t-Statistic	t-Statistic	t-Statistic
	(0.38)	(−1.39)	(−1.36)
BULLET TRAIN OPEN [−1]	−0.005	−0.008	0.017
	(−0.16)	(−0.49)	(0.11)
BULLET TRAIN OPEN [1]	0.052*	−0.042***	0.355**
	(1.68)	(−2.72)	(2.23)
BULLET TRAIN OPEN [2]	0.095**	−0.061***	0.172
	(2.30)	(−2.93)	(1.23)
BULLET TRAIN OPEN [3]	0.113**	−0.041*	0.404**
	(2.36)	(−1.76)	(2.43)
BULLET TRAIN OPEN [4+]	0.156**	−0.046	0.433***
	(2.50)	(−1.50)	(2.73)
CITY GDP GR	−0.236	0.160	−0.697
	(−0.86)	(1.18)	(−0.62)
CITY POPULATION GR	0.043	0.030	−0.925
	(0.16)	(0.22)	(−0.71)
CITY WAGE GR	0.136	−0.071	0.227
	(1.31)	(−1.22)	(0.44)
City Indicators	Included	Included	Included
Province × Year Indicators	Included	Included	Included
R-Squared	90.7%	87.4%	41.5%
No. of Observations	1948	1948	1948

Notes: Panel A in this table presents the sample selection procedure and Panel B presents descriptive statistics for the variables used in our analyses of bullet train openings and city-level audit market characteristics. Panel C presents data on the number of prefectural cities that open their first bullet train line each year as well as the average number of audit firms that have at least one client in a city, and the city-level audit market HHI. Panels D and E present results from regressions of city-level audit market characteristics on bullet train openings. The regressions in Panel D estimate a single post-treatment indicator and the regressions in Panel E estimate separate coefficients for each of the four years immediately before and after the opening of the first bullet train in a city. See the Variable Appendix for detailed variable definitions. The standard errors are clustered at the city-level. In Panel B, * indicates that the variable is standardized to have a mean of zero and a standard deviation of one in our regression analyses (but is reported before standardization in Panel B). In Panels D and E, ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

change in distance between clients and their auditors. The average *ΔDISTANCE* conditional on having auditor turnover is 105.6 miles, suggesting that bullet train connectivity increases the distance between auditors and their clients by approximately 36 miles. Table 2, Panel E and Fig. 1 show that there is no pre-treatment trend in the dependent variables, suggesting that changes in the audit market characteristics occur in response to a city's bullet train connectivity. These initial regressions and descriptive information support our use of the Chinese bullet train setting as a means to test the effect of changes in audit market competition on audit quality.

4. Research design

We estimate the regression below to test whether bullet trains affect audit quality:

$$y_{i,t} = \beta_1 \, BULLET\ TRAIN\ OPEN_{c,t} + \alpha_i + \alpha_{af} + \alpha_{ind} \times \alpha_t + \alpha_p \times \alpha_t + \gamma'X + \varepsilon_{i,t} \qquad (2)$$

where i, t, ind, af, c, and p indexes companies, years, industries, audit firms, cities, and provinces, respectively. *BULLET TRAIN OPEN* is an indicator variable that equals one for company-years headquartered in cities connected to the bullet train network, following the commencement of the city's first bullet train route. This variable is measured at the city-year level and thus, we cluster standard errors by city. α_i, α_t, α_{ind}, α_{af}, and α_p are company, year, industry (based on the China Securities Regulatory Commission (CSRC) industry classification), audit firm, and province indicator variables. X is a vector of controls.

$y_{i,t}$ is one of three proxies for audit quality. First, *GAAP VIOLATION* is an indicator variable that equals one for company-years in which a GAAP violation is committed. We focus on the year in which a GAAP violation is committed rather than detected (or announced) because we predict that audit market competition changes auditor behavior, which should affect the occurrence of a GAAP violation rather than its detection or announcement. Following Lennox et al. (2018), we manually classify sanctions issued by the CSRC, local regulatory agencies, and the Shanghai/Shenzhen stock exchanges as a GAAP

Panel A: Number of unique auditors serving clients in prefectural cities
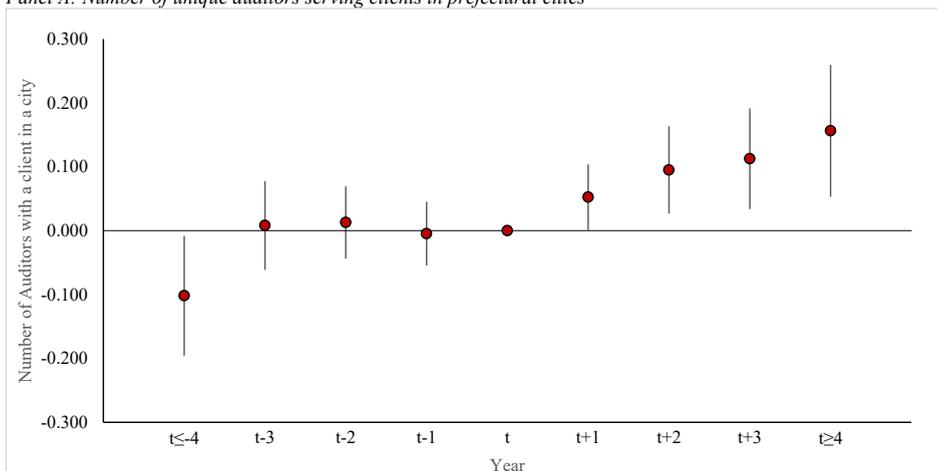
Panel B: Audit market concentration in prefectural cities
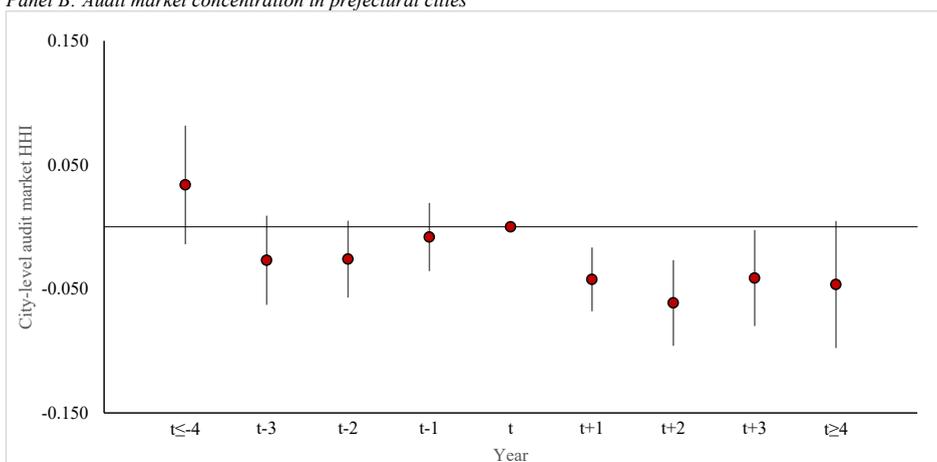
Panel C: Change in median distance between audit firms and clients in prefectural cities
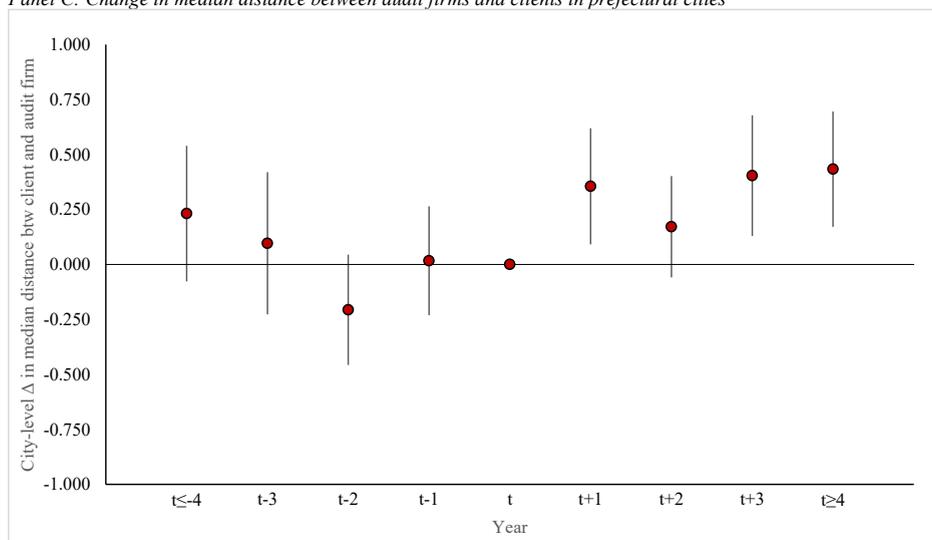
Fig. 1. Effect of bullet train connectivity on city-level audit market characteristics. *Notes:* In the figures above, the x-axis represents time relative to the year in which a prefectural city becomes connected to the bullet train network, and the y-axis in panel A (B, C) represents the number of unique auditors that have at least one client in a city (city-level audit market HHI, city-level median change in distance between audit firms and their clients). The figure plots the two-tailed 90% confidence interval around each point estimate of the relation between bullet train openings and city audit market characteristics.

violation if the sanction is related to the misstatements of assets, liabilities, owners' equity, cash flows, revenues or expenses, as well as the provision of inadequate or misleading disclosures in financial statement notes.[5]

Second, *MODIFIED OPINION* is an indicator variable that equals one for company-years in which a client receives a modified audit opinion (MAO). MAOs include instances in which an auditor issues an adverse opinion, a qualified opinion, an un-qualified opinion but with explanatory notes, or disclaimers. Chen et al. (2000) find that the issuance of a MAO elicits negative investor reactions. Further, the CSRC requires companies to explain the reasons why they received a MAO in their annual reports. Auditors that exert less effort and/or are more focused on client retention are less likely to issue a MAO, given that such opinions are likely to generate conflict between the auditor and its client (DeFond et al., 1999).[6]

INCOME DECREASE AUDIT ADJ is the unsigned difference between pre-audit earnings and audited earnings conditional on audited earnings being less than pre-audit earnings. This variable captures income-decreasing adjustments to year-end earnings during the company's financial statement audit. Prior research finds that audit adjustments are associated with audit quality improvements (Lennox et al., 2014, 2016).[7] Audit adjustments are particularly helpful in isolating the effect of an audit on earnings outcomes because audit adjustments (i.e., audited earnings minus pre-audit earnings) difference out all time-varying and time-invariant factors (e.g., performance, growth, etc.) that simultaneously affect pre-audit earnings and audited earnings but are unrelated to work performed by the auditor.

In addition to the fixed effects, we include several control variables in our regressions. When the dependent variable is *GAAP VIOLATION*, we follow Lennox et al. (2013) and control for auditor type (Big 10 vs. non-Big 10) and tenure, company size, age and profitability, and indicator variables for whether the company has a merger or acquisition and issues debt or equity. Following Huang et al. (2015), we also control for leverage and working capital intensity. Some prior research on restatements includes firm-specific controls for corporate governance structures, following which we control for CEO duality (i.e., an indicator for whether the CEO is also the chair of the board) and the percentage of independent directors on the board.

When the dependent variable is *MODIFIED OPINION*, besides controlling for company size, age, profitability, leverage, auditor type and tenure, we also control for the current ratio, accounts receivable, inventory, and stock performance because these characteristics may affect audit opinions (Guan et al., 2016). Following Huang et al. (2015), we also control for whether the company reports a loss and the importance of the client for the audit firm.

For *INCOME DECREASE AUDIT ADJ*, we follow Lennox et al. (2020) and control for pre-audit signed accruals because auditors are likely to require more audit adjustments when such accruals are larger, and internal control weaknesses because these can affect the incidence of misstatements in pre-audit financial statements. We include the above controls in addition to controlling for size, leverage, profitability, corporate governance characteristics, auditor type, auditor tenure, and client importance to the auditor.

We also include some common control variables across all the dependent variables we examine. Specifically, we follow Dong et al. (2020) and control for several city-level characteristics to mitigate concerns that the placement of bullet train lines is correlated with the economic development and growth of a city, and thus affects audit quality. We control for analyst following and institutional ownership because prior research finds that analysts and institutional investors serve a governance role and increase transparency.

A few important features of our research design are as follows. First, our regressions include company-, auditor-, province-year, and industry-year fixed effects. As a result, our main regressions estimate the effect of bullet train connectivity on audit quality using within-company *and* within-auditor variation in the timing of bullet train introductions in each city.[8] In other words, our research design benchmarks the change in audit quality of a company following the introduction of bullet trains in its city of headquarters to the change in audit quality of another company in the same province-year and industry-year whose city is 1) yet to be connected to the bullet train network or 2) was already connected to the bullet train network at an earlier date. As such, our fixed-effect structure mitigates the concern that time-invariant company or auditor attributes confound our results.

Second, we recognize that the placement of the bullet trains across Chinese cities is not random. However, the primary identification challenge in the context of our study is not *whether* a city is connected to the bullet train network, but rather what factors determine the *timing of its connection* because our source of identification is the relative timing of when a city is

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[5] We caveat that GAAP violations detected by regulators in China are likely to capture only a small fraction of the accounting irregularities among listed companies since enforcement is sporadic and uneven.

[6] In section 7.3., we address the possibility that auditors have a conservatism bias that leads them to issue more frequent MAOs prior to the inception of bullet train connectivity.

[7] A recent working paper by Zhang et al. (2020) suggests that companies audited by high-quality auditors supply higher quality financial statements anticipating a thorough audit, which results in such companies having few audit adjustments ex post. Thus, Zhang et al. (2020) argue that zero/small audit adjustments could be interpreted as 1) high audit quality if clients recognize that their auditor is strict and respond ex ante by preparing higher quality financial statements that require the auditor to make fewer audit adjustments ex post, or 2) low audit quality (which is our preferred interpretation based on prior research) if auditors are lenient and take the client-prepared financial statements as given. By examining multiple proxies for audit quality, our analyses can help tease out the alternative interpretations of audit adjustments. If bullet train connectivity simultaneously increases the probability of a GAAP violation and reduces audit adjustments, we can infer that lower audit adjustments imply the provision of lower audit quality. In untabulated analyses, we verify that GAAP violations are negatively correlated with the magnitude of income decreasing audit adjustments.

[8] If bullet train connectivity increases competition in a city, it is plausible that it also increases auditor turnover. Thus, by including auditor fixed effects in our regressions, we control for a portion of the hypothesized effect. In untabulated results, we verify that our results are similar without auditor fixed effects.

connected to the bullet train network. Prior research finds that several idiosyncratic factors influence the opening time of bullet train lines. For example, Lin (2017) shows that the construction work on 12 of the 45 existing railway lines began in 2005, following the passage of the Mid-to-Long Railway Plan in 2004. The timing of when each line opens was determined by the construction progress, which largely depended on engineering difficulties. In section 7.1., we use cross-city variation in engineering difficulties caused by city terrain as an instrument to mitigate endogeneity concerns.

Finally, our analyses focus on companies headquartered in prefectural cities. The fact that more than 75% of the prefectural cities in our sample have no audit firm office suggests that bullet train connectivity is likely to have a large impact on audit market competition in these cities.

5. Data and descriptive statistics

We use the China Railway Yearbook series to collect information related to the Chinese bullet train system, including information on construction starting dates, opening dates, route length, designed speeds, and ridership. We crosscheck the information on the stops along each existing line from the official railway service website (https://www.12306.cn/). We use the China Stock Market & Accounting Research (CSMAR) database and the CEIC China Premium Database as the primary data source for the company-level and city-level variables, respectively. However, the CSMAR database's coverage of sanctions noticeably drops after 2011, which is purported to be due to a change in the format of sanction announcements made by companies. Thus, we supplement the CSMAR data on sanctions with data from the CNRDS and WIND databases. Our sample period begins in 2007, when the current accounting standards were introduced, and ends in 2017, when the Railway Yearbook was last updated. We hand collect data on historical audit office locations from the industry management system of the CICPA and data on highway and air ridership from cities' Statistical Bureaus.

Table 3 presents our sample selection procedure. There are 26,404 company-year observations in the initial sample. We delete 15,658 observations related to companies that are headquartered in provincial cities. We then drop observations related to special treatment companies and those in the financial industry. Deleting observations missing the data necessary to construct control variables and auditor information leaves us with a final sample of 7675 company-year observations.

Chinese audit firms report the pre-audit and audited values of pre-tax earnings and total assets of their publicly listed clients to the Ministry of Finance (MOF). While these data are confidential, one of the co-authors of this study obtained authorization to access audit adjustment data for the years 2006–2015 for this study. Restricting the analyses of audit adjustments to the period 2007 to 2015 leaves us with a sample of 5667 observations for this analysis.

Table 4 presents the descriptive statistics for the variables used in our analyses. The table shows that 3.4% of the company-years in our sample have a GAAP violation while 9.9% of the company-years have a sanction related to non-GAAP disclosures. 2.7% of the company-years in our sample receive a modified audit opinion. Pre-audit earnings are adjusted downward by 4.4% on average, and downward adjustments are larger in magnitude and occur more frequently than upward adjustments (untabulated). In terms of financial performance, the average company has a 4.3% ROA, 20.5% sales growth and just over 9% of the company-years in the sample incur a loss.

6. Results

6.1. Main analyses

We first examine whether companies headquartered in prefectural cities observe a change in audit quality following the introduction of bullet trains in their cities. Table 5 presents the results. In Panel A, we find that the coefficient for *BULLET TRAIN OPEN* is positive and significant at the 5% level (coef. = 0.045; t-stat. = 2.40) in column 1, where we proxy for audit quality using *GAAP VIOLATION*. The coefficient estimate suggests that bullet train connectivity leads to a 4.5 percentage point (pp) increase in the probability of a GAAP violation. Column 2 repeats the above analyses using *MODIFIED OPINION* as the proxy for audit quality. We find that the coefficient for *BULLET TRAIN OPEN* is negative and significant at the 1% level (coef. = −0.017; t-stat. = −2.67), which suggests that the probability that a company receives a modified audit opinion reduces by 1.7 pp after

Table 3
Sample selection.

No.	Sample composition	Company-level
(1)	Company-year observations from 2007 to 2017	26,404
(2)	*Less:* Companies headquartered in provincial cities	(15,658)
(3)	*Less:* Companies in the financial industry or special treated	(324)
(4)	*Less:* Observations missing data to construct control variables	(1968)
(5)	*Less:* Observations missing auditor data	(779)
	Final sample of company-years available for analyses	7675

Notes: This table presents the sample selection procedure of our main analyses.

Table 4
Descriptive statistics.

Variables	Mean	SD	P25	P50	P75	N
GAAP VIOLATION	0.034	0.181	0.000	0.000	0.000	7675
NON-GAAP VIOLATION	0.099	0.298	0.000	0.000	0.000	7675
MODIFIED OPINION	0.027	0.161	0.000	0.000	0.000	7675
INCOME DECREASE AUDIT ADJ.	0.044	0.065	0.000	0.000	0.083	5667
BULLET TRAIN OPEN	0.488	0.500	0.000	0.000	1.000	7675
LN(ASSETS)*	21.929	1.152	21.113	21.797	22.564	7675
LEVERAGE*	0.432	0.209	0.268	0.417	0.587	7675
ROA*	0.043	0.060	0.013	0.038	0.070	7675
ANALYST FOLLOWING*	1.508	1.132	0.693	1.609	2.485	7675
%INSTITUTIONAL OWN*	0.140	0.112	0.048	0.115	0.208	7675
BIG10	0.447	0.497	0.000	0.000	1.000	7675
LN(TENURE)*	1.329	0.691	0.693	1.386	1.792	7675
CITY GDP GR*	0.100	0.039	0.076	0.091	0.121	7675
CITY GDP*	11.117	0.565	10.739	11.172	11.541	7675
CITY POPULATION GR*	0.050	0.047	0.019	0.047	0.074	7675
CITY POPULATION*	15.262	0.554	15.032	15.346	15.681	7675
CITY WAGE GR*	0.114	0.057	0.082	0.108	0.137	7675
CITY WAGE*	10.744	0.366	10.526	10.799	11.011	7675
AIR RIDERSHIP*	6.481	6.747	0.000	0.000	13.430	7675
HIGHWAY TRAFFIC*	9.209	0.994	8.532	9.204	9.896	7675
LN(AGE)*	2.626	0.393	2.398	2.708	2.890	7675
SALES GR*	0.205	0.500	−0.022	0.120	0.286	7675
M&A	0.376	0.484	0.000	0.000	1.000	7675
CAPITAL ISSUANCE	0.540	0.498	0.000	1.000	1.000	7675
WC ACCRUALS*	−0.006	0.099	−0.061	−0.009	0.045	7675
CEO DUALITY	0.241	0.428	0.000	0.000	0.000	7675
%INDEPENDENT DIR*	0.369	0.050	0.333	0.333	0.400	7675
LOSS	0.092	0.289	0.000	0.000	0.000	7675
CURRENT RATIO*	2.207	2.171	1.043	1.551	2.433	7675
ACC. RECEIVABLE*	0.106	0.092	0.031	0.086	0.154	7675
INVENTORY*	0.153	0.128	0.072	0.125	0.191	7675
RETURNS*	0.256	0.729	−0.219	0.054	0.513	7675
INFLUENCE*	0.184	0.281	0.021	0.056	0.186	7675
PRE-AUDIT ACCRUALS*	−0.009	0.078	−0.050	−0.009	0.031	5667
ICW	0.181	0.385	0.000	0.000	0.000	7675
# NEW COMPETITORS	0	0	0	0	1	7675
# TRAINS	0	0	0	0	1	7675
PROXIMATE INCUMBENT	0.573	0.495	0.000	1.000	1.000	7675
PRE-TREAT BIG10	0.416	0.493	0.000	0.000	1.000	7675
PRE-TREAT TRANSPARENCY	0.164	0.371	0.000	0.000	0.000	4871
PRE-TREAT ANALYSTS#	8	10	1	5	12	7675

Notes: This table presents the descriptive statistics for the variables used in our analyses. In the table above, * indicates that the variable is standardized to have a mean of zero and a standard deviation of one in our regression analyses (but is reported before standardization in the table above). # indicates the variable is transformed into an indicator variable that equals one (zero) when its value is above (below) median for regression analyses (here again, the variables are reported before transformation in the table above). See the Variable Appendix for detailed variable definitions.

the city is connected to the bullet train network. Column 3 shows that the coefficient for *BULLET TRAIN OPEN* is negative and significant at the 1% level (coef. = −0.016; t-stat. = −3.81) when the dependent variable is *INCOME DECREASE AUDIT ADJ*, suggesting that income-decreasing adjustments reduce by 1.6 pp after the client's city of headquarters is connected to the bullet train network. These results provide initial evidence that bullet train connectivity is associated with a reduction in audit quality for the companies headquartered in the city.

To mitigate endogeneity concerns, we investigate the trend in our audit quality proxies prior to the introduction of bullet trains. If our results are picking up a company-specific trend related to changing economic conditions, then such economic changes are likely to affect audit quality even before the opening of a bullet train line. However, if audit quality systematically declines only after the inception of a bullet train line, which is staggered in time, then it is less likely that a correlated omitted variable can explain the results.

Table 5, Panel B presents the results from repeating the analyses in Panel A after replacing *BULLET TRAIN OPEN* with eight *event-time* indicator variables. Each indicator variable equals one for the individual years in the eight-year period around the introduction of a bullet train in a city. Consistent with our expectation, we find that all pre-treatment indicator variables are insignificant for all three dependent variables we examine. Fig. 2 presents a graphical representation of the coefficients for all the indicator variables and their 90% confidence intervals. Panel A (B, C) in the figure corresponds to the regression results in

Table 5

Bullet train connectivity and audit quality.

Panel A: Static analyses

| Dependent Variable:	GAAP VIOLATION		MODIFIED OPINION		INCOME DECREASE AUDIT ADJ.	
	(1)		(2)		(3)	
	Coefficient	t-Statistic	Coefficient	t-Statistic	Coefficient	t-Statistic
BULLET TRAIN OPEN	0.045**	2.40	−0.017***	−2.67	−0.016***	−3.81
LN(ASSETS)	−0.009	−1.38	−0.065***	−6.00	−0.000	−0.09
LEVERAGE	0.026	1.19	0.314***	6.89	0.008	0.61
ROA	−0.008	−0.16	−0.246***	−4.39	−0.081***	−3.04
ANALYST FOLLOWING	0.000	0.13	0.002	0.72	0.003*	1.70
%INSTITUTIONAL OWN	−0.038	−1.14	0.028	0.83	0.027	1.39
BIG10	0.002	0.36	0.004	0.90	0.000	0.14
LN(TENURE)	−0.023**	−2.28	−0.008	−1.13	0.006**	2.21
CITY GDP GR	0.091	0.49	−0.058	−0.81	0.017	0.35
CITY GDP	−0.014	−0.78	−0.005	−0.31	0.007	0.71
CITY POPULATION GR	−0.165	−1.46	0.049	0.47	−0.032	−0.70
CITY POPULATION	−0.152**	−2.11	0.062	1.03	−0.002	−0.09
CITY WAGE GR	0.089	1.38	−0.062	−1.20	0.021	0.73
CITY WAGE	−0.002	−0.05	0.078**	2.06	−0.036*	−1.66
AIR RIDERSHIP	−0.002	−0.95	−0.003**	−2.02	−0.000	−0.67
HIGHWAY TRAFFIC	−0.004	−0.51	−0.004	−0.80	−0.005	−1.31
LN(AGE)	0.022	0.83	0.108***	4.08		
SALES GR	0.004	0.81				
M&A	0.010**	2.04				
CAPITAL ISSUANCE	0.001	0.20				
WC ACCRUALS	0.009	0.50				
CEO DUALITY	−0.007	−0.79			0.002	0.50
%INDEPENDENT DIR	−0.013	−0.20			−0.017	−0.52
LOSS			0.024**	2.15		
CURRENT RATIO			0.004***	2.93		
ACC. RECEIVABLE			−0.025	−0.40		
INVENTORY			−0.226***	−2.99		
RETURNS			−0.004	−1.01		
INFLUENCE			0.032*	1.91	−0.003	−0.42
PRE-AUDIT ACCRUALS					0.040**	2.29
ICW					−0.000	−0.04
Company Indicators	Included		Included		Included	
Audit Firm Indicators	Included		Included		Included	
Industry × Year Indicators	Included		Included		Included	
Province × Year Indicators	Included		Included		Included	
R-Squared	62.6%		55.3%		25.6%	
No. of Observations	7675		7675		5667	

Panel B: Dynamic analyses

Dependent Variable:	GAAP VIOLATION		MODIFIED OPINION		INCOME DECREASE AUDIT ADJ.	
	(1)		(2)		(3)	
	Coefficient	t-Statistic	Coefficient	t-Statistic	Coefficient	t-Statistic
BULLET TRAIN OPEN [-4]	0.022	1.45	−0.002	−0.12	−0.008	−1.02
BULLET TRAIN OPEN [-3]	0.018	1.07	0.005	0.47	−0.004	−0.59
BULLET TRAIN OPEN [-2]	0.008	0.77	−0.003	−0.28	−0.002	−0.27
BULLET TRAIN OPEN [-1]	0.003	0.44	0.010	1.19	0.000	0.02
BULLET TRAIN OPEN [1]	0.061***	2.76	−0.020***	−3.08	−0.016**	−2.44
BULLET TRAIN OPEN [2]	0.058**	2.38	−0.013	−1.60	−0.014***	−2.61
BULLET TRAIN OPEN [3]	0.018	1.12	−0.000	−0.01	−0.018***	−2.77
BULLET TRAIN OPEN [4+]	0.070**	2.03	−0.003	−0.23	−0.015*	−1.83
Control Variables	Included		Included		Included	
Company Indicators	Included		Included		Included	
Audit Firm Indicators	Included		Included		Included	
Industry × Year Indicators	Included		Included		Included	
Province × Year Indicators	Included		Included		Included	
R-Squared	62.9%		55.4%		25.6%	
No. of Observations	7675		7675		5667	

Notes: This table presents the results from regressions of audit quality proxies on an indicator variable for the introduction of bullet train connectivity in a city and control variables. See the Variable Appendix for detailed variable definitions. The regressions in Panel A estimate a single post-treatment indicator and the regressions in Panel B estimate separate coefficients for each of the four years immediately before and after the opening of the first bullet train in a city. The standard errors are clustered at the city-level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Panel A: Measuring audit quality using GAAP violations
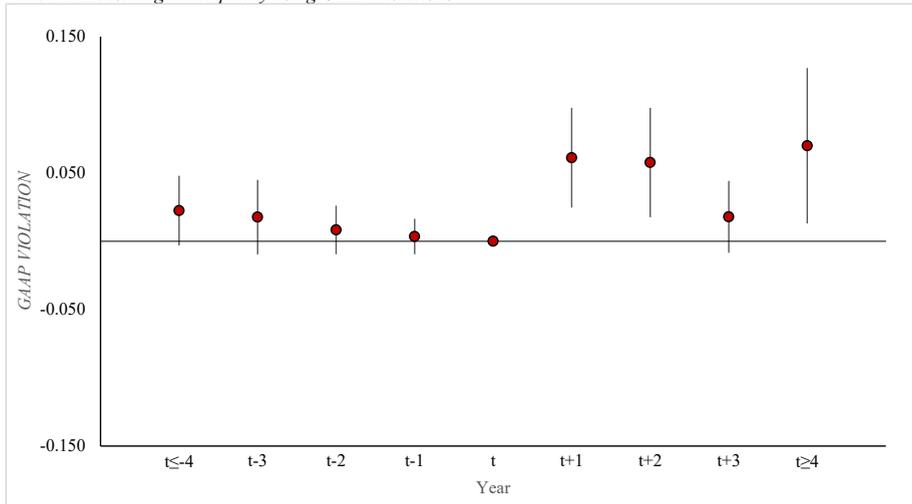
Panel B: Measuring audit quality using modified audit opinions
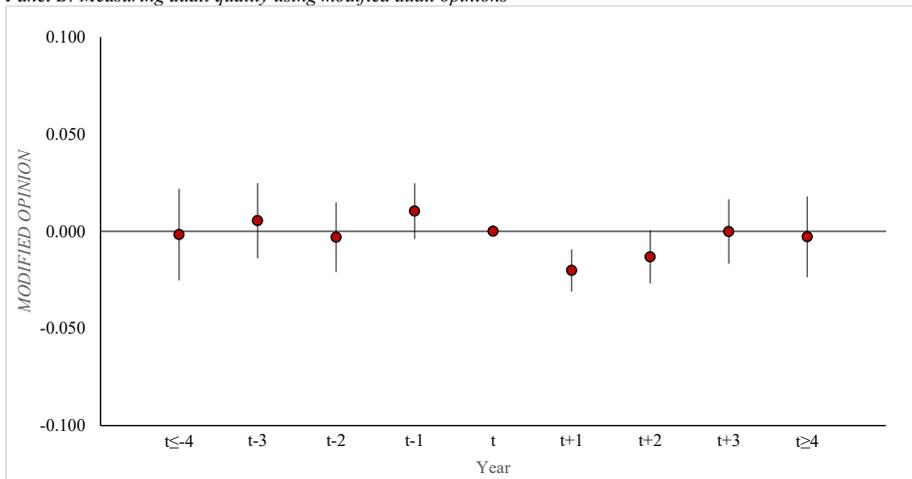
Panel C: Measuring audit quality using income-decreasing adjustments to earnings during year-end audits
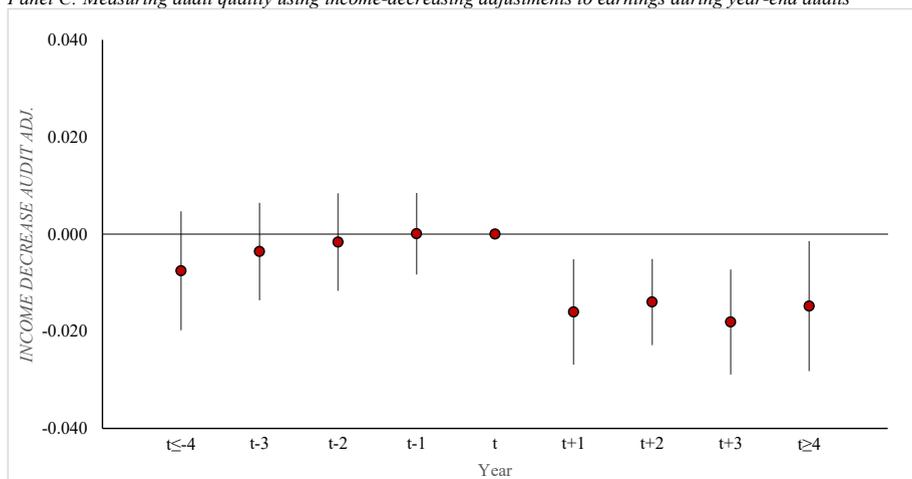
Fig. 2. Effect of bullet train connectivity on audit quality. *Notes:* In the figures above, the x-axis represents time relative to the year in which a company's (prefectural) city of headquarters becomes connected to the bullet train network, and the y-axis in panel A (B, C) represents the average number of GAAP violations (modified audit opinions, income decreasing audit adjustments to earnings) for companies in each period. The figure plots the two-tailed 90% confidence interval around each point estimate of the relation between bullet train openings and audit quality proxies.

the first (second, and third) column in Table 5, Panel B.[9] Overall, these results are consistent with the hypothesis that bullet train connectivity increases audit market competition and thus results in lower audit quality.

6.2. Cross-sectional analyses: intensity of competitive effect

To corroborate our inference, we examine whether the relation between bullet train connectivity and audit quality is stronger when such connectivity puts greater competitive pressure on the incumbent audit firm. First, we exploit variation in the number of competing audit firms that have an office within a short bullet train ride of the company's headquarters. Prior studies show that bullet trains offer a comparative advantage over air travel for distances of up to 500 miles; for journeys under 100 miles, road transportation is preferable (Amos et al., 2010; Dong et al., 2020). Thus, we predict that incumbent auditors whose clients have a large pool of competing audit firms located within a 100-to-500 mile radius of the client's office will face a greater competitive threat, following the introduction of bullet trains. We focus on the number of *additional* audit firms that might compete for the audit business of a client rather than the *total* number of audit firms located near the client. Bullet train connectivity is likely to have the largest effect on audit market competition in instances where clients had few audit firm options prior to its introduction and several additional options after its introduction. However, if a company could choose from a large pool of audit firms even before the introduction of bullet trains or has relatively few options to choose from even after the introduction of bullet trains, then the effect of bullet train connectivity on audit market competition is likely to be muted.

We test this prediction by creating an indicator variable, *# NEW COMPETITORS*, that equals one if the number of audit firms located within a 100-to-500 mile radius of the client's office is greater than the sample median *and* the number of audit firms located within a 100-mile radius of the client is less than the sample median. Further, we construct this variable using the number of audit firms located in cities connected to the client's city via a bullet train (rather than using all cities within a 500-mile radius of the company). We then modify equation (2) to include an interaction term between *# NEW COMPETITORS* and *BULLET TRAIN OPEN*.[10] Columns 1, 4, and 7 in Table 6, Panel A, present the regression result when the dependent variable is *GAAP VIOLATION*, *MODIFIED OPINION*, and *INCOME DECREASE AUDIT ADJ*, respectively. Column 1 (4 and 7) shows that the coefficient for *BULLET TRAIN OPEN* × *# NEW COMPETITORS* is positive (negative) and significant at the 10% level or better. These coefficients indicate that the incidence of GAAP violations is higher, the incidence of clients receiving a modified audit opinion is lower, and the magnitude of income-decreasing audit adjustments is lower among companies that are brought "closer" to a larger pool of competing audit firms as a result of bullet train connectivity.

Next, we exploit cross-sectional variation in the intensity of bullet train connectivity in a city. Some prefectural cities have more than 200 trains passing through the city each day while others have fewer than 20 trains passing through the city each day. The impact of bullet train connectivity on audit market competition is presumably greater in cities that offer relatively more bullet train connections to nearby cities where competing audit firms are located. Thus, we predict that the negative relation between the introduction of bullet trains and audit quality would be stronger when more trains connect the focal company's city to nearby cities where competing audit firms are located. To test this prediction, we create an indicator variable, *# TRAINS*, that equals one if the number of bullet trains connecting the focal company's city to other cities with at least one audit firm is greater than the sample median. We then modify equation (2) to include an interaction term between *# TRAINS* and *BULLET TRAIN OPEN*. Columns 2, 5, and 8 in Table 6, Panel A, present the regression result when the dependent variable is *GAAP VIOLATION*, *MODIFIED OPINION*, and *INCOME DECREASE AUDIT ADJ*, respectively. Column 2 (5 and 8) shows that the coefficient for *BULLET TRAIN OPEN* × *# TRAINS* is positive (negative) and significant at the 10% level or better. These coefficients indicate that the incidence of GAAP violations is higher, the incidence of clients receiving a modified audit opinion is lower, and the magnitude of income-decreasing audit adjustments is lower for companies located in cities that have a higher frequency of bullet trains connecting the focal city to other cities with competing audit firms.

Finally, we examine whether the relation between bullet train connectivity and audit quality is stronger when an incumbent auditor is located near its client. Bullet train connectivity can strengthen auditor-client relationships where the incumbent auditor is located at a distance from the client because bullet trains potentially reduce travel time between the offices of such 'non-local' incumbent auditors and their clients. In contrast, when the incumbent audit firm is located close to its client, it does not benefit from bullet train connectivity but faces additional competitive pressure from non-local audit firms insofar as the distance between non-local audit firms and a client's headquarters constrained the client's choice of audit firm pre-bullet trains.

To test this prediction, we create an indicator variable, *PROXIMATE INCUMBENT*, that equals one for companies located within 100 miles of their incumbent auditor's office (we focus on the incumbent auditor in the pre-treatment period). We modify equation (2) to include an interaction term between *PROXIMATE INCUMBENT* and *BULLET TRAIN OPEN*. Columns 3, 6,

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[9] We find that the effect of bullet train connectivity on modified audit opinions becomes insignificant in years three and four onwards after treatment. We suspect that the lack of significance is due to the few instances of companies receiving a modified audit opinion during our sample period combined with the declining trend in the percentage of company-years with a modified audit opinion. Unconditionally, 2.7% or 207 company-year observations receive a modified audit opinion. Further, the average percentage of company-years with a modified audit opinion decreases from 4.2% during 2007–2009 to 2.4% during 2015–2017, perhaps because the early part of our sample overlaps with the global financial crisis.

[10] *# NEW COMPETITORS* is a time-invariant city characteristic. Thus, its main effect is absorbed by our fixed effects.

Table 6

Cross-sectional analyses based on variation in the effect of bullet trains on audit market competition.

Panel A: Full sample

Dependent Variable:	GAAP VIOLATION			MODIFIED OPINION			INCOME DECREASE AUDIT ADJ.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.
BULLET TRAIN OPEN [A]	0.038**	0.037**	0.044**	−0.015**	−0.011*	−0.017***	−0.015***	−0.011**	−0.015***
	(2.44)	(1.97)	(2.37)	(−2.30)	(−1.68)	(−2.67)	(−3.57)	(−2.08)	(−3.35)
...× # NEW COMPETITORS [B]	0.030**			−0.008*			−0.008**		
	(2.19)			(−1.85)			(−2.57)		
...× # TRAINS [C]		0.017*			−0.011**			−0.008**	
		(1.83)			(−2.11)			(−2.26)	
...× PROXIMATE INCUMBENT [D]			0.027***			−0.013***			−0.007**
			(2.98)			(−2.96)			(−2.22)
# NEW COMPETITORS	−0.007			0.003			0.001		
	(−1.01)			(0.82)			(0.31)		
PROXIMATE INCUMBENT			−0.004			0.012**			0.004
			(−0.52)			(2.19)			(1.21)
		p Value			*p* Value			*p* Value	
[A] + [B]	0.012			0.003			0.000		
[A] + [C]		0.008			0.001			0.000	
[A] + [D]			0.004			0.001			0.000
Control Variables	Included	Included	Included	Included	Included	Included	Included	Included	Included
Company Indicators	Included	Included	Included	Included	Included	Included	Included	Included	Included
Audit Firm Indicators	Included	Included	Included	Included	Included	Included	Included	Included	Included
Industry × Year Indicators	Included	Included	Included	Included	Included	Included	Included	Included	Included
Province × Year Indicators	Included	Included	Included	Included	Included	Included	Included	Included	Included
R-Squared	62.8%	62.6%	62.8%	55.4%	55.4%	55.4%	25.7%	25.6%	25.7%
No. of Observations	7675	7675	7675	7675	7675	7675	5667	5667	5667

Panel B: Sub-sample of company-year observations for years 2014−2017

Dependent Variable:	GAAP VIOLATION		MODIFIED OPINION		INCOME DECREASE AUDIT ADJ.	
	(1)	(2)	(3)	(4)	(5)	(6)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.
BULLET TRAIN OPEN	0.037	0.059**	−0.019	−0.021	−0.021	−0.008*
	(1.44)	(2.13)	(−1.32)	(−1.41)	(−1.49)	(−1.91)
...× # NEW COMPETITORS	0.028	0.051*	−0.023**	−0.018*	−0.005	−0.014***
	(1.42)	(1.77)	(−1.98)	(−1.80)	(−0.67)	(−3.63)
Control Variables	Included	Included	Included	Included	Included	Included
Company Indicators	Included	Excluded	Included	Excluded	Included	Excluded
Audit Firm Indicators	Included	Included	Included	Included	Included	Included
Industry × Year Indicators	Included	Included	Included	Included	Included	Included
Province × Year Indicators	Included	Included	Included	Included	Included	Included
R-Squared	74.4%	25.6%	65.7%	23.0%	59.4%	12.7%
No. of Observations	3800	3800	3800	3800	1792	1792

Panel C: Sub-sample of company-year observations for years 2014−2017

Dependent Variable:	GAAP VIOLATION		MODIFIED OPINION		INCOME DECREASE AUDIT ADJ.	
	(1)	(2)	(3)	(4)	(5)	(6)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.
BULLET TRAIN OPEN	0.052*	0.044*	−0.038***	−0.031**	−0.017	−0.004
	(1.78)	(1.71)	(−2.88)	(−2.26)	(−1.26)	(−0.97)
...× # TRAINS	0.002	0.039**	−0.019**	−0.016*	−0.018**	−0.005*
	(0.14)	(2.59)	(−2.15)	(−1.85)	(−2.49)	(−1.90)
Control Variables	Included	Included	Included	Included	Included	Included
Company Indicators	Included	Excluded	Included	Excluded	Included	Excluded
Audit Firm Indicators	Included	Included	Included	Included	Included	Included
Industry × Year Indicators	Included	Included	Included	Included	Included	Included
Province × Year Indicators	Included	Included	Included	Included	Included	Included

Table 6 (*continued*)

Panel C: Sub-sample of company-year observations for years 2014−2017

Dependent Variable:	GAAP VIOLATION		MODIFIED OPINION		INCOME DECREASE AUDIT ADJ.	
	(1)	(2)	(3)	(4)	(5)	(6)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.	*t*-Stat.
R-Squared	74.3%	24.5%	65.7%	23.1%	59.4%	12.2%
No. of Observations	3800	3800	3800	3800	1792	1792

Notes: This table presents the results from regressions of audit quality proxies on an indicator variable for the introduction of bullet train connectivity in a city, an interaction term between the bullet train indicator and the cross-sectional partitioning variable, and control variables. Panel A presents the results using our full sample period and Panels B and C restrict the sample to the period 2014 to 2017. See the Variable Appendix for detailed variable definitions. The standard errors are clustered at the city-level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

and 9 in Table 6, Panel A, present the regression result when the dependent variable is *GAAP VIOLATION*, *MODIFIED OPINION*, and *INCOME DECREASE AUDIT ADJ*, respectively. Column 3 (6 and 9) shows that the coefficient for *BULLET TRAIN OPEN* × *PROXIMATE INCUMBENT* is positive (negative) and significant at the 5% level or better. These coefficients indicate that the incidence of GAAP violations is higher, the incidence of clients receiving a modified audit opinion is lower, and the magnitude of income-decreasing audit adjustments is lower among companies located near their auditor after their city is connected to the bullet train network.[11] These results imply that incumbent auditors located near their clients face a greater competitive threat from the introduction of bullet trains and resort to allowing their clients greater reporting discretion. In sum, results from three cross-sectional tests support our inference that the introduction of bullet trains leads to a decline in audit quality due to an increase in the competitive threats faced by incumbent auditors.

Note that we are unable to obtain historical train schedules and thus use the current schedule as a proxy for the historical schedule. This data limitation creates measurement error in the variables *# NEW COMPETITORS* and *# TRAINS*. To mitigate measurement error concerns, we examine whether the results of the cross-sectional tests employing the variables *# NEW COMPETITORS* and *# TRAINS* are similar when we restrict our sample to the most recent four years in our data (2014−2017), when the measurement error from using the current train schedule in lieu of historical schedules is likely to be muted. Table 6, Panels B and C present the results for the cross-sectional test related to *# NEW COMPETITORS* and *# TRAINS*, respectively. In Panel B, we find that the coefficient for *BULLET TRAIN OPEN* × *# NEW COMPETITORS* is similar in terms of sign and magnitude compared to those reported in Panel A but insignificant for GAAP violations and audit adjustments. The insignificant results are potentially due to a drop in sample size and the significantly reduced variation in *BULLET TRAIN OPEN* post-2014 (i.e., more than 52% of the cities in our sample were connected to the bullet train network by 2014). If we relax the fixed effects structure by excluding company-fixed effects (considering we use only four years of data for this test, which greatly limits the number of pre- and post-treatment years for an individual company), we find that the coefficient for *BULLET TRAIN OPEN* × *# NEW COMPETITORS* is significant with the predicted sign in all three regressions.

In Panel C, we find that the coefficient for *BULLET TRAIN OPEN* × *# TRAINS* is insignificant for GAAP violations (which again could be due to a drop in sample size) but continue to remain significant for modified opinions and audit adjustments. Further, if we relax the fixed effects structure by excluding company-fixed effects, we find that the coefficient for *BULLET TRAIN OPEN* × *# TRAINS* is significant with the predicted sign in all three regressions.

6.3. Cross-sectional analyses: clients' ex ante preference for audit quality

Prior research finds that market and institutional forces lead to differences in companies' incentives to be transparent in their financial reporting (Ball et al., 2003; Wang et al., 2008). Following an increase in audit market competition, companies that have greater incentives to be transparent are less likely to bargain for greater financial reporting discretion with their auditors in a manner that lowers audit quality. In other words, we predict that companies' ex ante incentive to be transparent will moderate the negative effect of competition on audit quality.

We use three approaches to identify companies' ex ante incentives to be transparent. First, we exploit variation in companies' ex ante auditor choice to infer their preference for audit quality. Prior research argues that clients and auditors select each other based on their preferences for audit quality and audit fees (DeAngelo, 1981; Dye, 1993; Gerakos and Syverson, 2015). Thus, a company's decision to hire an auditor with a reputation for supplying high-quality audits 'reveals' its preference for audit quality. Prior research in the Chinese setting finds that the ten largest audit firms in China supply higher-quality audits than the remaining audit firms (DeFond et al., 1999; Lennox et al., 2016). Thus, we proxy for a company's

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[11] One subtle point to note is that the main effect for *PROXIMATE INCUMBENT* is identified in our regressions even though we control for company- and auditor-fixed effects. This occurs because our sample includes companies located in cities that never launch bullet trains during our sample period, and for these companies, *PROXIMATE INCUMBENT* is coded based on these companies' auditor choices in period t-1. Since these 'never treated' companies do on occasion change auditors during our sample period, the inclusion of company-fixed effects in our regressions does not absorb the main effect of *PROXIMATE INCUMBENT*.

Table 7
Cross-sectional analyses based on variation in companies' ex ante demand for audit quality.

Dependent Variable:	GAAP VIOLATION			MODIFIED OPINION			INCOME DECREASE AUDIT ADJ.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.
BULLET TRAIN OPEN [A]	0.046**	0.053**	0.046**	−0.017***	−0.018**	−0.017***	−0.016***	−0.013**	−0.018***
	(2.42)	(2.22)	(2.40)	(−2.70)	(−2.01)	(−2.67)	(−3.85)	(−2.28)	(−4.21)
...× PRE-TREAT BIG10[B]	−0.013*			0.007*			0.008***		
	(−1.97)			(1.95)			(2.85)		
...× PRE-TREAT TRANSPARENCY [C]		−0.017*			0.011**			0.006**	
		(−1.96)			(2.27)			(2.39)	
...× PRE-TREAT ANALYSTS [D]			−0.020***			0.012***			0.006**
			(−2.70)			(3.31)			(2.11)
PRE-TREAT BIG10	0.007			0.004			−0.004		
	(1.41)			(1.03)			(−1.55)		
PRE-TREAT TRANSPARENCY		0.004			−0.002			−0.002	
		(0.44)			(−0.44)			(−0.46)	
PRE-TREAT ANALYSTS			0.004			−0.003			0.013***
			(0.59)			(−0.58)			(3.40)
	p Value			p Value			p Value		
[A] + [B]	0.097			0.165			0.112		
[A] + [C]		0.112			0.442			0.263	
[A] + [D]			0.090			0.485			0.015
Control Variables	Included	Included	Included	Included	Included	Included	Included	Included	Included
Company Indicators	Included	Included	Included	Included	Included	Included	Included	Included	Included
Audit Firm Indicators	Included	Included	Included	Included	Included	Included	Included	Included	Included
Industry × Year Indicators	Included	Included	Included	Included	Included	Included	Included	Included	Included
Province × Year Indicators	Included	Included	Included	Included	Included	Included	Included	Included	Included
R-Squared	62.6%	66.4%	62.7%	55.4%	59.7%	55.4%	25.7%	32.8%	26.2%
No. of Observations	7675	4871	7675	7675	4871	7675	5667	3539	5667

Notes: This table presents the results from regressions of audit quality proxies on an indicator variable for the introduction of bullet train connectivity in a city, an interaction term between the bullet train indicator and the cross-sectional partitioning variable, and control variables. See the Variable Appendix for detailed variable definitions. The standard errors are clustered at the city-level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

ex ante preference for greater audit quality using an indicator variable that equals one if the company is audited by a Big-10 audit firm pre-treatment (*PRE-TREAT BIG10*). We then test our prediction by modifying equation (2) to include an interaction term between *PRE-TREAT BIG10* and *BULLET TRAIN OPEN*.

Columns 1, 4, and 7 in Table 7 present the regression result when the dependent variable is *GAAP VIOLATION*, *MODIFIED OPINION*, and *INCOME DECREASE AUDIT ADJ*, respectively. Column 1 (4 and 7) shows that the coefficient for *BULLET TRAIN OPEN × PRE-TREAT BIG10* is negative (positive) and significant at the 10% level or better, indicating that the incidence of GAAP violations is lower, the incidence of clients receiving a modified audit opinion is higher, and the magnitude of income-decreasing audit adjustments is higher among companies audited by a Big-10 auditor prior to the increase in audit market competition brought about by the inception of bullet trains in a city.[12]

Next, we exploit cross-sectional variation in a company's pre-treatment transparency to infer its preference for transparency. Several market and institutional factors affect the demand for, and supply of, disclosure and financial reporting transparency, which is ultimately reflected in the equilibrium degree of transparency observed. We use the pre-treatment transparency of a company to capture the aggregate effect of all factors that affect companies' incentives to be transparent. We then test whether companies with greater ex-ante transparency are less likely to observe audit quality declines following the increase in audit market competition induced by bullet train connectivity. We test this prediction by partitioning companies into two groups based on a transparency index created by the Shenzhen Exchange. Since 2001, the Shenzhen Exchange assesses the degree to which companies are transparent in their disclosures, and gives each company a grade ranging from "A" to "D."[13] We create an indicator variable, *PRE-TREAT TRANSPARENCY*, that equals one for companies

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[12] As indicated earlier, the main effect for *PRE-TREAT BIG10* is identified in our regressions even though we control for company- and auditor-fixed effects because our sample includes companies that are never treated during our sample period, and for these companies, *PRE-TREAT BIG10* is coded based on these company's auditor choice in period t-1. Auditor changes in the 'never treated' companies help identify the main effect of *PRE-TREAT BIG10*. The same argument applies to the next two cross-sectional results presented in Table 7.

[13] The Shenzhen Exchange assigns its transparency grade based on 1) The authenticity, accuracy, completeness, timeliness, fairness, and legal compliance of the information disclosed; 2) Effectiveness of information disclosure; 3) Specifications for voluntary information disclosure; 4) Investor relations management; 5) Disclosure of the performance of social responsibilities; 6) Information disclosure affairs management; 7) being punished, or taken supervisory measures; 8) Cooperation between the listed company and the Exchange; 9) Other circumstances identified by the Exchange.

that receive a transparency grade of "A" in the pre-treatment period. We then modify equation (2) to include an interaction term between *BULLET TRAIN OPEN* and *PRE-TREAT TRANSPARENCY*.[14]

Column 2 (5 and 8) in Table 7 shows that the coefficient for *BULLET TRAIN OPEN × PRE-TREAT TRANSPARENCY* is negative (positive) and significant at the 10% level or better when the dependent variable is *GAAP VIOLATION* (*MODIFIED OPINION* and *INCOME DECREASE AUDIT ADJ*). These coefficients indicate that the probability of receiving a GAAP violation (modified audit opinion) is lower (higher), and the magnitude of income-decreasing audit adjustments is higher for companies that are more transparent in the pre-treatment period following the introduction of bullet trains in a city.

Lastly, we examine whether companies that have greater analyst following pre-treatment are less likely to see their audit quality decline following the inception of bullet trains. Prior studies find that analysts play a governance and monitoring role, such that greater analyst coverage helps curb earnings management (Yu, 2008; Bradley et al., 2017). To the extent analysts promote transparency, we predict that the negative relation between bullet train connectivity and audit quality will be weaker for companies that have greater analyst coverage pre-treatment. To test this prediction, we create an indicator variable, *PRE-TREAT ANALYSTS*, that equals one for companies with above-median analyst coverage in the pre-treatment period. We then estimate equation (2) with an interaction term between *PRE-TREAT ANALYSTS* and *BULLET TRAIN OPEN*.

Column 3 (6 and 9) in Table 7 shows that the coefficient for *BULLET TRAIN OPEN × PRE-TREAT ANALYSTS* is negative (positive) and significant at the 5% level or better when the dependent variable is *GAAP VIOLATION* (*MODIFIED OPINION* and *INCOME DECREASE AUDIT ADJ*). These results indicate that the probability of GAAP violations (modified audit opinion) is lower (higher), and the magnitude of income-decreasing audit adjustments is higher for companies with greater analyst coverage after their city is connected to the bullet train network. Collectively, the above evidence supports the argument that the negative effect of audit market competition on audit quality is weaker when companies have greater incentives to be transparent.

7. Additional analyses and robustness tests

7.1. Instrumental variables and placebo tests

Our primary source of identification is the introduction of a bullet train route in a city, which is staggered over time. However, as we discussed earlier, the timing and placement of bullet train routes are unlikely to be random; rather, such key infrastructure decisions are often a function of expected economic benefits based on current/expected city growth rates, political connections, financing availability, etc. If the factors that affect the time when, or likelihood that, a city becomes connected to the bullet train network also affect audit quality for reasons unrelated to changes in audit market competition, then our inferences could be biased. Consider, for example, that a city's propensity to be connected to the bullet train network and audit quality are correlated due to a third variable − city growth. Fast-growing cities have a rising demand for transportation and the central government is more likely to place a bullet train station in such cities sooner rather than later. At the same time, the companies located in high-growth cities are more likely to be fast growing, which could result in such companies having lower accrual quality and a higher propensity to violate GAAP.[15] If so, city growth could be a correlated omitted variable that biases our inferences.

To mitigate such endogeneity concerns, we first estimate a two-stage-least-squares (2SLS) regression and instrument for the timing when bullet train connectivity commences using a proxy for the engineering and construction difficulties that would be encountered to connect a city to the bullet train network due to its natural geographic attributes (Lin, 2017; Dong et al., 2020). Specifically, we follow Dong et al. (2020) and use the average geographic slope within each city's administration area to instrument for when a bullet train route becomes operational. The idea is that sloped terrains significantly increase the cost and difficulty of construction activity (Saiz, 2010), and thus cities with such terrains are likely to be connected to the bullet train network in a later period relative to cities with flat terrains. Following Dong et al. (2020), our instrument (*SLOPE*) is calculated by first computing the difference between the maximum and minimum elevation in each 10 × 10 km grid in a city's administrative area. *SLOPE* is then the average difference in elevation across all 10 × 10 grids in the city. Cities with a smaller average slope have a higher probability of being connected to the bullet train network early on, following the government's decision to connect the city to the bullet train network.

The exclusion restriction for our instrument to be valid is that the geographic terrain of a city does not differentially affect audit quality pre-vs. post-bullet train connectivity (independent of its effect via changes in audit market competition). Since the slope instrument is a function of a city's location, and location can affect company growth (and thus audit quality), it is plausible that our instrument does not satisfy the exclusion restriction. To mitigate this concern, we control for several variables related to a city's size and growth (e.g., city-level GDP, population and average wages in both levels and changes, highway and air ridership, etc.). However, we caveat that we are unable to completely resolve endogeneity concerns.

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[14] We restrict our sample to companies listed on the Shenzhen Exchange for this test. However, we find that our results are insensitive to using other proxies for reporting quality, including discretionary accruals and accruals quality.

[15] Audit adjustments are less likely to be affected by such correlated omitted variable concerns to the extent both, pre-audit earnings and audited earnings, are similarly affected by city growth or other determinants of company earnings.

Table 8
Instrumental variable analyses.

Panel A: 1st stage regressions			
Dependent Variable:		BULLET TRAIN OPEN	
		1st Stage Regressions	
	GAAP VIOLATION	MODIFIED OPINION	INCOME DECREASE AUDIT ADJ.
	(1)	(2)	(3)
SLOPE	−0.021***	−0.021***	−0.024***
	(−4.20)	(−4.24)	(−4.09)
Kleibergen-Paap F Statistic	17.628	17.96	16.688
Control Variables	Included per Table 5	Included per Table 5	Included per Table 5
Company Indicators	Not Included	Not Included	Not Included
Audit Firm Indicators	Included	Included	Included
Industry × Year Indicators	Included	Included	Included
Province × Year Indicators	Included	Included	Included
R-Squared	63.7%	63.8%	62.7%
No. of Observations	7675	7675	5667
Panel B: 2nd stage regressions			
Dependent Variable:	GAAP VIOLATION	MODIFIED OPINION	INCOME DECREASE AUDIT ADJ.
		2nd Stage Regressions	
	(1)	(2)	(3)
BULLET TRAIN OPEN	0.177*	−0.133**	−0.027**
	(1.82)	(−2.11)	(−2.25)
Control Variables	Included per Table 5	Included per Table 5	Included per Table 5
Company Indicators	Not Included	Not Included	Not Included
Audit Firm Indicators	Included	Included	Included
Industry × Year Indicators	Included	Included	Included
Province × Year Indicators	Included	Included	Included
R-Squared	4.2%	4.4%	0.9%
No. of Observations	7675	7675	5667

Notes: Panel A (B) in this table presents the results from the first (second) stage of an instrumental variables regression of audit quality proxies on an indicator variable for the introduction of bullet train connectivity in a city and control variables. We instrument for the likelihood of a city being connected to the bullet train network using the average slope or elevation of a city. See the Variable Appendix for detailed variable definitions. The standard errors are clustered at the city-level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

The results from our 2SLS regressions are reported in Table 8. We are unable to include company-fixed effects in these regressions because our instrument, *SLOPE*, is a time-invariant city characteristic and will be absorbed by the company indicators. Panel A presents the results from the first stage regressions, where we find that the coefficient for *SLOPE* is negative and significant at the 1% level in all three regressions (we estimate three first-stage regressions because the set of control variables differs based on the audit quality proxy examined). The first stage coefficients indicate that cities with more sloped terrains are significantly less likely to be connected to the bullet train network early in our sample period. The Kleibergen−Paap F-statistic exceeds the rule-of-thumb value of 10 used in prior research and the Stock and Yogo (2005) 10% critical values, providing comfort that the instrument is relevant. Panel B presents the second stage results. We find that the coefficient for *BULLET TRAIN OPEN* is positive (negative) and significant at the 10% level or better when the dependent variable is *GAAP VIOLATION* (*MODIFIED OPINION* and *INCOME DECREASE AUDIT ADJ*). These results support our main inference that bullet train induced increases in audit market competition lead to lower audit quality.[16]

Next, we conduct a placebo test where we examine whether the bullet train connectivity affects audit quality for companies headquartered in provincial cities. All 36 provincial cities in China are home to several audit offices (see Table 1). Thus, we predict that bullet train connectivity will not affect audit quality for companies located in provincial cities because it does not affect audit market competition. In contrast, if the relation between bullet train connectivity and audit quality is driven by a correlated omitted variable such as city growth, then we should find that bullet train connectivity is associated with audit quality even for companies located in provincial cities. Table 9 presents the results. Consistent with our prediction, we find that bullet train connectivity is not associated with audit quality for companies headquartered in provincial cities.[17]

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[16] In untabulated analyses, we verify that our results are robust to excluding companies located in cities that are the starting or ending points of train routes (since connection to such cities is more likely to be endogenous).

[17] In untabulated analyses, we test whether companies located in more developed regions (but still in prefectural cities) that have greater access to alternative modes of transportation infrastructure (highways and airplane connectivity) are relatively less impacted by the commencement of bullet train operations in the city. We find that the association between bullet train connectivity and audit quality is weaker in more developed regions in China (where we measure regional development based on Fan Gang's economic development index).

Table 9
Placebo test: Bullet train connectivity and audit quality in provincial cities.

Dependent Variable:	GAAP VIOLATION	MODIFIED OPINION	INCOME DECREASE AUDIT ADJ.
	(1)	(2)	(3)
	Coefficient	Coefficient	Coefficient
	t-Statistic	t-Statistic	t-Statistic
BULLET TRAIN OPEN	0.021	0.005	0.005
	(0.79)	(0.45)	(0.58)
Control Variables	Included	Included	Included
Company Indicators	Included	Included	Included
Audit Firm Indicators	Included	Included	Included
Industry × Year Indicators	Included	Included	Included
Province × Year Indicators	Included	Included	Included
R-Squared	48.2%	54.6%	23.7%
No. of Observations	11,872	11,872	8788

Notes: This table presents the results from regressions of audit quality proxies on an indicator variable for the introduction of bullet train connectivity in a city and control variables for the sample of companies headquartered in provincial cities (rather than prefectural cities as we do for the rest of analyses in the paper). See the Variable Appendix for detailed variable definitions. The standard errors are clustered at the city-level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

7.2. Effect of bullet trains on the behavior of other stakeholders

Bullet train connectivity not only affects audit markets and the behavior of audit firms, but is also likely to affect the behavior of other stakeholders in a company, such as institutional investors, financial analysts, and regulators. For example, by reducing travel time, bullet trains could reduce information processing costs for analysts and investors located in other cities (Chen et al., 2022; Gao et al., 2021), or enforcement costs for regulators located in other cities, which could then affect our audit quality proxies and confound the interpretation of our results.

To mitigate such concerns, in our main tests, we control for other institutional forces that might confound the interpretation of our results. Specifically, we control for institutional ownership, analyst following, and province × year fixed effects. Since regulatory oversight is typically determined at the province-level, province × year fixed effects control for enforcement changes over time. In addition, we design our empirical tests in a manner that helps isolate the effect of bullet train connectivity on audit market competition from other effects of bullet train connectivity (e.g., we focus on companies located in prefectural cities where pre-treatment audit market competition is low). We also note that by examining multiple audit quality proxies, we limit the set of alternative interpretations of our results. For example, the evidence that bullet train connectivity increases the likelihood of a GAAP violation and simultaneously decreases the magnitude of income-decreasing audit adjustment is consistent with our hypothesis but is inconsistent with the idea that bullet train connectivity improves stakeholder monitoring, which then would improve transparency and audit quality.

To further assuage the above concern, we examine whether bullet train connectivity affects the quality of pre-audit earnings and changes the likelihood of regulatory sanctions related to a company's non-GAAP voluntary disclosures that are outside the purview of auditors (e.g., instances where companies delay disclosure, issue misleading disclosures outside audited financial statements, etc.). If bullet train connectivity reduces investor/analyst information acquisition costs and

Table 10
Effect of bullet train connectivity on pre-audit financial reporting quality.

Dependent Variable:	NON-GAAP VIOLATION	PRE-AUDIT |DISC. ACCRUALS|	PRE-AUDIT ACCRUALS QUALITY
	(1)	(2)	(3)
	Coefficient	Coefficient	Coefficient
	t-Statistic	t-Statistic	t-Statistic
BULLET TRAIN OPEN	−0.015	−0.001	−0.003
	(−1.06)	(−0.44)	(−0.79)
Control Variables	Included	Included	Included
Company Indicators	Included	Included	Included
Audit Firm Indicators	Included	Included	Included
Industry × Year Indicators	Included	Included	Included
Province × Year Indicators	Included	Included	Included
R-Squared	35.8%	78.0%	59.5%
No. of Observations	7675	5667	5667

Notes: This table presents the results from regressions of proxies for pre-audit financial reporting quality on an indicator variable for the introduction of bullet train connectivity in a city and control variables. See the Variable Appendix for detailed variable definitions. The standard errors are clustered at the city-level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

Table 11

Effect of bullet trains on the rate of false negatives in modified audit opinions.

Dependent Variable:	Special Treatment Firm Indicator	
	Observations with MAO = 0	
	Coefficient	t-Statistic
BULLET TRAIN OPEN	0.029**	2.45
LN(ASSETS)	−0.030**	−2.01
LEVERAGE	0.277***	4.90
ROA	0.122	1.19
ANALYST FOLLOWING	−0.011**	−2.24
%INSTITUTIONAL OWN	−0.038	−0.60
BIG10	−0.005	−0.63
LN(TENURE)	−0.026***	−2.92
CITY GDP GR	−0.098	−0.99
CITY GDP	0.005	0.22
CITY POPULATION GR	−0.101	−0.79
CITY POPULATION	0.210**	2.56
CITY WAGE GR	−0.007	−0.12
CITY WAGE	0.047	0.77
AIR RIDERSHIP	−0.001	−0.47
HIGHWAY TRAFFIC	−0.001	−0.08
LN(AGE)	0.144***	2.61
LOSS	0.101***	5.70
CURRENT RATIO	0.003	1.05
ACC. RECEIVABLE	−0.048	−0.39
INVENTORY	−0.237***	−3.42
RETURNS	−0.002	−0.42
INFLUENCE	0.000	0.01
Company Indicators	Included	
Audit Firm Indicators	Included	
Industry × Year Indicators	Included	
Province × Year Indicators	Included	
R-Squared	58.6%	
No. of Observations	7470	

Notes: This table presents the results from a regression of an indicator variable for whether a company becomes financially distressed within the next three years on an indicator variable for the introduction of bullet train connectivity in a city and control variables, conditional on the company receiving no modified audit opinion. Distressed companies are those given 'special treatment' by the Shanghai or Shenzhen stock exchanges. See the Variable Appendix in the paper for other variable definitions. The standard errors are clustered at the city-level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

facilitates better monitoring, then we should observe changes in not just audited earnings but also the quality of pre-audit earnings and unaudited voluntary disclosures.

Table 10 presents the results from estimating equation (2) using non-GAAP violations and pre-audit earnings quality as our dependent variables. *NON-GAAP VIOLATION* is an indicator variable that equals one for company-years in which a non-GAAP violation is committed. We classify sanctions issued by the CSRC, local regulatory agencies, and the Shanghai/Shenzhen stock exchanges as a non-GAAP violation if the sanction is related to a company's disclosure but does not concern its audited financial statements. We also construct two accrual based measures of earning quality using pre-audit accruals: the absolute magnitude of pre-audit performance matched discretionary accruals (Kothari et al., 2005) and pre-audit accruals quality (Dechow and Dichev, 2002; McNichols, 2002).[18] We find no significant association between *BULLET TRAIN OPEN* and any of the three proxies for pre-audit disclosure quality. These results help mitigate the concern that the relation between bullet train connectivity and our audit quality proxies is explained by changes in the behavior of a company's other stakeholders rather than its auditor.

7.3. Interpretation of modified audit opinions as a proxy for audit quality

Prior research notes that auditors can have a conservatism bias that leads them to issue a modified audit opinion (MAO) more frequently than a neutral application of GAAP would require (DeFond and Zhang, 2014). Thus, an alternative interpretation of our results is that an increase in audit market competition reduces auditors' conservatism bias (rather than audit quality). To distinguish between these two interpretations of the results, we conduct the following test.

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[18] We acknowledge that these accrual quality proxies have limitations, and suggest caution interpreting the results.

Guan et al. (2016, p.520) indicate that Chinese auditing standards require auditors to issue a MAO when they foresee that a company will face distress. Thus, instances where a company does not receive a MAO and yet faces distress in the next three years can be interpreted as auditors being *less conservative*. And evidence that bullet train connectivity increases the likelihood that companies without a MAO face imminent distress is consistent with bullet train connectivity reducing audit quality. To conduct such a test, we identify companies facing imminent financial distress as those given 'special treatment' designation in periods t+1, t+2, and/or t+3. Table 11 shows that among the sample of companies that did not receive a MAO, bullet train connectivity is positively associated with the probability a company faces distress in the next three years. This result suggests that the negative relation between bullet train connectivity and MAO is due to a reduction in auditor independence and audit quality rather than a reduction in auditor conservatism.

7.4. Effect of competition on audit fees

Greater audit market competition can affect not only audit quality but also other features of the audit market, particularly audit fees. On the one hand, prior research tests and finds that greater audit market concentration (which is typically interpreted as less competition) leads to higher audit fees, as economic theory dictates (Pearson and Trompeter, 1994; Numan

Table 12
Effect of bullet train connectivity on audit fees.

Dependent Variable:	LN(AUDIT FEES)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.	Coef.
	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.	t-Stat.
BULLET TRAIN OPEN [A]	0.014	0.016	0.018	0.012	0.014	0.009	0.014
	(0.99)	(1.07)	(1.22)	(0.89)	(1.02)	(0.51)	(1.05)
...× # NEW COMPETITORS [B]		−0.009					
		(−0.90)					
...× # TRAINS [C]			−0.008				
			(−0.79)				
...× PROXIMATE INCUMBENT [D]				0.006			
				(0.50)			
...× PRE-TREAT BIG 10[E]					−0.003		
					(−0.32)		
...× PRE-TREAT TRANSPARENCY [F]						−0.022**	
						(−1.98)	
...× PRE-TREAT ANALYSTS [G]							0.006
							(0.59)
# NEW COMPETITORS		0.002					
		(0.28)					
PROXIMATE INCUMBENT				0.009			
				(0.83)			
PRE-TREAT BIG 10					−0.002		
					(−0.32)		
PRE-TREAT TRANSPARENCY						0.005	
						(0.48)	
PRE-TREAT ANALYSTS							−0.011
							(−1.08)
	p Value						
[A] + [B]		0.632					
[A] + [C]			0.535				
[A] + [D]				0.299			
[A] + [E]					0.489		
[A] + [F]						0.444	
[A] + [G]							0.238
Control Variables	Included	Included	Included	Included	Included	Included	Included
Company Indicators	Included	Included	Included	Included	Included	Included	Included
Audit Firm Indicators	Included	Included	Included	Included	Included	Included	Included
Industry × Year Indicators	Included	Included	Included	Included	Included	Included	Included
Province × Year Indicators	Included	Included	Included	Included	Included	Included	Included
R-Squared	91.6%	91.6%	91.6%	91.6%	91.6%	91.3%	91.6%
No. of Observations	7675	7675	7675	7675	7675	4871	7675

Notes: This table presents the results from regressions of audit fees on an indicator variable for the introduction of bullet train connectivity in a city, an interaction term between the bullet train indicator and the cross-sectional partitioning variable, and control variables. See the Variable Appendix for detailed variable definitions. The standard errors are clustered at the city-level. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent levels, respectively.

and Willekens, 2012; Gerakos and Syverson, 2015). But on the other hand, a report by the Government Accountability Office (GAO, 2008) finds no evidence that audit fees are related to concentration. More importantly, Chinese regulators hold the view that price wars adversely affect audit quality and thus have taken actions to mitigate price competition (Ministry of Finance, 2010; CICPA, 2007, 2012). Specifically, several provinces have prescribed standardized fees for audit services, loosely based on clients' size, measured as total assets or revenues. However, standardized audit fees have been repealed in some provinces since 2012 (Chang et al., 2019). Thus, it is ex ante unclear whether increased audit market competition would affect audit fees in the Chinese setting.

To examine this relation, we estimate regressions with the natural logarithm of audit fees as the dependent variable on *BULLET TRAIN OPEN*, our complete fixed effects structure, and control variables (following Huang et al. (2016) and DeFond and Zhang (2014; Table 3)). Column 1 in Table 12 shows that the coefficient for *BULLET TRAIN OPEN* is insignificant (coef. = 0.014; t-stat. = 0.99). The lack of an association between bullet train connectivity and audit fees is consistent with both 1) Chinese regulators discouraging audit firms from competing on price and 2) the evidence in the GAO report of no association between audit market concentration and audit fees in the U.S. However, it is also plausible we do not find an '*on average*' relation between audit market competition and audit fees because some clients use their increased bargaining power to negotiate for greater reporting discretion while others negotiate for lower audit fees. To explore further, we examine whether the relation between bullet train connectivity and audit fees differs across cross-sectional partitions of our sample examined in Tables 6 and 7. Columns 2 to 7 in Table 12 present the results. We find that bullet train connectivity is negatively associated with audit fees for companies that are more transparent pre-treatment. We find no significant association between bullet train connectivity and audit fees in any other partition of the data.

7.5. Untabulated analyses and robustness tests

We conduct several robustness tests to support our inferences. First, we conduct diagnostics tests to address concerns raised in recent studies regarding biases that arise when using a staggered difference-in-differences (DID) regression. Specifically, recent studies find that using OLS to estimate a DID regression with staggered treatment timing can lead to biases when the treatment effect varies over time, or the treatment takes multiple periods to fully affect the dependent variable (Goodman-Bacon, 2019). Following Barrios (2021) and Baker et al. (2022), we conduct the following tests to mitigate concerns arising from our use of a staggered DID design: 1) we tabulate the distribution of treatment (i.e., the number of cities that opened a bullet train route each year in Table 2, Panel B) and verify that there is significant variation in treatment timing in our sample; 2) we verify that our inferences are robust to the exclusion of all covariates (except our independent variable of interest and fixed effects); 3) we trace out the timing of changes in audit quality between treated and control units using eight time indicators (relative to a reference pre-treatment period) in our eleven-year sample period, excluding only three years to avoid multicollinearity; and finally 4) we verify that our results are robust to using the stacked regression approach proposed by Cengiz et al. (2019) and the Roth and Sant'Anna (2021) estimator.

Second, we examine whether our documented results are different for state-owned enterprises (SOEs) vs. non-state-owned enterprises (non-SOEs). Prior research finds that SOEs are more likely to hire audit firms from the same province in which they are headquartered because such audit firms have greater local knowledge and are more likely to come under political pressure and collude with their SOE clients (Chan et al., 2006; Wang et al., 2008). On the one hand, it is plausible that audit firms located in the same province as their SOE clients are immune from changes in audit market competition because local politicians pressure SOEs to hire a local auditor. Thus, bullet train connectivity might not affect audit quality for SOEs. On the other hand, it is plausible that bullet train connectivity increases competition among the audit firms located in different cities *within* a province. We find that the effect of *BULLET TRAIN OPEN* on *MODIFIED OPINION* and *GAAP VIOLATION* is statistically no different for SOEs vs. non-SOEs. However, we find that bullet train connectivity decreases the magnitude of *INCOME DECREASE AUDIT ADJ* for SOEs by a greater magnitude than that for non-SOEs. The latter result (weakly) suggests that auditors face greater competitive threats for their SOE clients than their non-SOE clients, following the introduction of bullet trains in a city.

Third, we examine whether our results are robust to dropping observations related to cities that are never connected to the bullet train network during our sample period. Since our main regressions include company-fixed effects, the coefficient for *BULLET TRAIN OPEN* is identified only from changes in bullet train connectivity during our sample period. Thus, observations related to cities that never connect to the bullet train network soak up residual variation without directly contributing to the identification of the coefficient for *BULLET TRAIN OPEN*. Nevertheless, we test and find that our results have similar economic magnitudes and statistical significance when we drop companies located in cities not connected to bullet train network during our sample period.

Fourth, we verify that our results are robust to dropping observations related to companies that changed their auditor during our sample period. Our main regression specification controls for auditor-fixed effects and thus the coefficient for *BULLET TRAIN OPEN* is estimated using within auditor (and within company) variation in bullet train connectivity. Nevertheless, we test and verify that our results are unchanged when we drop companies that change auditors.

Finally, we verify that our results are robust to controlling for 1) audit partner-fixed effects, 2) excluding time-varying control variables, 3) using the logistic model (with a relaxed fixed effects structure) to estimate regressions when the dependent variables are *GAAP VIOLATION* or *MODIFIED OPINION*. We find that our inferences are unchanged by these tests.

8. Conclusion and policy implications

This study examines the effect of competition on audit quality. Regulators around the world have expressed concerns that companies have too few choices in the audit market and believe that the lack of choice is resulting in low competition. As a result, regulators conjecture that audit firms supply low-quality audits and/or charge high fees. Proposals aimed at increasing audit market competition include breaking up the largest four audit firms to increase the number of audit firms available for companies to choose from. Several prior studies examine the relation between audit market concentration and audit quality/ fees, but they find mixed results depending on how audit market concentration is measured (e.g., at the national- or city-level, within the Big-4 vs. between the Big-4 and the non-Big-4, etc.). Further, most prior studies do not directly address endogeneity concerns related to how markets become more or less competitive/concentrated.

In this paper, we examine the relation between audit market competition and audit quality using a quasi-natural experiment in China. Specifically, we rely on the observation that audit production requires audit staff to spend significant amounts of time at the client location, and the time spent commuting between the auditor's office and the client's office affects the cost of conducting an audit. We use the staggered inception of bullet train connectivity to prefectural cities in China as shocks to the number of audit firm choices available to companies located in these cities. By reducing travel time to the city, bullet train connectivity makes it economically viable for a significantly larger pool of audit firms to compete for the audit business of companies located in connected cities. Thus, bullet train connectivity significantly increases the threat of competition for incumbent auditors of companies located in cities that have few audit offices.

Using a generalized difference-in-differences design, we test and find that bullet train connectivity increases the incidences of GAAP violations by 4.5 percentage points (pp), decreases the likelihood that a client receives a modified audit opinion by 1.7 pp, and decreases the magnitude of income-decreasing adjustments to earnings during year-end audits by 1.6 pp. These results suggest that greater competition leads audit firms to increase their focus on client retention at the expense of auditor independence, thereby resulting in lower audit quality. Cross-sectional tests based on the extent to which bullet train connectivity affects the threat of competition for incumbent auditors and companies' ex ante preference for audit quality support our inference.

We conjecture that increases in competition lower audit quality because audits possess characteristics of a credence good, where buyers of such services (e.g., companies, investors) find it difficult to assess the quality of the service that was performed (Causholli and Knechel, 2012). As a result, when incumbent auditors are faced with a greater threat of competition, they can respond by lowering quality (i.e., reducing professional skepticism, pushing back less on clients' accounting choices, etc.) in order to retain their clients. Due to the credence good aspects of auditing, such decreases in audit quality might not catch the attention of the client company and/or its investors (Shroff, 2020), barring instances of ex-post audit failure (e.g., restatements, fraud, etc.). If regulators took policy initiatives that helped outsiders better evaluate audit quality, then perhaps greater competition would not have a negative relation with audit quality. We look forward to future research that builds on our findings.

Variable Appendix

Detailed definitions of all variables used in our empirical analyses

This table describes the procedure used to compute each variable used in our analyses. Our data are obtained either through the China Stock Market & Accounting Research (CSMAR) database, Chinese Research Data Services Platform (CNRDS), WIND, the CEIC, the China Railway Yearbook series, the China City Statistical Yearbook, the CICPA websites, the cities' Statistical Bureaus, or the Exchange websites. All continuous variables are winsorized at 1% and 99% of the distribution. The variables are listed in alphabetical order.

Variable	Definition
ACC. RECEIVABLE	The ratio of accounts receivables (data A001111) to total assets (data A001) for each company-year.
AIR RIDERSHIP	Natural logarithm of air ridership plus one for each city-year (collected from the Statistical Bureau of each city).
ANALYST FOLLOWING	Natural logarithm of one plus the number of analysts following a company in a fiscal year.
#AUDITORS	The number of unique audit firms with at least one client headquartered in a city each year.
AUDITOR CHANGE	An indicator variable that equals one for company-years that change their auditor in a fiscal year.
BIG10	An indicator variable that equals one for companies audited by one of the Big-10 audit firms. The list of Big-10 auditors is announced by CICPA in each fiscal year.
BULLET TRAIN OPEN	An indicator variable that equals one for company-years headquartered in a city *after* the opening of the *first* bullet train route.
BULLET TRAIN OPEN [-4]	An event time indicator that equals one for company-years four or more years before its city of headquarters began the first bullet train route.
BULLET TRAIN OPEN [-3]	An event time indicator that equals one for the fiscal year ending three years before the company's city of headquarters began its first bullet train route.
BULLET TRAIN OPEN [-2]	An event time indicator that equals one for the fiscal year ending two years before the company's city of headquarters began its first bullet train route.

(*continued*)

Variable	Definition
BULLET TRAIN OPEN [-1]	An event time indicator that equals one for the fiscal year immediately preceding the year the company's city of headquarters began its first bullet train route.
BULLET TRAIN OPEN [1]	An event time indicator that equals one for the fiscal year immediately following the year the company's city of headquarters began its first bullet train route.
BULLET TRAIN OPEN [2]	An event time indicator that equals one for the fiscal year ending two years after the company's city of headquarters began its first bullet train route.
BULLET TRAIN OPEN [3]	An event time indicator that equals one for the fiscal year ending three years after the company's city of headquarters began its first bullet train route.
BULLET TRAIN OPEN [4+]	An event time indicator that equals one for company-years four or more years after its city of headquarters began the first bullet train route.
CAPITAL ISSUANCE	An indicator equals one if the sum of a company's new long-term debt (data C003003) plus new equity (data C003001) exceeds 20% of total assets (data A001) in a fiscal year, following Lennox et al. (2013).
CEO DUALITY	An indicator equals one if a company's CEO is also Chairman of the Board in a fiscal year.
CITY GDP	Natural logarithm of GDP per capita of each city-year.
CITY GDP GR	GDP growth for each city-year.
CITY POPULATION	Natural logarithm of population for each city-year.
CITY POPULATION GR	Population growth in each city-year.
CITY WAGE	Natural logarithm of average wage in each city-year.
CITY WAGE GR	Average wage growth in each city-year.
CURRENT RATIO	The ratio of current assets (data A0011) to current liabilities (data A0021) for each company-year.
ΔDISTANCE	The median change in distance (in miles) at the city-year level where the change in distance is computed as the distance between auditors and their clients in year 't' minus the distance between auditors and their clients in year 't-1'.
GAAP VIOLATION	An indicator equals one for fiscal years in which a company's financial statements violated GAAP. We construct this variable using data from the CNRDS, WIND and CSMAR databases, which collect data on sanctions issued by the CSRC, local regulatory agencies, and the Shanghai/Shenzhen stock exchanges. We manually classify each sanction as a GAAP violation if it relates to the misstatements of assets, liabilities, owners' equity, cash flows, revenues, expenses, earnings, and inadequate or misleading disclosures in financial statement notes.
HHI	The Herfindahl Index of the number of unique audit firms with at least one client headquartered in a city each year.
HIGHWAY TRAFFIC	Natural logarithm of one plus the highway ridership for each city-year (collected from the Statistical Bureau of each city).
ICW	An indicator equals one for company-years that disclose any internal control material weakness or significant deficiency.
INCOME DECREASE AUDIT ADJ.	This variable is computed as the absolute value of the difference between audited earning and pre-audit earnings scaled by pre-audit earnings conditional on audited earnings being less than pre-audit earnings. The data to compute this variable are available from 2006 to 2015 and are obtained from the Chinese Minister of Finance.
%INDEPENDENT DIR	The ratio of the number of independent directors to the number of directors for each company-year.
INFLUENCE	The ratio of the audit fee received from the listed company to total audit revenue of the audit office in a fiscal year.
%INSTITUTIONAL OWN	The percentage of stock owned by institutional investors for each company-year.
INVENTORY	The ratio of inventory (data A001123) to total assets (data A001) for each company-year.
LEVERAGE	The ratio of total liabilities (data A002) to total assets (data A001) for each company-year.
LN(#AUDITORS)	Natural logarithm of one plus the number of unique audit firms with at least one client headquartered in a city each year.
LN(AGE)	Natural logarithm of the difference between the current fiscal year end and the company's founding year.
LN(ASSETS)	Natural logarithm of total assets (data A001).
LN(AUDITFEES)	Natural logarithm of audit fee paid each company-year.
LN(ΔDISTANCE)	Natural logarithm of one plus the median change in distance between auditors and their clients in each city-year.
LN(TENURE)	Natural logarithm of one plus the difference between the current year and the first year an audit firm began working as an auditor for a company.
LOSS	An indicator equals one for the fiscal years in which a company's net income before extraordinary items (data B002) is negative.
M&A	An indicator equals one if a company has an acquisition that contributes to sales in a fiscal year.
MODIFIED OPINION	An indicator that equals one for fiscal years in which a company receives 1) an unqualified audit opinion with explanatory notes, 2) a qualified audit opinion, 3) a disclaimer, or 4) an adverse audit opinion.
# NEW COMPETITORS	An indicator that equals one if the number of unique audit firms within 100-to-500 mile radius of the clients' headquarter is greater than the sample median and the number of unique audit firms located within a 100-mile radius of the clients' headquarter is less than the sample median. We construct this variable using the number of audit firms located in cities connected to the client's city via a bullet train. However, we are unable to obtain historical train schedules and thus use the current train schedule as a proxy for the historical schedule.
NON-GAAP VIOLATION	An indicator equals one for fiscal years in which a company's violated a CSRC disclosure rule that does not relate to its audited financial statements. We construct this variable using data from the CNRDS, WIND and CSMAR databases, which collect data on sanctions issued by the CSRC, local regulatory agencies, and the Shanghai/Shenzhen stock exchanges. We manually classify each sanction as a non-GAAP violation if it relates to the company's disclosure but does not concern the misstatements of assets, liabilities, owners' equity, cash flows, revenues, expenses, earnings, and inadequate or misleading disclosures in financial statement notes.
PRE-AUDIT ACCRUALS	Preaudit annual earnings minus net operating cash flows, scaled by preaudit total assets for each company-year.
PRE-AUDIT ACCRUALS QUALITY	Pre-audit accruals quality estimated using the Dechow and Dichev (2002) model and modified by McNichols (2002). We first estimate industry-year regressions of the following form: pre-audit working capital accruals = $\alpha_1 + \alpha_2$ (operating cash flows$_{t-1}$) + α_3 (operating cash flows$_t$) + α_4 (operating cash flows$_{t+1}$) + α_5 (Δ total sales − Δ accounts

(continued)

Variable	Definition
	receivable) $+ \alpha_6$ (gross PPE) $+ \varepsilon$. Pre-audit working capital accruals is pre-audit operating income plus depreciation & amortization minus operating cash flows. We scale each independent variable by the lagged value of pre-audit total assets. The unsigned residual from the above regression multiplied by minus one is our proxy for accruals quality.
PRE-AUDIT |DISC. ACCRUALS|	Unsigned performance-matched discretionary (pre-audit) accruals estimated using the Kothari et al. (2005) model. Pre-audit discretionary accruals are estimated using the modified Jones model: Pre-audit accruals $= \alpha_1 + \alpha_2$ (1/pre-audit total assets) $+ \alpha_3$ (Δ total sales $- \Delta$ accounts receivable) $+ \alpha_4$ (gross PPE) $+ \varepsilon$, where pre-audit accruals is post-audit accruals plus/minus audit adjustments. We scale each independent variable by the lagged value of pre-audit total assets. We match a company's pre-audit accruals with another company's pre-audit accruals based on year, industry, and closest value of pre-audit cash flows from operations.
PRE-TREAT ANALYSTS	An indicator variable that equals one if the number of analysts covering a company is greater than the sample median in the pre-treatment period. For companies headquartered in cities that do not become connected to the bullet train network during our sample period, this indicator equals one if the number of analysts covering the company in period t-1 is greater than the sample median.
PRE-TREAT BIG 10	An indicator variable that equals one for companies audited by one of the Big-10 audit firms in the year before the company's city was connected to the bullet train network. For companies headquartered in cities that do not become connected to the bullet train network during our sample period, this indicator equals one if a company's auditor in period t-1 is one of the Big-10 audit firms. The list of Big-10 auditors is announced by CICPA in each fiscal year.
PRE-TREAT TRANSPARENCY	An indicator variable that equals one for companies that receive an 'A' grade in the pre-treatment period on the Shenzhen Exchange transparency index. For companies headquartered in cities that do not become connected to the bullet train network during our sample period, this indicator equals one if a company receives an 'A' grade on the index in period t-1.
PROXIMATE INCUMBENT	An indicator variable that equals one for company-years in which the geographic distance between the company headquarters and its audit firm's closest office (in the pre-treatment period) is less than 100 miles.
RETURNS	Annual stock return for each company-year.
ROA	The ratio of income before extraordinary items (data B002) to average total assets of the current and proceeding year (data A001) for each company-year.
SALES GR	Total sales in the current fiscal year (data B0011) divided by total sales in the preceding fiscal year, minus one.
SLOPE	Following Dong et al. (2020), this variable is computed by first dividing each city's administrative area into ten square km grids based on longitude and latitude data. Next, calculate the difference between the maximum and minimum elevations for each grid. The *SLOPE* for each city is the average of these differences.
Special Treatment Firm Indicator	An indicator equals one for company-years that are given 'special treatment' designation in years t+1, t+2, or t+3.
# TRAINS	An indicator variable that equals one if the number of bullet trains connecting the focal company's city to other cities with at least one audit firm is greater than the sample median. We are unable to obtain historical train schedules to construct this variable, and thus assume that the current train schedule was in effect during our entire sample period (once bullet train connectivity commences in a city).
WC ACCRUALS	The change in non-cash current assets (data A0011-A001101-A001107) minus the change in current liabilities (excluding short-term debt and taxes) (data A0021-A002101-A002113) minus depreciation (data D000103+D000104), scaled by total assets (data A001), following Dechow et al. (2009).

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