Loans and Lies: Does Bank Monitoring Reduce Corporate Misreporting?[1]

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Abstract

We propose a model of bank monitoring and borrower financial misreporting. Using the staggered liberalization of the banking sector after China's accession to WTO, we find that, consistent with the model's prediction, entry by more efficient foreign banks reduces corporate misreporting fraud. Fraud reduction is greatest among borrowers of foreign banks, but fraud also drops among borrowers of domestic banks, suggesting a spillover effect. As predicted by the model, fraud reduction is greatest for borrowers with higher levels of fixed assets or lower levels of working capital. The liberalization effects are also concentrated among industries with higher growth.

Keywords: bank monitoring, corporate securities fraud, misreporting, banking liberalization.

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1. Introduction

This paper lies at the intersection of two important areas of financial research: that on bank monitoring, and that on corporate financial fraud. Beginning with Diamond (1984), a large literature has shown that monitoring their borrowers is a critical economic function of banks and related institutions. Similarly, a large literature on financial misreporting and other types of corporate fraud has shown that such fraud can impose significant costs on fraudulent firms' shareholders, reduce stock market participation, and harm the overall economy (e.g., Giannetti and Wang, 2016).

Given the importance of bank monitoring and banks' reliance on financial information from borrowers, one would think banks might have an important role in deterring financial misreporting. Nevertheless, there has been almost no theoretical or empirical work on whether improved bank monitoring does in fact reduce the incidence of misreporting fraud. We seek to fill this gap by creating a simple model of bank lending and financial misreporting and then testing this model's predictions using Chinese data from 2000-2010, a period that includes China's 2001 accession to the WTO and its subsequent bank liberalization.

We begin with a simple model that adapts the model of Povel, Singh, and Winton (2007) on shareholder monitoring to a bank lending setting. In our model, borrowers that learn their firm is in trouble may commit fraud, falsifying financial reports in the hopes of increasing the chance that their firm is refinanced by their bank. Banks monitor firms to get a better sense of the firm's actual health before they decide whether to continue or liquidate the loan. Critically, this monitoring does not only focus on financial statements per se; extensive work has documented that banks' access to their borrowers' checking account information provides them with superior information on borrowers' financial health.[3] Even though bank monitoring does not directly reveal whether financial reports are fraudulent or not, more intensive monitoring deters fraud by reducing the chance that banks will only rely on free but possibly fraudulent reports: borrowers will not commit fraud and run the risk of discovery and penalties if they know they will be liquidated anyway.

To maximize expected loan repayment, the optimal bank monitoring intensity trades off the benefits of more precise liquidate-versus-continue decisions against the incremental cost of monitoring more intensively. A decrease in marginal monitoring costs naturally makes monitoring

[3] See Mester et al. (2007), Norden and Weber (2010), and Puri et al. (2017). We thank Naveen Khanna for raising this point.

more attractive, increasing monitoring intensity and thus decreasing the likelihood of fraud. As the fixed assets (which retain most of their value in financial distress) are a larger fraction of total assets or working capital (which tends to lose much of its value in financial distress) is a lower fraction of total assets, making precise early liquidation decisions is less crucial, so monitoring decreases and fraud increases. However, the cross-derivatives of monitoring cost and these two asset composition factors have the opposite signs: essentially, if either of these two factors favors more intensive monitoring to begin with, a decrease in monitoring costs has less impact, and vice versa if monitoring incentives are weak to begin with. In other words, when making precise early liquidation decisions is crucial, monitoring intensity and fraud become less responsive to a change in monitoring costs because banks are already monitoring borrowers closely. As we discuss below, we use these cross-derivatives to fine-tune our empirical tests.

We also model how improving industry conditions affect bank monitoring. In our model, such improvement causes a decrease in monitoring because there is less chance the firm needs to be liquidated early; perversely, this leads to more fraud. For the cross-derivatives, a decrease in monitoring costs has a greater impact on a firm's propensity to commit fraud when industry conditions are favorable (so that monitoring incentives are weak to begin with). However, the effects of monitoring costs on overall incidence of fraud do not vary with industry conditions.[4]

A critical empirical challenge for testing our model is that bank monitoring intensity and its costs are not easily observable and that monitoring intensity is endogenous. We take advantage of the unique institutional settings in China during 2000-2010, and exploit documented evidence that banking development in China varied significantly across provinces to begin with and then changed after the exogenous shock of bank liberalization.[5] Specifically, we use the sample of China Securities Regulatory Commission (CSRC) enforcement actions concerning financial misreporting of firms listed on mainland China's two stock exchanges (i.e., Shanghai Stock Exchange and Shenzhen Stock Exchange) from 2000 to 2010. First, we show that, during this time, there was a negative association between the level of provincial banking sector development and

[4] While the effects of monitoring costs on a (bad) firm's propensity to commit fraud are larger when industry conditions are favorable, there are fewer bad firms at those times. Because of the simple form of our model, the cross-derivative of overall incidence of fraud with respect to industry conditions and monitoring costs is exactly zero.

[5] As we discuss in Section 3, compared with developed economies, banking development in China during our sample period varied substantially across provinces and cross-region bank lending was relatively rare, leading to highly segmented banking markets. Additional advantages of studying the link between banking development and fraud in China include: the locations of Chinese firms (or even Chinese workers) were relatively exogenous; the same corporate and securities laws applied to all provinces; and all provinces were regulated by the CSRC.

the likelihood that firms headquartered in that province faced CSRC enforcement actions related to financial misreporting.[6] This finding is consistent with our model prediction that lower monitoring costs are associated with less misreporting fraud.

Next, we use the 2001-2006 banking liberalization of China's banking system as a natural experiment to see how the entry of foreign banks with superior monitoring functions affected the likelihood that corporate borrowers committed fraud. Before this liberalization, foreign banks were not allowed to conduct local-currency transactions anywhere in China; the liberalization opened up the cities of China to such transactions according to a staggered schedule that was relatively exogenous. This should have improved the monitoring of borrowers through two channels. First, at this time, foreign banks tended to have better monitoring incentives and capabilities and hence lower monitoring costs than Chinese banks (Berger, Hasan, and Zhou, 2010; Bailey, Huang, and Yang, 2012; Qian, Strahan, and Yang, 2015); second, the entry of foreign banks should have improved Chinese banks' monitoring incentives and capabilities through competitive pressure and technological spillover (Xu and Lin, 2007; Mao and Li, 2009; Xu, 2011). Thus, the staggered introduction of foreign bank entry gives us a well-identified test of the hypothesis that improved bank monitoring ability reduces the prevalence of fraud.[7]

Our analysis shows that, compared with firms located in adjacent non-liberalized cities with similar levels of ex-ante financial and economic development, firms located in cities that allowed foreign bank entry were subsequently significantly less likely to commit fraud. The decrease in fraud was greatest among firms that borrowed from foreign banks, but fraud also decreased for firms that borrowed exclusively from domestic banks, consistent with a spillover effect. Moreover, we find that the decline in fraud was greater for borrowers of local Chinese banks than for borrowers of national Chinese banks. This is consistent with local Chinese banks having been more

[6] Online Appendix A gives an overview of the CSRC's role in regulating Chinese securities markets during our sample period.

[7] For a number of reasons, China is a more suitable setup for the test of our theoretical model than is the U.S. First, most datasets on fraud in the U.S. do not go back to the 1980s (Karpoff, Lee, and Martin, 2008), which is when states in the U.S. began to remove restrictions on bank geographic expansions. The only exception, the SEC AAER (Accounting and Auditing Enforcement Releases) dataset, reports a relatively small number of fraud cases during that period, making it less likely to have enough interstate variation to exploit the shock. Moreover, mechanisms for preventing fraud were probably more advanced in the U.S. even in the 1980s than they were in some provinces of China in the 2000s due to longer reliance on stock market financing and regulation by the SEC. Heath et al. (2020) also argue that repeated usage of the same natural experiment across many outcome variables would lead to false positives. Since banking deregulation in the U.S. has already been reused extensively as an experimental setting, studying banking liberalization in China should alleviate this inference problem.

exposed to competitive pressure from foreign banks than the local offices of national Chinese banks were, so that local banks undertook greater improvements in their monitoring than the local offices of national banks did.

We also explore how the composition of a firm's assets affected the decrease in fraud after liberalization. We find that this decrease was greatest for firms with relatively more tangible assets and for firms with more fixed assets in particular; by contrast, it was smaller for firms with relatively more working capital. Since working capital tends to lose more of its value during financial distress than fixed assets do, these results are consistent with our model's predictions.

Finally, we examine how industry prospects affected the decrease in fraud after liberalization. We find that firms in industries with above-average growth prospects experienced a bigger reduction in fraud after liberalization. This particular result differs from the strict interpretation of our simple theoretical model, where the effects of liberalization on overall incidence of fraud do not vary with industry conditions. However, several realistic complications that our model omits are consistent with this finding. We return to this issue in Section 3.3.

Because our difference-in-difference tests depend heavily on the assumption of parallel trends before liberalization, we use a battery of tests to show that this assumption is warranted. Our results are also robust to the inclusion of various firm characteristics, ownership structure, governance mechanisms, and political connection measures. As in the U.S., we find that misreporting fraud was less prevalent for firms that were less levered, more profitable, or had higher sales growth. Furthermore, larger private or state share ownership reduced the prevalence of financial misreporting fraud. In all cases, the coefficients on banking development level or bank liberalization indicator remain highly significant, suggesting that the effects of bank monitoring were not due to spurious correlation with observable firm characteristics or political connections.

Our paper lies at the intersection of four related streams of research: work on bank monitoring, on bank liberalization, on corporate securities fraud, and on financial development. Although there is a large literature on bank monitoring (e.g., Diamond, 1984; Rajan, 1992; Rajan and Winton, 1995), we are not aware of any papers that share our focus by examining the relationship between bank monitoring and corporate fraud. Perhaps the closest paper in this literature to ours is that of Graham, Li and Qiu (2008). Using a sample of U.S. firms, they find that loans initiated after restatement have harsher contract terms, particularly for fraudulent restatement, and fewer lenders per loan—i.e., corporate fraud affects subsequent bank lending.[8] Our paper complements this by establishing causality in the opposite direction—we show that bank lending affects fraud.

Our paper is also part of the literature on banking liberalization. Dell'Ariccia and Marquez (2004) model the tradeoffs between two forces: foreign banks are likely to be larger, more technically advanced, and better-diversified, giving them a funding advantage over domestic banks, but domestic banks are likely to have better information about the quality of smaller local firms they are already lending to. In equilibrium, domestic banks end up "capturing" those of their existing borrowers that are riskier, while foreign banks capture safer and more transparent borrowers, as well as newer borrowers for whom the domestic banks do not have inside information. These results are consistent with Berger, Klapper, and Udell's (2001) findings in their empirical analysis of lending by domestic and foreign banks in Argentina following banking liberalization. By contrast from these studies, we show that, when foreign banks have better monitoring incentives and technologies than local banks, banking liberalization can lead to improved monitoring by local banks. Together with the superior monitoring of foreign banks, this competitive spillover can lead to an overall reduction of corporate fraud.[9]

Our work is also part of the growing literature on corporate securities fraud. Previous studies on such fraud in the U.S. (e.g., Yu and Yu, 2011; Wang, 2013; Wang, Winton and Yu, 2010) or in China (e.g., Chen, Firth, Gao, and Rui, 2005, 2006; Yiu, Xu, and Wan, 2014; Cumming, Leung, and Rui, 2015) do not investigate the role of bank monitoring and banking development. To our knowledge, we are the first to model the role of banks in curbing corporate fraud and to use the

[8] Financial reporting quality in general has been shown to drive bank lending. For example, Costello and Wittenberg-Moerman (2011) examine how the revelation of internal control weaknesses (ICWs) affect bank loan contracts with that borrower. After ICWs are disclosed, banks rely less on accounting-based covenants and more on security provisions and performance pricing.

[9] Our paper is also related to Gormley, Kim, and Martin (2011), who examine financial liberalization in India and find that timely loss recognition increases in areas where foreign banks enter. Our paper differs from their work in several key aspects. First, unlike our paper, Gormley, Kim, and Martin (2011) focus only on timely loss recognition rather than fraud. Given the negative externalities created by fraud, our findings suggest policy concerns that go beyond that of creating accounting data that is more useful to lenders. Second, since Gormley, Kim, and Martin (2011) do not have data on firm-bank relationships, their result is consistent with both foreign banks demanding better information and with domestic banks upping their game to compete with foreign lenders. By contrast, our data on firms borrowing exclusively from domestic banks allows us to isolate the spillover effect. Third, after liberalization there were no restrictions on where foreign banks could choose to establish new branches in India. Therefore, the presence of foreign banks in Gormley, Kim, and Martin (2011) may reflect unobserved borrower and city characteristics. In our paper, the Chinese government's policy of phasing in liberalization in successive cohorts of pilot cities helps alleviate this identification concern.

banking liberalization in China to test our model's causal predictions. This suggests that, in addition to the benefits from improved credit allocation and cost-efficiency, banking liberalization also yields benefits by reducing financial fraud, which in turn can improve trust in financial markets.

Finally, our paper is closely related to the financial development literature, which argues that banking development can alleviate agency and asymmetric information problems and promote firm growth (e.g., Demirguc-Kunt and Maksimovic, 1998; Rajan and Zingales, 1998; Beck, Demirguc-Kunt, and Maksimovic, 2005, 2008; Giannetti and Ongena, 2009; Ayyagari, Demirguc-Kunt, and Maksimovic, 2010). While these papers focus on firm growth and patterns of financing activities, we focus on corporate fraud, which is a direct consequence of agency and asymmetric information problems. Since the prevalence of fraud increases the cost of capital and restricts the availability of external financing (e.g., Murphy, Shrieves, and Tibbs, 2005; Karpoff, Lee, and Martin, 2008), our paper provides a new insight into the mechanisms through which financial development affects firm growth.

The rest of our paper is organized as follows. Section 2 sets out our theoretical model. Section 3 reviews background information on China's banking development and regulation during the period of our study, and develop the main hypotheses for our empirical work. Section 4 reviews our data sources and descriptive statistics. Section 5 establishes our baseline results of fraud, and Section 6 contains our analysis of China's bank liberalization as a natural experiment. Finally, Section 7 concludes.

2. Theoretical model of bank monitoring and financial misreporting

We now present a simple model of how bank monitoring choices interact with borrower incentives to commit financial misreporting ("fraud"). This gives several intuitive results which form the basis of our empirical work in later sections. After going through the model, we will discuss the related theoretical literature on corporate fraud and highlight our contribution.

2.1. Model framework

Our model has two sets of agents, banks and firms, and takes place over three dates, 0, 1, and 2. At Date 0, banks make unit loans to firms with a gross interest rate $R > 1$. For now, we take R as given; in Section 2.4 we will endogenize this. Banks also decide at Date 0 on the intensity of their credit monitoring process.

At Date 1, the firm turns out to either be good (type "g"), with probability θ, or bad (type "b"), with probability $1-\theta$. If allowed to continue operating to Date 2, firms of type i return X with probability q_i and C with probability $1-q_i$, where $X > 1 > C \geq 0$ and $q_g > q_b$. For simplicity, we assume that $q_g = 1$ and $q_b = 0$; relaxing this would not change our qualitative results but would complicate notation. Managers of firms that continue to operate through Date 2 receive a non-contractible control benefit $B > 0$.

At Date 1, several additional things happen. First, the manager of the firm privately learns whether the firm is good or bad.[10] Second, the bank receives a public noisy signal s which is either high (h) or low (ℓ). In the absence of fraud, this free signal has the following conditional probabilities:

$$\Pr[s = h|i = b] \equiv \beta < \Pr[s = h|i = g] \equiv \gamma.$$

For simplicity, we will assume $\gamma = 1$; i.e., good firms never send low signals.

Critically, we assume that managers observe what the public signal will be before it is released, and that, if it is low (ℓ), they can commit fraud, which "flips" the signal to high (h). As we will see, managers of bad firms may wish to do this so as to avoid liquidation and receive their control benefit B.

After the public signal is released, the bank that lent to the firm can decide whether to demand payment on the loan or let the firm continue operations. Because the firm is assumed to have no cash on hand, demanding repayment causes the firm to be liquidated for an amount L, where we assume $C < L < 1$; since L is less than the promised payment R, the bank gets all the proceeds from liquidation. The bank will make this decision so as to maximize its total repayment conditional on whatever information it has at Date 1.

We note here that, while we have defined L as the value of the firm in actual liquidation, L could reflect the value of the firm once a more conservative strategy is forced upon it by the threat

[10] We could assume that managers are not perfectly informed about the firm's type; what matters most is that they have better information than the public signal s. The consequences of having managers' information be noisy but more precise than s are similar to assuming that bad firms have a positive chance of success ($q_b > 0$).

of actual liquidation. This conflict, where lenders prefer to force the firm to pursue conservative "safety" activities as opposed to riskier but higher upside "growth" activities, is first studied by Berlin and Mester (1992) in their theoretical analysis of loan covenants and renegotiation.

Because we have fixed the size of the firm's investment at 1 unit, we can also interpret L and C as proportions of total assets recovered if the firm is liquidated preemptively or allowed to fail, respectively. We return to this interpretation in our hypothesis development in Section 3.3 below.

As noted above, at Date 0 the bank creates its credit monitoring system. We index the intensity of the monitoring system by $\mu \in [0,1]$, where μ is the probability with which the bank spends effort $m > 0$ and monitors the firm. Monitoring reveals the firm's type i at Date 1 without noise. Again, we assume μ is chosen ex ante, and thus cannot be contingent on the realization of the public signal. Assuming that the bank chooses the monitoring probability in advance is a shorthand for a model where such a system requires some upfront cost and results in organizational procedures that are difficult to change ex post.[11]

Finally, we impose several assumptions on various parameters:

Assumption 1. $\theta X + (1-\theta) C < 1$.

Assumption 2. $\theta X + (1-\theta) L - m > 1$.

Assumption 1 implies that ignoring signals or monitoring and letting the firm continue its project unconditionally does not let the bank break even on its loan. Assumption 2 implies that bank loans are feasible if the bank monitors with sufficiently high probability. Finally, Assumptions 1 and 2 imply that $m < (1-\theta)(L-C)$, which means the marginal cost of monitoring more intensively is less than potential gain to liquidating bad firms at Date 1, which guarantees that there is some benefit to monitoring in equilibrium.

2.2. Firm manager's decision

The manager's decision comes at Date 1, when she must decide whether or not to commit fraud after seeing a low signal. Note that, because we have assumed good firms always send high signals ($\gamma = 1$), a low signal is a clear sign that the firm is bad. Because $C < L$, the bank prefers liquidation to continuation, and will always liquidate after a low signal is received. For simplicity,

[11] Not allowing the decision to monitor to be affected by the public signal realization simplifies analysis but does not change the main thrust of our results.

we assume that, in equilibrium, the bank will not liquidate the firm if the public signal is high, and the bank has not monitored the firm.

We assume that a given firm j that commits fraud is caught later with probability $\lambda\phi_j$, where λ is an exogenous parameter between 0 and 1 that reflects the efficacy of the legal enforcement system, and ϕ_j is a firm-specific variable distributed uniformly over the unit interval.[12] Managers caught committing fraud receive an exogenous penalty f.

Note that we do not incorporate the bank's monitoring into the chance that fraud is caught and punished. There are several reasons for taking this approach. For one thing, a bank that monitors a firm's business situation and decides the firm is weaker than its financial reports suggest does not know whether this discrepancy is due to fraud or instead to the inherent gap between historical accounting values and economic reality. For another, even if a bank does suspect fraud, it may not be able to prove this to the standards required for legal judgments without further work—work that does not help the bank's bottom line. Finally, banks may not wish to allege fraud because it may harm current or future business relationships. As noted above, what we have called "liquidation" could simply be a more conservative action the firm is forced to follow; this may leave the firm as a customer of the bank either for future loans or other banking products, and the bank may not wish to disrupt such future business opportunities.

Suppose firm j is bad, receives a low public signal, and commits fraud. With probability μ, the firm is monitored by the bank, which sees that it is bad and liquidates it. With probability $1-\mu$, the firm is not monitored, it sends a high signal, and (by assumption) is allowed to continue, giving the firm's manager a control benefit of B. In either case, the manager is later found to have committed fraud with probability $\lambda\phi_j$.

It follows that, for bad firms, the expected benefit from committing fraud is $(1-\mu)$ B, while the expected cost is $\lambda\phi_j f$. This implies that managers of bad firms with low public signals commit fraud if and only if $\lambda\phi_j f \leq (1-\mu)$ B, or, equivalently $\phi_j \leq (1-\mu)$ B/(λf). We impose the following assumption:

Assumption 3. B $\leq \lambda$f.

[12] Having the probability of being caught depend on an exogenous firm-specific parameter ϕ_j is not essential; if the probability of being caught was a flat λ for all firms, then in equilibrium managers would choose mixed strategies for committing fraud, but analysis would be similar. Our assumption allows us to avoid discussion of mixing in equilibrium—see Harsanyi (1968).

Assumption 3 implies $B/\lambda f \leq 1$, so that managers weakly prefer not to commit fraud if their firm's ϕ_j is sufficiently high. It follows that there is some value of $\phi(\mu)$ between 0 and 1 such that $\phi(\mu) = (1-\mu)\ B/(\lambda f)$. Since all managers with ϕ_j less than this value commit fraud and ϕ_j is distributed uniformly on [0,1], it follows that $\phi(\mu)$ is the fraction of firms that commit fraud after seeing that the public signal would otherwise be low.

2.3. Bank's decision

To explore how banks choose their level of monitoring intensity, we work backwards from Date 1. Suppose the bank has chosen monitoring with intensity μ. If it does monitor a given firm, it expends effort m and is perfectly informed, so it lets good firms continue and liquidates bad firms. Its total profit (net of the initial loan amount 1) is $\theta R + (1-\theta)\ L - m - 1$.

If the bank does not monitor the firm, then it must rely on the public signal. Good firms always send a high signal, as do bad firms who either naturally send a high signal or who get a low signal but then commit fraud. Bad firms that get low signals and do not commit fraud are liquidated. Assuming that the bank allows firms with high signals to continue, its expected profits are

$$\theta R + (1-\theta)\ \beta C + (1-\theta)\ (1-\beta)\phi(\mu)C + (1-\theta)(1-\beta)(1-\phi(\mu))L - 1$$

$$= \theta R + (1-\theta)\ L - (1-\theta)\ [\beta + (1-\beta)\ \phi(\mu)](L-C) - 1. \qquad \textbf{(1)}$$

Note that the probability that a bad firm sends a high signal is $\beta + (1-\beta)\ \phi(\mu)$. Thus, the bank's expected profits from relying on the public signal are the fully informed profits $\theta R + (1-\theta)\ L - 1$ less $\beta + (1-\beta)\phi(\mu)$ times the opportunity loss L–C from letting bad firms continue operating to Date 2 rather than liquidating them at Date 1.

It follows that a bank that monitors with probability μ has expected profits

$$\Pi = \theta R + (1-\theta)\ L - (1-\mu)\ (1-\theta)\ [\beta + (1-\beta)\phi(\mu)](L-C) - 1 - \mu m, \qquad \textbf{(2)}$$

where again $\phi(\mu)$ equals $(1-\mu)\ B/\lambda f$.

The bank chooses μ to maximize Π. It is easy to show that Π is concave in μ, so, assuming we have an interior maximum, the optimal μ satisfies the following first order condition:

$$0 = -m + (1-\theta)\,(L-C)\,[\beta + 2(1-\beta)\phi(\mu)] \tag{3}$$

Here, m is the marginal cost of monitoring more intensively (increasing μ), and the second term is the marginal benefit from increasing μ and making more accurate liquidation v. continuation decisions. The benefit is multiplied by $\beta + 2(1-\beta)\,\phi(\mu)$ rather than $\beta + (1-\beta)\phi(\mu)$ because increasing μ reduces the likelihood that a manager commits fraud, reducing the overall number of incorrect high signals as well as correctly dealing with any bad firm that is monitored. Solving this FOC for μ, we have

$$\mu = 1 - \left[\frac{m}{(1-\theta)(L-C)} - \beta\right] \cdot \frac{\lambda f}{2(1-\beta)B}. \tag{4}$$

With this in hand, we now have the following proposition (all proofs are in Appendix A):

Proposition 1 (Monitoring and fraud choices.) (a) If $m/(1-\theta)\,(L-C) \leq \beta$, then the bank chooses to monitor as intensively as possible ($\mu = 1$) and there is no fraud ($\phi = 0$).

(b) If $\beta < m/(1-\theta)\,(L-C) < \beta + 2(1-\beta)\,B/\lambda f$, then the bank chooses an interior level of monitoring μ as given by Equation (4). The incidence of fraud ϕ satisfies $\phi = \phi(\mu) = (1-\mu)B/\lambda f$.

(c) If $m/(1-\theta)\,(L-C) \geq \beta + 2(1-\beta)\,B/\lambda f$, then the bank does not monitor at all ($\mu = 0$) and the incidence of fraud is maximized at $\phi = B/\lambda f < 1$.

Our next result explores the comparative statics of monitoring intensity μ and the incidence of fraud among bad firms, ϕ. Since monitoring and fraud are constant in the regions defined by Proposition 1(a) and 1(c), we focus on the case of Proposition 1(b), where the bank chooses an interior monitoring level.

Corollary 1. When Proposition 1(b) holds, then

(a) The bank's monitoring intensity μ is increasing in the base chance β that bad firms send high signals, the manager's private continuation benefit B, and the firm's liquidation value (as a proportion of total assets) L. It is decreasing in the cost of monitoring m, the probability θ that the firm is good, and the firm's worst-case value (as a proportion of total assets) C. It is not affected by the gross interest rate R.

(b) The incidence of fraud ϕ is increasing in m, θ, and C. It is decreasing in β and L, and is unaffected by changes in B or R.

(c) The negative impact of higher monitoring cost m on monitoring intensity μ is smaller in magnitude as the firm's liquidation value L is larger. It is larger in magnitude as the firm's worst-case value C or the probability the firm is good θ is larger.

(d) The positive impact of higher monitoring cost m on the incidence of fraud ϕ is smaller in magnitude as the firm's liquidation value L is larger. It is larger in magnitude as the firm's worst-case value C or the probability that the firm is good θ is larger.

The results in part (a) of Corollary 1 follow directly from Equation (4) and are largely intuitive. If bad firms are more likely to send high signals even without fraud (i.e., the public signal is less reliable), the bank has more incentive to monitor firms and find out their type directly. Higher private benefits increase the manager's willingness to commit fraud, which reduces the reliability of the public signal, encouraging bank monitoring. Raising the firm's Date 1 liquidation value L increases the incentive to monitor so as to make better-informed liquidation v. continuation decisions; raising the bank's minimum Date-2 value C decreases this incentive. Increasing the cost of monitoring m makes monitoring less attractive, as does lowering the probability that firms end up being bad (*increasing* θ). Because we assume that the bank's net gain to early liquidation L−C is independent of the loan rate R, changing R does not affect the bank's incentive to monitor.[13]

Because the probability $\phi(\mu)$ that a bad firm faced with a low public signal commits fraud is decreasing in μ, the effects of parameter changes on the incidence of fraud for bad firms clearly have the opposite sign from their effects on monitoring intensity.

Parts (c) and (d) of the corollary look at cross-derivatives[14] that speak to how the effects of a change in monitoring cost vary across different firms. While it is true that an increase in monitoring

[13] If bad firms had a chance of success $q_b > 0$, then changes in R would affect the bank's possible return from letting bad firms continue. In this case, raising R would reduce the bank's incentive to monitor. As q_b falls to zero, however, this effect on monitoring incentives would fall to zero as well.

[14] We note that predictions involving cross derivatives offer more rigorous tests than first-order derivative predictions. In reality, the correlations between fraud and empirical proxies of L, C, and θ might come from many channels besides bank monitoring. However, only the component related to bank monitoring would vary with banking liberalization.

costs always decreases equilibrium monitoring and increases the incidence of fraud for bad firms, these effects are muted for firms with higher liquidation value L and amplified for firms with higher worst-case value C. Intuitively, firms with higher liquidation value or lower worst-case value are precisely the kind of firms where bank monitoring incentives are strongest (the net value of liquidating bad firms, $L-C$, is greater), so a change in the cost of monitoring has a lesser impact. The opposite is true for firms where liquidation value is lower or worst-case value is higher. Similarly, changes in monitoring cost matter more for monitoring choice and fraud commission as the probability θ that the firm is good increases. When θ is low, the bank has incentive to monitor intensively regardless of m, because most firms are bad and thus more likely to commit fraud; when θ increases, the bank's monitoring incentive declines, and changes in monitoring cost have a bigger impact on monitoring intensity.

We now turn our analysis to the incidence of fraud among all firms, good and bad. This overall incidence Φ is the joint probability that the firm turns out to be bad, generates a low public signal, and then has the manager commit fraud to flip the signal to a high one: that is, $\Phi = (1-\theta)(1-\beta)\phi(\mu)$. Making use of Proposition 1 and Corollary 1, we have the following result.

Corollary 2. (a) If $m/(1-\theta)(L-C) \leq \beta$, then the overall incidence of fraud is $\Phi = 0$.

(b) If $\beta < m/(1-\theta)(L-C) < \beta + 2(1-\beta)B/\lambda f$, then $\Phi = [m/(L-C) - \beta(1-\theta)]$.

(c) If $m/(1-\theta)(L-C) \geq \beta + 2(1-\beta)B/\lambda f$, then $\Phi = (1-\theta)(1-\beta)B/\lambda f$.

(d) In case (b), (i) Φ is increasing in monitoring cost m, in the probability θ that the firm is good, and in the firm's worst-case value (as a proportion of total assets) C. (ii) Φ is decreasing in the probability β that a bad firm sends a high signal and in the firm's liquidation value (as a proportion of total assets) L. (iii) The positive impact of higher monitoring cost m on Φ is smaller in magnitude as L is larger and larger in magnitude as C is larger, and it is *not* affected by θ.

(e) In case (c), Φ is *decreasing* in θ. It is also decreasing in β. It is not affected by m, L, or C.

Parts (a) through (c) are immediate consequences of Proposition 1 and the definition of Φ. In the case of an interior monitoring choice (part (b)), most of the comparative statics for Φ are identical to those for ϕ, since Φ is just the latter times $(1-\theta)(1-\beta)$. The only difference is that now the effect of changes in monitoring cost is independent of the probability that the firm is good. This accounts for the results in part (d) of the proposition.

When the bank does not monitor at all (part (c) of the proposition), most parameters have no effect on the overall incidence of fraud. The exceptions are θ and β; as these increase, the overall incidence of bad firms and their chance of getting a low signal both fall, decreasing the overall cases where fraud occurs. This accounts for part (e) of the proposition.

One final note is in order. If we look at the overall incidence of fraud as a function of the probability θ, for low values of θ there is no fraud; then, the incidence of fraud increases; and then, at very high levels of θ, it decreases. This hump-shaped pattern can also be found in the more complex model of Povel, Singh, and Winton (2007); Wang, Winton, and Yu find empirical evidence for this in their study of fraud in U.S. IPOs.

In what follows, we use the Chinese banking liberalization of 2001-2006 to test many of the results in our model. As we will discuss below, this setting slows us to find empirical proxies for a number of key parameters in our model, including the overall incidence of fraud Φ, bank monitoring costs m, the probability θ that the firm is good, and the firm's liquidation value L and worst-case value C. In particular, although we cannot observe monitoring costs directly, existing evidence suggests we can reasonably assume that monitoring costs of banks in China are lower in developed provinces, and that monitoring costs decreased after the exogenous shock of bank liberalization in China. Furthermore, following Rajan and Winton (1995), we can interpret L and C as reflecting different types of collateral: C is collateral (such as land or other fixed assets) that loses little value in financial distress, whereas L−C represents collateral (such as accounts receivable or inventories) that is significantly impaired by financial distress. We therefore measure C (L−C) using a firm's fixed assets (working capital) as a proportion of total assets. Finally, we can use measures of industry growth prospects as proxies for the relative number of good firms in an industry.

2.4. Results when lending rates are endogenous

Up to now, we have taken the gross lending rate R on loans as given.[15] To incorporate competition in a simple way, we assume that, given characteristics of each pool of similar

[15] Although this may not have been totally off in the days before China began liberalizing bank lending rates—until 2007, there were ceilings and floors on bank loan rates, with ceilings being removed in 2007 and floors in 2012—the

borrowers (θ, L, etc.), the bank first sets its loan rate R and *then* sets its monitoring intensity μ so that its expected profits Π are equal to zero. More formally, we assume the following:

(1) Given R, the bank sets monitoring intensity μ so that profits are maximized. We continue to assume that we are in the case of an interior maximum, so that μ is determined by Equation (4).

(2) Given the monitoring function choice μ, the bank sets R such that $\Pi(R,\mu) = 0$.

With this new framework in hand, we have the following result:

Corollary 3 (Comparative Statics for Competitive Loan Rates). When R is set so that $\Pi = 0$, all the comparative statics given in Corollaries 1 and 2 continue to hold.

The intuition for this result is simple: in the framework we have outlined, the loan rate R does not directly affect the optimal choice of monitoring intensity μ, though the reverse is not true. (Changing μ affects bank profits, affecting the breakeven value of R.) As a result, all the comparative statics in the case of R exogenous continue to hold when R is determined by competitive pressure.

2.5. Discussion of related theoretical literature on fraud

There is a very limited theoretical literature on financial fraud and monitoring by outside financiers such as banks or shareholders. Work by Povel, Singh, and Winton (2007) examines how monitoring by outside shareholders influences firm managers' incentives to commit fraud. In their model, potential shareholders can monitor firms at a cost; critically, such monitoring does not seek to detect fraud per se, but instead seeks to learn the firm's true prospects. Nevertheless, more intensive monitoring may discourage fraud: because fraud aims to convince investors to finance poor-quality firms, it loses its value if investors will still figure out that the firm's quality is poor. Generally speaking, better industries conditions lead investors to take good results at face value, which can exacerbate managers' incentives to commit fraud.

While Povel, Singh, and Winton's model does look at the interaction between monitoring and misreporting, it has critical limitations as a model of monitoring by bank lenders. First, they focus on monitoring that occurs *before* investors put money into the firm; by contrast, evidence suggests

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advent of foreign banks should have made rates more subject to competition, and thus more responsive to borrower risks and returns.

that monitoring *after* a bank first lends to a borrower is the more critical bank activity in corporate lending.[16] Thus, a useful model of bank lending and monitoring should incorporate a bank's decision whether to continue or seek to liquidate existing loans. Second, they assume that the investor captures all cash flows that the firm produces, whereas banks typically hold debt and do not capture all of a borrowing firm's cash flows. Finally, they do not examine the role of collateral or borrower asset structure that is critical to bank lenders.

As stated earlier, to our knowledge, we are the first to model how bank monitoring can discourage corporate fraud. Our model captures the intuition that banks do not have to focus on detecting fraud to make this happen; improvements in monitoring efficiency can indirectly deter fraud, even when the monitoring focuses on other indicators of firm health rather than financial reports alone. Lenders are profit maximizers, not regulators. From the manager's point of view, this model says that bank monitoring reduces financial fraud not by increasing the odds of detection but by decreasing the odds that the fraud produces a benefit. As detailed in the accounting literature, financial fraud is often aimed at securing access to new or continued funding at artificially good terms. Our focus on hiding near-bankrupt status is an extreme example; in this case, *any* funding is being provided on artificially good terms.

3. Background information on China and hypothesis development

In this section, we review background information on China's banking development and regulation during the period of our study. Next, we apply the theoretical framework in Section 2 to the institutional settings in China, and develop our main hypotheses.

3.1. Banking development across provinces in China

Unlike cross-country studies where countries differ on both financial development and legal systems, China uses a single nationwide investor protection law; however, during the time of our study (2000 to 2010), banking sector development varied substantially across various provinces (Fan, Wang, and Zhu, 2011). In comparison to developed countries, cross-region bank lending was rare during this period, and the People's Bank of China (the central bank) enforced a loan quota

[16] See for example Lummer and McConnell (1989).

system in which the ceiling of total credits in each province was determined annually, resulting in severely segmented banking markets.

The location of Chinese firms was also less endogenous than many developed economies due to the red tape for firm registration and approval and labor market segmentation brought about by the Hukou system. This system links an individual's social benefits, education, and employment opportunities to his/her residence, and change is only allowed through government approval under very limited circumstances such as a marriage or an application filed by a prestigious state-owned employer immediately after college graduation. Overall, the segmentation of Chinese financial and labor markets led to the unique nature of differences in banking sector development across provinces in China during the time of our study.

3.2. Banking liberalization in 2001

After joining the WTO in December 2001, the Chinese government implemented a series of policies to fulfill its commitments on banking sector liberalization. At the end of 2001, qualified foreign banks registered in one of four pilot cities (Shanghai, Shenzhen, Tianjin, and Dalian) were allowed to conduct RMB transactions in that city. This process continued with five more cities in December 2002, four more in December 2003, five more in December 2004, and seven more in December 2005.[17] Finally, in December 2006, all remaining geographic and clientele restrictions on foreign banks' RMB business were eliminated. Due to the gradual nature of this liberalization process, exposure to foreign banks varied across cities and across time. The pilot cities in each cohort were geographically diverse (drawn across coastal, central, and western regions) and were chosen by the central government, making this liberalization a suitable framework for our natural experiment. Moreover, subsequent studies show that foreign banks had significant presence in China after banking liberalization (e.g., Xu and Lin, 2007; Xu, 2011); in our empirical analysis, we confirm this and show that foreign bank penetration was concentrated in the liberalized cities.

As mentioned earlier, there are two reasons for thinking that banking liberalization would improve the quality of bank monitoring. First, research has shown that foreign banks tended to be more profit-oriented than Chinese state-owned banks, giving them better incentives to monitor

[17] In order, the specific cities were Guangzhou, Zhuhai, Qingdao, Nanjing, and Wuhan in December 2002, Jinan, Fuzhou, Chengdu, and Chongqing in 2003, Kunming, Beijing, Xiamen, Xian, and Shenyang in 2004, and Shantou, Ningbo, Harbin, Changchun, Lanzhou, Yinchuan, and Nanning in 2005.

firms that borrowed from them. (See Xu and Lin (2007) for evidence that foreign banks were more profitable than Chinese banks and see Bailey, Huang, and Yang (2012). Cao et al. (2018), and Qian, Strahan, and Yang (2015) for a description of incentive problems associated with Chinese banks.) Second, research has shown that foreign bank entry was associated with an increase in banking competition and efficiency, a higher degree of financial development, technological spillover to domestic banks, and improved access to credit for loan customers (e.g., Xu, 2011; Lin, 2011; Qian, Strahan, and Yang, 2015). The entry of foreign banks in China, therefore, should have increased monitoring by foreign banks and improved monitoring incentives and capabilities of Chinese banks through competitive pressure and technological spillover.

3.3. Hypothesis development

In terms of empirical implications, our model clearly predicts that a decrease in monitoring costs should reduce the incidence of fraud, all else equal. Because Chinese banks had poor internal lending procedures and incentives, foreign banks were expected to bring a new monitoring technology—one with lower effective monitoring costs. Knowing this, during the run-up to actual liberalization, domestic banks invested heavily in improving their internal lending incentives and credit review procedures. For these reasons, we expect that liberalization in a given city should have led to lower fraud among bank borrowers, though arguably with a greater drop for borrowers at foreign banks than for borrowers at domestic banks.

The model prediction above differs from the existing theoretical literature on how bank liberalization and subsequent entry by foreign banks affect the efficiency and composition of bank lending in the liberalized country. In particular, the model of Dell'Ariccia and Marquez (2004) assumes that foreign banks are not good at monitoring, but have a lower cost of funds than incumbent domestic banks do. As a result, foreign banks "skim the cream", targeting firms that are relatively transparent or very good credit quality. This suggests that there should be little overall drop in fraud after foreign bank entry; instead, borrowers from foreign banks, being more transparent, already have lower fraud rates, whereas the borrowers who remain with domestic banks are more opaque and naturally have higher fraud rates to begin with.[18]

[18] Dell'Ariccia and Marquez's model has several key limitations when applied to the institutional settings in China. First, they assume that both domestic and foreign banks have the same incentives, and focus exclusively on the information advantage a domestic bank has in lending to its existing borrowers. Second, they assume bank capabilities and incentives are static. As we have noted, these assumptions do not fit the situation in China during bank

Our model's results on cross-interactions suggest an additional hypothesis. Specifically, we predict that any decrease in monitoring costs m brought about by bank liberalization would have stronger effects on firms where proportional liquidation value L is lower relative to worst-case proportional value C or where C is higher relative to L; i.e., firms with relatively low levels of working capital assets or firms with relatively high levels of fixed assets (where relative levels are measured as proportions of total assets).

Based on our discussion, and assuming that most observations fall into a region where bank monitoring choices are interior (Proposition 1(b)), we have the following empirical hypotheses:

Hypothesis 1. An increase in banking development reduces fraud.

Assuming that monitoring costs are lower in developed provinces, this prediction immediately follows from Corollary 1(b) and Corollary 2(d).

Hypothesis 2. Foreign bank entry reduces fraud among clients of foreign banks.

Assuming that monitoring cost is lower among foreign banks due to better incentives and technology, this prediction immediately follows from Corollary 1(b) and Corollary 2(d). Because foreign banks are only present after liberalization, before liberalization their clients were either unmonitored or monitored by domestic banks with higher monitoring costs. Either way, these clients should be monitored more heavily after liberalization, leading to a reduction in fraud.

Hypothesis 3. Foreign bank entry reduces fraud among clients of domestic banks, but this reduction should be smaller than that for clients of foreign banks.

liberalization: foreign banks probably had better incentives and monitoring experience to begin with, and domestic banks improved their incentives and abilities as foreign entry approached. A third limitation is that they do not allow for the possibility of fraud: borrowers do not represent or misrepresent their quality; instead, domestic banks are endowed with better information about their existing borrowers, but borrowers themselves are completely passive and simply take the best interest rate they are offered. Given that our focus is on whether bank monitoring ability discourages fraud, this is a major drawback.

Assuming that technological/competitive spillover lowers monitoring costs for domestic banks, the first part of this prediction immediately follows from Corollary 1(b) and Corollary 2(d). The second part follows if we assume that, despite the spillover effect of foreign bank entry, monitoring costs at domestic banks remain higher at domestic banks than at foreign banks.

Hypothesis 4. Foreign bank entry reduces fraud more for firms with higher proportions of fixed assets or lower proportions of working capital assets.

Assuming that fixed assets' proportion of total assets corresponds to C in the model and working capital assets' proportion of total value corresponds to L−C, this prediction immediately follows from Corollary 1(d) and Corollary 2(d). As discussed there, a decrease in monitoring costs such as that brought about by foreign bank entry should reduce fraud more for firms with higher proportions of fixed assets or lower proportions of working capital.

Hypothesis 5. Foreign bank entry reduces fraud by the same amount in industries that are booming and in industries with weaker prospects.

Assuming that the probability of a good firm, θ, is higher within a booming industry than within an industry with weaker prospects, Corollary 2(d) implies that a decrease in monitoring costs reduces the overall incidence of fraud, Φ, by the same amount regardless of industry prospects. However, our empirical tests condition on a number of firm-specific variables, so that what we measure as fraud is in fact somewhere between the completely unconditional incidence Φ and the incidence for bad firms only, which is ϕ. (From Corollary 1(d), foreign bank entry reduces ϕ more in booming industries where monitoring incentives are weak to begin with.) In addition, Corollary 2(d) relies on our very simple model and our focus on interior solutions, where the overall incidence of fraud turns out to be additively separable in monitoring cost and industry prospects. This is not the case in a more elaborate setup such as Povel, Singh, and Winton (2007), who find that a drop in monitoring costs reduces fraud for most industry conditions but may increase fraud for extremely high industry prospects. Taking these considerations into account, along with Corollary 1(d), we have the following modification of the previous hypothesis:

Hypothesis 5a. Foreign bank entry reduces fraud more for industries that are booming than for industries with weaker prospects.

4. Data sources and descriptive statistics

4.1. Data sources

Our initial sample consists of firms listed on mainland China's two stock exchanges (i.e., Shanghai Stock Exchange and Shenzhen Stock Exchange) from 2000 to 2010.[19] We retrieve the following information from China Stock Market and Accounting Research (CSMAR) database: (1) fraud characteristics such as information on the detection of financial misreporting; (2) firm characteristics, for example, firm size and leverage; (3) ownership structure; (4) other governance variables, such as CEO compensation and board characteristics; (5) loan-level data on the maturity, size, and creditor information on each bank loan borrowed by listed companies.

We use the National Economic Research Institute (NERI) index of banking industry development (Fan, Wang, and Zhu, 2011) to measure the banking sector development of different provinces in China during our sample period. For robustness, we collect province-level macroeconomic information such as total credit and number of lawyers and accountants per capita from China National and Provincial Bureau of Statistics. We construct a set of political connection variables by collecting the biographies of past and incumbent CEOs from firms' annual reports and then manually identifying whether a specific CEO has worked for the government or military, a state-owned company, or other government agencies such as the NPC (National People's Congress). In addition, we collect the birthplaces of past and incumbent CSRC chairs to examine whether a firm's headquarters are located at the incumbent CSRC chairperson or vice chairperson's birth city (i.e., *CSRC Chair Connected*). We also collect China's Five-Year Plans for National Economic and Social Development during our sample period from the government's website to determine whether a firm was then operating in a government-supported industry (i.e., *Government-Supported Industries*). The number of observations varies across regressions due to data availability of the required variables.

[19] A relatively small number of firms (i.e., less than 1% of all firms) was subject to CSRC enforcement actions during the 1990s as the CSRC went through several rounds of structural reforms after it was established in 1992. Our sample therefore starts in 2000, when the regulatory body and framework stabilized. (Specifically, the Securities Law became effective in July 1999).

4.2. Variable construction

The key dependent variable of this study is *Fraud Indicator,* an indicator variable that equals one if a firm is subject to a CSRC enforcement action due to financial misreporting in a specific year, and zero otherwise. We construct this key dependent variable from the CSRC enforcement action dataset compiled by the CSMAR database.

The following serve as the explanatory variables in our study. We provide detailed definitions of all variables in Online Appendix B.

(1) *Banking Development* is the NERI index of banking industry development by Fan, Wang, and Zhu (2011). The authors constructed this index for each year from 1997 on to assess banking development in each province of mainland China. The index is based on two factors: banking industry competition as measured by the percentage of deposits held by non-state-owned banks, and banking market efficiency as measured by the percentage of credits allocated to non-state-owned enterprises.[20]

(2) Firm characteristics: *Size* is measured as the logged value of total assets; *Leverage* is measured as total liabilities divided by total assets; *Return on Assets (ROA)* is measured as earnings before interest and tax (EBIT) divided by total assets; *Sales Growth* is the percentage change in net sales from last year; *Stock Return* is annual stock return; and *Stock Turnover* is measured as annual trading value divided by market capitalization.

(3) Ownership Structure: *Largest Shareholder* is measured as the percentage of outstanding shares held by the largest shareholder, and *State Ownership* is measured as the percentage of outstanding shares held by the state. As a robustness check, we control for *Foreign Ownership*, which is the percentage of outstanding shares held by foreign individuals or entities.

(4) Other governance mechanisms: We control for the following governance mechanisms in our robustness tests: *CEO Ownership* is measured as the percentage of shares held by the CEO; *CEO Compensation* is CEO annual compensation; *CEO Duality* is an indicator variable that equals one if the CEO is also the chairman of the board; *Independent Board* is measured as percentage of independent directors on the board; *Board Size* is the total number of directors on the board; and

[20] The NERI index of banking industry development used in our study is a subset of the NERI index of marketization used in Jiang, Lee, and Yue (2010), Liu and Siu (2011), and Li et al. (2011). Specifically, the overall marketization index includes other categories such as development of legal environment, market intermediaries, and non-state economies.

Foreign Auditor is an indicator variable that equals one if a firm hires an auditing firm that is registered or headquartered outside mainland China.

(5) Factors affecting fraud detections: We construct the unexpected ex-post detection variables for the bivariate probit model as follows. We define *Industry Litigation* as the logarithm of the total market value of firms that are subject to CSRC enforcement actions in a specific industry and year. *Abnormal Industry Litigation* is the yearly deviation from the average value of *Industry Litigation*. *Disastrous Stock Return* is an indicator variable that equals one if annual stock return is in the bottom decile of the sample distribution (i.e., < −45%), and zero otherwise. *Abnormal Stock Turnover* is defined as the deviation from the average stock turnover for a specific firm.

4.3. Descriptive statistics

Table 1 Panel A presents the distribution of fraudulent (misreporting) firms by year. Over the entire sample period, the probability of a firm being subject to a CSRC enforcement action due to financial misreporting was 3.65%—a rate at least as great as that in the U.S.[21] We include year fixed effects in our main regressions to control for any potential time trend in CSRC enforcement actions.

Insert Table 1 here

Online Appendix Table 1 presents the distribution of fraudulent firms by banking development. We follow the "Seventh Five-Year Plan" adopted by the Sixth National People's Congress (1986) to group the provinces in China into three regions during our sample period: the most developed eastern and coastal region, the less developed central region, and the least developed western region. We find that fraud tended to be less prevalent in financially developed provinces. For example, the percentage of firms investigated for fraudulent activity in our sample is 2.94% for provinces in the eastern and coastal region (e.g., Shandong, Zhejiang, and Guangdong) with an average banking development score of 8.34 during this time. In comparison, the fraud likelihood is 4.26% in the central region (e.g., Hubei, Heilongjiang, and Hunan) with an average banking development score of 6.25, and 5.24% in the western region (e.g., Gansu and

[21] In a sample of 15,117 observations of U.S. firms, Wang (2013) documents 406 cases of corporate fraud, or 2.7%. However, Wang's sample includes private securities class action lawsuits as well as SEC enforcement actions. SEC enforcement actions are only roughly one-third of fraud cases in her sample.

Ningxia) with an average banking development score of 5.7.[22] Overall, during our sample period, fraud propensity decreases with banking development.

Table 1 Panel B presents summary statistics for our main variables. We winsorize all firm-level variables at the 1% and 99% level to mitigate the effects of outliers. An average firm in our sample is headquartered in a province with a banking development index of 8.42 and has log value of total assets of 21.27,[23] leverage of 50.65%, ROA of 4.82%, sales growth of 3.41%, stock return of 36.72%, and stock turnover of 2.66. These sample characteristics are in line with contemporaneous studies of Chinese firms, such as Chen, Chen, Schipper, Xu, and Xue (2012).

Next, we turn to shareholder ownership. On average, the single largest shareholder holds 37.8% of the company, and state owners hold 26.2% of the shares, respectively. By construction, 10% of our sample observations have disastrous annual stock returns. For an average firm in our sample, the deviation from average stock turnover is zero, and the deviation from average logged market value of CSRC litigated firms in an industry is 0.11.

In Panel C of Table 1, we compare the characteristics of fraudulent versus non-fraudulent firms one year before fraud detection, and carry out two-tailed *t*-tests for testing differences in sample means. Fraudulent firms are more likely to be headquartered in a province with a less developed banking sector. They are significantly smaller, are more highly levered, and have lower profitability and sales growth. Fraudulent firms also have lower stock returns and higher turnover prior to fraud detection than non-fraudulent firms do. In addition, they tend to have smaller block and state shareholdings, and are subject to higher industry litigation and more likely to have disastrous stock return and high abnormal turnover prior to fraud detection than non-fraudulent firms.

5. Baseline results on banking development and misreporting

In this section, we examine how provincial banking development affects fraud propensity. We estimate the following probit regression:

[22] Kedia and Rajgopal (2011) find that U.S. firms located close to SEC offices are less likely to commit fraud. We note that our banking development measure is not highly correlated with the distance from the headquarters of CSRC in Beijing. Provinces with well-developed banking markets during our sample period are generally scattered along the coastlines or navigable waters. For example, financially developed Guangdong province is further from Beijing than less developed Henan province. We also control for the distance from Beijing in the baseline regression and find that our banking development results are robust.

[23] This corresponds to 1.72 billion RMB of total assets.

Probability (Fraud $\text{Indicator}_{i,t} = 1$) = $b_0 + b_1$ Banking Development $_{i,t-1}$ + B_2 Firm Characteristics $_{i,t-1}$ + B_3 Ownership Structure $_{i,t-1}$ + B_4 Industry and Year Dummies + $e_{i,t}$ (5)

where B_1 and B_2 denote a vector of coefficients. We measure our main dependent variable, *Fraud Indicator*, at the detection year and explanatory variables at one year before detection. The specification of model (5) is based on the timing of fraud detection because each CSRC enforcement action report has precise information on the detection year but may not include a clear statement on the commission year for each type of fraudulent activity when multiple types of fraudulent activities (for example, financial misreporting and tunneling) are detected. Furthermore, the median detection period between fraud commission and detection in our sample is one year—shorter than the average three-year detection period documented by Wang (2013) based on U.S. data.[24] For robustness checks, we obtain similar results when using two alternative specifications: (1) measuring *Fraud indicator* still at the detection year but explanatory variables at two years before the detection, and (2) measuring *Fraud indicator* at the first year of fraud commitment[25] and explanatory variables at one year before fraud commitment.[26] We adopt the same empirical strategy regarding the time window of dependent and independent variables throughout the paper.

Table 2 displays the results of these regressions. First, we find that fraud is less likely to occur when firms are in provinces with well-developed banking sectors: the coefficients on the banking development index are negative and statistically significant in all columns. This negative relation persists when we examine the size of the banking sector using provincial total credit divided by GDP for robustness, although we do not find a significant relation between the size of stock market and fraud reduction.[27]

Insert Table 2 here

[24] We describe our sample by detection period in Table 2 of the online appendix. Wang (2013) documents an average three-year detection period of U.S. listed companies in SEC's AAERs (Accounting and Auditing Enforcement Releases) and private securities class action lawsuits database. Our statistics suggest that it generally takes much less time for fraudulent activities to be detected in China.

[25] When multiple types of fraudulent activities are mentioned in the CSRC enforcement action report, we assume these activities started in the same year.

[26] We report these robustness results using alternative specifications in Table 3 of the online appendix.

[27] We examine the impact of the size of the stock market and banking sector as measured by stock market capitalization divided by GDP and total credit divided by GDP, respectively, in Columns 1–2 of Table 4 in the online appendix. Our results show that size and quality of the banking sector play particularly crucial roles in deterring fraud as compared with the impact of stock markets.

Banking development may affect fraud through various channels. For example, firms in financially developed provinces might be subject to scrutiny by more competent bank loan officers. Consistent with this notion, Qian, Strahan, and Yang (2015) find that local bank loan officers in China play an important role in information use and production. Financially developed provinces are also more likely to have a critical mass of lawyers, accountants, and financial regulators. In unreported analysis, we verify that broader measures of institutional development, such as per capita lawyers and accountants, and whether the firm is located in a province that includes cities or treaty ports leased to foreign countries during the Opium War, have a significant impact on the incidence of fraud during our sample period.[28]

In the second column, we control for firm characteristics and find that corporate fraud prevails among smaller firms with lower profitability and higher leverage. The coefficients on leverage are positive and significant, consistent with the view that firms are likely to manage earnings to avoid violating debt covenants (Healy and Wahlen, 1999).[29] Our measure of profitability yields negative and significant coefficients, consistent with U.S. evidence that firms are more likely to engage in fraudulent activities when they suffer operating troubles (e.g., Arlen and Carney, 1992; Alexander and Cohen, 1999; Chidambaran, Kedia, and Prabhala, 2012). We also find that firm size is negatively and significantly related to the occurrence of fraud, which is contrary to findings from the U.S.; we return to this contrast toward the end of this section when we discuss the results of our bivariate probit analysis.

Turning to firm stock market characteristics, however, we find that neither stock return nor stock turnover is statistically significant. It may be that the price and trading volume of Chinese stocks during our sample are driven by factors that are unrelated to a firm's fundamentals, such as behavioral noise trading. The disparity between market price and fundamentals in China may also be explained by various capital market imperfections and government regulations, such as short-sale restrictions (Allen, Qian, Shan, and Zhu, 2017).

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[28] We report the robustness results using per capita accountants and lawyers in Table 4 in the online appendix. Following Lu, Pan and Zhang (2013), we also use a dummy variable indicating whether a province includes cities or treaty ports leased to foreign countries during the Opium War as a proxy for better institutional development to address the reverse causality concern. Establishment of the leased territories or treaty ports is likely to have a positive effect on local institutional and financial development due to the introduction of Western culture and legal and financial systems. However, it is unlikely that such establishments were affected by corporate fraud during 2000 to 2010. Our results show that both institutional development measures play significant roles in deterring fraud.

[29] In Online Appendix Table 12, using loan-level data, we find that this leverage effect comes from short-term loans. We show that short-term loans are associated with higher probability of fraud, while longer-term loans are negatively related to fraud occurrence.

Next, we study the effects of ownership structure on financial misreporting, using the two different measures of ownership structure described in the previous section. The impact of large blockholders on the frequency of fraud is not obvious. On the one hand, such blockholders have the capacity and incentive to monitor management and prevent fraud. On the other hand, they may be prone to collude with management in expropriating minority shareholders.

The results are given in Column (3).[30] We find that fraud is significantly less frequent when the largest shareholder's block or state ownership is higher.[31] We also examine the impact of foreign and managerial ownership and conventional governance factors.[32] Our results show that all these additional measures are insignificant, but the banking development coefficient remains negative and significant. Our results suggest that, in contrast to the U.S., conventional proxies for corporate governance, including board size, board independence, and separating the CEO and chair roles, are not effective deterrents of fraud in China. (For U.S. studies that find these governance features reduce fraud, see Beasley, 1996; Dechow, Sloan, and Sweeney, 1996; Efendi, Srivastava, and Swanson, 2007; Khanna, Kim, and Lu, 2015).[33] Our results, however, are consistent with Agrawal and Chadha (2005), who find that the independence of boards and audit committees is unrelated to the probability of a company restating earnings in the U.S.

In the last column, we retain all variables that are statistically significant in the previous regressions: *Banking Development, Size, Leverage, ROA, Sales Growth, Largest Shareholder,* and

[30] Note that the number of observations in Column (3) drops to 8,340 from 11,681 in Column (2) mostly due to missing information on the ownership structure. This causes the number of observations in the baseline regression (Column 4) to be smaller than the full sample as reported in Table 1. While we do not find a significant difference in the banking development indicator between the full sample and the baseline sample, we observe that firms in the full sample tend to be smaller than firms in the baseline sample. This suggests that smaller firms are more likely to have missing variables. To mitigate this selection concern, we examine how our main results vary across firm size. We find that banking development and banking liberalization have stronger effects on smaller firms. (See Table 10 of the online appendix). This means that the missing data on small firms biases against us finding significant results.

[31] Prior papers have also shown that blockholders affect governance and firm value in China. For example, Berkman, Cole, and Fu (2009) find that the identity of blockholders affects the likelihood of loan guarantees to related parties. Berkman, Cole, and Fu (2014) show that block transfers among government agencies, SOEs, and private investors increase firm value.

[32] The results are presented in Table 5 of the online appendix.

[33] Existing evidence on the role of corporate governance in China is mixed. On one hand, Allen, Qian, and Qian (2005) suggest that a weak auditing profession and inefficient board monitoring are partially responsible for the relatively sluggish growth of China's listed sector. Allen, Qian, Shan, and Zhu (2017) indicate that the governance issue related to self-dealing (i.e., tunneling) is one of the main contributors of the poor performance of China's stock market despite high economic growth. By documenting the severity of tunneling activities of Chinese listed companies through inter-corporate loans, Jiang, Lee, and Yue (2010) conclude that institutional ownership, auditors, and other governance mechanisms are inadequate in mitigating this tunneling practice. On the other hand, Giannetti, Liao, and Yu (2015) find that board characteristics affect firm performance in China. Our finding that conventional governance measures do not affect fraud highlights the fact that the role of conventional governance mechanisms in China is context specific.

State Ownership. We will use these variables in the following probit regression, which serves as our baseline throughout the remainder of the paper:

We examine the economic significance of key explanatory variables in Online Appendix Table 13 Panel A. Columns (1) and (2) report the probit regression coefficient estimates and marginal effects estimated at the means of covariates in Model (2). Sample means and standard deviations of the explanatory variables are reported in Columns (3) and (4). We present absolute and percentage changes in predicted probability if we increase one explanatory variable from its mean by one standard deviation while keeping other determinants at the mean in Columns (5) and (6), respectively.

It is immediate that these key variables have effects on fraud that are economically as well as statistically significant. In absolute magnitude, the smallest impact of a one standard deviation increase occurs for sales growth, which decreases fraud by 0.34 percentage points, which is a still sizeable 11.71% decrease in relative terms. The largest absolute impact is for return on assets, which decreases fraud by 1.09 percentage points, or 37.79% in relative terms. To a similar degree, a one standard deviation increase in banking development leads to a 34.13% decrease in fraud probability in relative terms. These results suggest that provincial banking development has a very large impact on corporate fraud in China.

Fraud commitment is not directly observable as we only observe fraud after it is detected. Therefore, our dependent variable is the product of the probability fraud is committed and the probability that committed fraud is detected. Following recent literature on corporate fraud (e.g., Wang, Winton, and Yu, 2010; Wang, 2013; Wang and Winton, 2014; Khanna, Kim, and Lu, 2015), we use the bivariate probit model to separate the determinants of fraud commitment and detection. The results are reported in Online Appendix Table 6.

Our bivariate probit estimation suggests that most of the significant factors from our earlier probit analysis have opposite effects on fraud commission and fraud detection. For example, larger firms are less likely to commit fraud and more likely to be detected if they do commit fraud; the same is true for firms that are more profitable or where the largest shareholder has a higher stake. Conversely, higher leverage reduces the probability of detection and increases the probability of commission. In all cases, the effect on fraud commission has the same sign as the coefficient in the simple probit regression.

For our purposes, the most critical finding is that firms in provinces with higher levels of banking development are less likely to commit fraud and more likely to be detected if they do commit fraud. This is consistent with such provinces having better bank monitoring as in our model, but it could also reflect better monitoring and enforcement by others whose skill level is likely to be correlated with development, such as accountants, lawyers, and regulators. By contrast, an alternative explanation of our simple probit results—firms in financially developed provinces are less likely to be detected for fraud because they have more financial resources to bribe regulators—is not supported by our bivariate probit results.

For robustness, we also investigate whether some of our results are driven by political connections, which might affect both a firm's incentives to commit fraud and the likelihood that the government will investigate fraud. On the one hand, politically connected firms might already be very profitable so that management does not have incentive to manipulate accounting statements;[34] on the other hand, fraud committed by politically connected firms is perhaps less likely to be detected, or less likely to draw severe penalties, both of which may encourage fraud commission. (See Stuart and Wang, 2016, for evidence on high-technology Chinese firms, and Berkman, Cole, and Fu, 2010, for evidence from governance reforms). Yu and Yu (2011) find that U.S. firms that lobby have a significantly lower hazard rate of being detected for fraud. We thus control for the role of political connections in corporate fraud.

The results are reported in Online Appendix Table 7.[35] While we find that a few political connections do matter in intuitive ways, our key finding—higher provincial banking development reduces fraud commission and increases fraud detection—remains unchanged.

[34] A large literature shows that such connections have positive value for firms; for example, see Fisman (2001), Faccio (2006), Calomiris, Fisman and Wang (2010), and Piotroski and Zhang (2014).

[35] The results after controlling for political connections are reported in Table 7 of the online appendix. We investigate three main types of political connections: (1) whether a firm's past and incumbent CEOs are politically connected during a specific year, (2) whether a firm is connected to the incumbent CSRC chairperson or vice chairperson, and (3) whether a firm operates in a government-supported industry. Within (1), we examine four types of political connections that CEOs may have: (a) whether the CEO has worked for a state-owned company, (b) whether the CEO has worked as a central or local government official or has been in the military, (c) whether the CEO has been a deputy in the Chinese People's Political Consultative Conference (CPPCC), and (d) whether the CEO has been a deputy in the National People's Congress (NPC). For (2), we use a dummy variable that indicates whether a firm's headquarters are located at the incumbent CSRC chairperson's or vice chairperson's birth city. We note that even though firms in China rarely relocate, this variable exhibits significant time variations due to the turnovers of CSRC executives. Finally, for (3), we use a dummy variable that indicates whether the company operates in government-supported industries based on China's Five-Year Plans for National Economic and Social Development.

6. Banking liberalization as a natural experiment on banking development

Our results thus far show that banking development is strongly associated with lower incidence of fraud. Although this is consistent with Hypothesis 1, it could also reflect reverse causality or spurious correlation. For example, banks may choose to lend to firms that are less likely to engage in fraudulent activities; prevalence of fraud may itself hinder development of the banking sector. Also, banking development may be correlated with omitted variables that also affect the prevalence of fraud, such as unobserved growth opportunities of local companies or differences in provincial legal and culture norms. Accordingly, we now turn to testing Hypotheses 2-5, using China's banking liberalization following its accession to the WTO as a natural experiment.

We start by describing foreign bank presence in China. Next, we use difference-in-difference and matching-city approaches to investigate the effects of foreign bank entry on fraud reduction. We also show how such effects vary across lender types, borrower asset structure, and industry conditions. Last, we address possible concerns about the parallel-trends assumption that is implicit in our tests. In all our work, we return to using simple probit regressions; our city-by-city subsamples are not large enough for bivariate probit analysis to yield meaningful results.

6.1. Summary statistics on foreign bank presence in China

We use the CSMAR loan-level data to examine the presence of foreign banks following China's banking liberalization.[36] We rank foreign banks in terms of their total value of loans issued to Chinese listed companies after liberalization during the 2000 to 2010 period. Our results show that HSBC (U.K.), The Bank of East Asia (Hong Kong), OCBC (Singapore), Standard Chartered (U.K.), and Citibank (U.S.) are among the top five foreign banks with total loan values of 110, 30, 24, 24, and 14 billion RMB, respectively. We also summarize the number of foreign banks and foreign loans by pilot cities after the banking liberalization. The pilot cities with most foreign banks in our sample are Shenzhen (33 banks), Shanghai (33 banks), and Beijing (22 banks).[37] We

[36] We present the summary statistics on foreign bank presence following banking liberalization in Table 8 of our online appendix.
[37] These numbers are in line with Xu (2011)'s finding that foreign bank penetration, while being only 2% of total Chinese banking system in 2007, was substantially larger in selected cities: 14.5% in Shanghai and 7.2% in Beijing.

note that these numbers may be the lower bound of qualified foreign banks in the pilot cities since CSMAR loan database only covers loans granted to listed companies.

To examine changes in foreign loan usage after liberalization, we calculate the number of firms with new loans and, among them, the percentage of firms with new foreign loans.[38] We find that firms gradually shifted to borrowing from foreign banks: 3.38% of firms with new loans during the first year after liberalization borrowed from foreign banks; 5.56% of firms with new loans during the fifth year after liberalization borrowed from foreign banks; and 11.69% of firms with new loans during the ninth year after liberalization borrowed from foreign banks. All in all, these findings confirm those in Xu and Lin (2007) and Xu (2011), showing that foreign banks have significant presence in China after banking liberalization took place.

6.2. Main results and matching-city approach

We begin our analysis by showing that banking liberalization reduces fraud. To do this, we add a dummy variable, *Financial Liberalization*, to the baseline probit regression given in Equation (5). This dummy variable equals one if a firm is located in a city that allows foreign banks to conduct RMB business in a specific year, and zero otherwise. We report these results in Column (1) of Table 3. Because provincial factors other than banking development (such as income and education level) may also affect the probability of fraud, we replace *Banking Development* with provincial fixed effects. The estimated coefficient of *Financial Liberalization* is negative and significant at the 1% level, which is consistent with the hypothesis that a firm is associated with less fraud if it is located in a city that allows foreign bank entry.[39]

Insert Table 3 here

Nevertheless, it is possible that liberalized cities were more developed than non-liberalized cities before the liberalization, in which case the difference in fraud propensity might be caused by this pre-liberalization difference rather than the liberalization itself. To mitigate this concern, we adopt a matching-city approach by restricting our sample to two groups of firms: firms located

[38] The results are presented in Panel C of Table 8 in the online appendix.
[39] China established five Special Economic Zones (i.e., the city of Shenzhen, Zhuhai, Xiamen, and Shantou, and Hainan province) in the 1980s. Cities in the Special Economic Zones may have better institutional development, and therefore be subject to less fraud. We show in Table 9 of the online appendix that our results on banking liberalization and fraud reduction are not confounded by the effects of Special Economic Zones.

in liberalized cities (the treatment group) and firms located in *comparable* non-liberalized cities (the control group).

In Column (2), we use firms located in non-liberalized cities in the same province as the control group.[40] In order to do so, we restrict our sample to provinces with both liberalized and non-liberalized cities in a specific year. In other words, for each province, we drop years when none of its cities were liberalized or all of its cities were liberalized. (Thus, by construction, the restricted sample covers the transitional period from 2002 to 2006). By comparing firms in liberalized cities with firms in the non-liberalized cities of the same province at the same time, we control for any province-level effects. Note that for firms located in the non-provincial special districts (e.g., Shanghai), we use adjacent provinces or special districts with a similar level of banking development as the comparison group (e.g., Zhejiang and Jiangsu provinces for Shanghai and Sichuan province for Chengdu).

One may still argue that the liberalized cities are more developed than are the other areas of the same province. In Column (3), instead of using other cities in the same province as the control group, we construct a sample with *matched* cities. For each liberalized city, we select the matched non-liberalized cities based on two criteria: (1) the matched cities must share a border with the liberalized city; (2) the difference in annual GDP between the liberalized city and each matched city must be less than one standard deviation of the full sample GDP distribution.[41] By doing so, we ensure that our matched listed companies are in nearby cities with similar levels of ex-ante financial and economic development. [42]

Regardless of which matching approach we use, the effect of banking liberalization on fraud is statistically significant at the 1% level. We also show that the economic impact of banking

[40] Our matching method obliviates the need to include higher order fixed effects, such as province*year fixed effects, in the regressions. We use the matching method because including higher order fixed effects in non-linear models often results in inconsistent estimators.

[41] As a robustness test, we define matched cities according to the urban agglomeration development plan by the central or provincial Chinese government. For example, according to the Yangzi River Delta Region Development Plan approved by the State Council, this urban agglomeration consists of Shanghai (central city), Nanjing, Suzhou, Hangzhou and 12 other adjacent cities. Our main results still hold when we adopt this matching criterion.

[42] The matching-city approach could help isolate the direct effects of banking liberalization from other confounding factors. For example, a concern is that the liberalization of bank activities generated booms in local economies, increased demand for firms' products, and relaxed financial constraints. Our research design should alleviate this concern since liberalization-induced positive demand shocks that may affect firms' growth opportunities should affect the nearby firms across the city border alike as companies in adjacent cities are likely to be in the same product markets and share suppliers and customers.

liberalization is large. Panel B of Online Appendix Table 13 reports the economic significance of the *Financial Liberalization* coefficient. Liberalization results in a relative decline in the incidence of fraud that ranges from 44.9% to 52.09%, depending on the specification used while holding other determinants at the means. The results in this subsection are consistent with the hypothesis that improved banking development after banking liberalization reduces corporate fraud.[43]

One might be concerned that the accession to WTO brought other changes to the legal systems and accounting standards or that there were other regulatory changes in China during the same time period. We note that unless the omitted shocks hit the pilot cities (and not the matched cities) with the same timing as the staggered liberalization schedule, our matching-city approach has already addressed this issue.

Another concern is that liberalization affects fraud through banks but the economic channel is not increased monitoring. Particularly, foreign bank entry may increase credit supply and fraud is reduced through the relaxation of financial constraints. We note that our baseline controls include financial leverage which is largely driven by bank loans. Therefore, our coefficient on *Financial Liberalization* captures the effects of bank monitoring conditioned on a given level of bank credit already. Moreover, the asset-structure and industry-condition tests below show that the financial constraint story is unlikely to explain our results.

6.3. Monitoring by foreign versus domestic banks

In the previous subsection, we find that banking liberalization significantly reduces fraud among firms in liberalized cities. We now turn to testing Hypotheses 2 and 3, which predict how changes in fraud in liberalized cities should vary across different types of banks.

6.3.1. Direct effects versus spillover effects on domestic banks

Using loan-level information from CSMAR loan database, we first split borrowers into those that ever borrow from foreign banks and those that borrow exclusively from domestic banks over the sample period. (Firms that borrow from both foreign and domestic banks are included in the

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[43] Our finding that bank liberalization reduces fraud does not contradict our earlier finding that high leverage increases fraud; bank liberalization improves banks' incentives to monitor for any amount of debt they hold, but having too much debt in total is well-known to increase the agency costs of debt, including fraud and risk-shifting. Further, we show later in Online Appendix Table 12 that the effect of leverage in increasing fraud in China mainly comes from short-term debt.

first group.) Then, we re-estimate the baseline specification from Column 1 of Table 3 for each subsample and examine the economic significance. The results are reported in Table 4.

Insert Table 4 here

We indeed find that fraud propensity in liberalized cities dropped after liberalization, both among firms that borrow from foreign banks and among those that borrow exclusively from domestic banks. The reduction in fraud propensity is greater for firms with loans from foreign banks. The coefficient on *Financial Liberalization* is estimated at -0.510 for foreign banks and -0.220 for domestic banks. (Untabulated analyses of economic significance suggest that liberalization results in a relative decline in the incidence of fraud of 72% for firms borrowing from foreign banks and of 39% for firms borrowing only from domestic banks). This is consistent with Hypotheses 2 and 3. Note that our finding that fraud drops even for borrowers of domestic banks is not consistent with the pure selection story of Dell'Ariccia and Marquez (2004): if foreign banks simply cherry pick high-quality borrowers and there is no spillover effects on domestic banks, then the incidence of fraud among domestic banks should *increase* since they are left with a pool of more fraudulent borrowers. Our finding is consistent with foreign bank entry improving monitoring at domestic banks.

Finally, we divide the subsample of domestic borrowers into borrowers of national banks and borrowers of local banks and re-estimate the baseline regression. The results are reported in Columns 3 and 4 of Table 4, respectively. We find that the liberalization effect is stronger among firms that borrow from local banks. The coefficient on *Financial Liberalization* is estimated at -0.211 for national banks and -0.290 for local banks. This is consistent with the idea that the foreign bank entry will affect the monitoring of local domestic banks more than that of national domestic banks, suggesting higher competitive pressure or larger room for technological spillover compared to national banks.

6.3.2. Borrower selection by foreign banks

Earlier, we found that the decline in fraud after liberalization is more pronounced among the clients of foreign banks. Although our evidence of declines in fraud for borrowers at domestic banks is not consistent with selection, it remains an open question whether the decline for firms that switch to foreign banks is due to pure selection, foreign banks' superior monitoring skills, or both. In this subsection, we look for evidence of selection by foreign banks by regressing a foreign-bank client indicator (which equals one if the firm receives loans from a foreign bank) on various firm characteristics.[44] We use both the full sample and the after-liberalization subsample for more concise comparison since by definition there are no foreign bank loans before foreign bank entry. We examine specifications both with and without industry dummies since most variation in asset tangibility comes from cross-industry variation. The results are reported in Online Appendix Table 11.

We find evidence that foreign banks do tend to select larger and more profitable borrowers with higher sales growth, block ownership and borrowing history (higher lagged leverage). Foreign banks are also less likely to lend to state-owned firms.[45] Without industry dummies, foreign banks lend more to firms with tangible assets. However, this result disappears once we include industry dummies, suggesting that foreign banks focus on industries with high asset tangibility. Overall, our finding suggests that the greater reduction in fraud among foreign bank clients could be driven in part by their selection of larger and more profitable firms with block ownership and better growth trajectory. However, our findings are also consistent with a combination of selection and monitoring stories. Foreign banks may have better monitoring incentives, so they are better at preventing borrower moral hazard in general. Nevertheless, they are at an information disadvantage relative to domestic banks, so they select more transparent firms and more tangible industries where their information disadvantage is less critical.

6.4. Borrower Asset Structure

In this subsection, we examine whether the effects of foreign bank entry vary across borrower asset structure. We start by testing Hypothesis 4.[46]

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[44] Our explanatory variables include all variables from the baseline model as well as asset tangibility which may affect bank monitoring according to prior literature. We also note that most observations whose foreign bank client indicator is zero are clients of domestic banks because there are very few firms with no loan at all. So, our regressions effectively compare foreign bank clients against domestic bank clients.

[45] These results are generally consistent with Lin (2011), who finds that after foreign bank entry profitable (high ROA) firms and non-state-owned firms use more long-term loans; whereas firms with higher value of potential collateral (firms in industries with above median fixed asset ratios) do not.

[46] We note that we test the prediction that the effects of banking liberalization vary across borrower asset structure types (triple-difference specification) rather than the prediction that the propensity of fraud in general varies across borrower asset structure types (single-difference specification). The effects of firm's asset composition on fraud might come from many channels besides bank monitoring. However, only the component related to bank monitoring would vary with banking liberalization. So, the triple-difference specification helps us address this identification issue.

We compute asset tangibility ratio (tangible assets divided by total assets), percent fixed assets (fixed assets divided by total assets), and percent current assets (current assets divided by total assets) at the industry level. We choose industry-level variables because asset composition at the firm level might be endogenous to growth opportunities. We argue that asset composition at the industry level is more exogenous. For example, certain industries may have to hold more fixed assets if their production technology requires larger machines or more land. We split the sample at the median of the asset-structure variables and re-estimate the baseline specification from Column 1 of Table 3 Panel A for each subsample. The results are reported in Table 5 Panel A.

Insert Table 5 here

We find that the liberalization effect is stronger in industries with higher asset tangibility ratio, higher levels of fixed assets, and lower levels of current assets.[47] In fact, the effects of banking liberalization is only significant for the subsamples with above-the-median fixed assets and below-the-median current assets. These findings are consistent with Hypothesis 4, supporting the notion that the effects of banking liberalization we documented in the full sample are related to changes in bank monitoring ability.

6.5. Industry conditions

In this subsection, we test Hypotheses 5 and 5a. A unique feature of both predictions is that they allow us to test our bank monitoring model against an alternative story in which banking liberalization relaxed financial constraints and thus decreased firms' incentives to commit fraud. [48] If liberalization does decrease fraud through the relaxation of financial constraints, then fraud reduction should be more pronounced in industries that are financially constrained before liberalization (i.e., low-growth industries). By contrast, Hypothesis 5 predicts no difference across

[47] We note that the difference across subsamples partitioned by asset tangibility is smaller than those partitioned by the fractions of fixed or current assets. Tangible assets include both the fixed component which tends to retain value and the current component which tends to lose value during bankruptcy. Partitioning by fixed or current assets tests our model more directly. Partitioning by tangibility is, however, a direct test of the financial constraint story: if liberalization decrease fraud through the relaxation of financial constraints, then fraud reduction should be more pronounced in low asset tangibility industries which are more constrained before liberalization. (Prior studies, such as Vik (2013), show only tangible assets can be effectively collateralized.) Our results reject the financial constraint story.

[48] While our control variables include many firm characteristics and our matching-city approach should account for many unobserved factors already, it is still possible that there are omitted variables correlated with financial constraints.

industries in liberalization's impact on fraud, and Hypothesis 5a predicts the opposite: that is, fraud reduction should be more pronounced in *high*-growth industries.

Based on Wang, Winton, and Yu (2010), we divide the full sample by two industry conditions: industry sales growth and number of IPOs. (Booming industries with high growth opportunities are likely to attract IPOs.) For each variable, we create above-median and below-median subsamples and examine the effects of banking liberalization across these subsamples. The results are reported in Table 5 Panel B. We find that industries with higher sales growth and more IPOs experience greater reduction in fraud after banking liberalization. In fact, the coefficients on banking liberalization are only significant in the high-sales growth and high IPOs subsamples. Therefore, our results reject the financial constraint story and Hypothesis 5 in favor of Hypothesis 5a.

6.6. Addressing parallel-trend assumption

The difference-in-difference estimation relies on comparison in levels, while necessitating the counterfactual trend behavior of treatment and control groups to be the same (the parallel-trend assumption). Our matching-city approach with staggered event dates, in many respects as elaborated below, already mitigates the concerns about this assumption.

First, we have multiple treatment and control groups, which decrease noises and biases associated with a single comparison. According to Roberts and Whited (2013), while the treatment and control groups should be similar along outcome-related dimensions, differences across groups within each category (treatment or control) are useful as these differences likely come with different biases. Many prior papers use similar identification strategies in the U.S., exploiting events that occurred at different times for different states, such as bank branch deregulations (Jayaratne and Strahan, 1996) and passage of antitakeover laws (Bertrand and Mullainathan, 2003).

Second, inclusion of firm-level controls and adjacent-city matching help balance the treatment and the control groups. We include a number of firm characteristics in all regression specifications to ensure that our results are not driven by the control-treatment difference along these observable dimensions. Our matching-city approach further balances the treatment and the control groups to the extent that firms in adjacent cities share similar unobserved characteristics.

Third, we perform a number of subsample tests which are akin to triple-difference tests.[49] (See Morse (2011) for example.) We find that the difference-in-difference coefficients are larger among clients of foreign banks, firms with new long-term bank loans, and less-monitored industries (i.e., booming industries/industries with higher levels of fixed assets or lower levels of working capital assets). These triple-difference tests indicate that liberalization reduces fraud through bank monitoring channels rather than other alternatives such as local economic booms or improvement in auditing quality.[50]

In this section, we further perform a falsification sensitivity test to ensure that alternative forces do not drive the liberalization effects documented. Following Angrist and Krueger (1999) and Roberts and Whited (2013), we conduct a placebo experiment by fictionally assuming that liberalization took place one, two, and three years before and after the actual year, while still distinguishing between treatment and matched cities according to the "true" liberalization program. Table 6 Panel A reports the results. The placebo results confirm that indeed there are no statistically significant results before the actual liberalization dates. The estimated coefficients on liberalization are most significant during the liberalization year and one year afterward. The *t+2* coefficient is marginally significant. Thus, the placebo experiment suggests that our main results do not appear to be driven by alternative forces.

Insert Table 6 here

As an alternative approach to alleviate the parallel-trend concern, we also control for a linear time trend specific to the liberalized cities, following Chava et al. (2013). This additional control allows us to more precisely identify the effect of banking liberalization using deviation from group-specific trend that might be driven by confounding factors. Table 6 Panel B reports the results. Even though the trend specific to liberalized cities may partly capture the effects of liberalization, we still find significant coefficient on the liberalization term in both baseline and matched cities specifications.

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[49] The subsample tests create a counterfactual framework for triple-difference approach. For example, in Online Appendix Table 12, given that the effects of bank monitoring should concentrate among firms with bank loans, the subsample of firms without new loans should provide an unbiased benchmark how firms in liberalized and non-liberalized cities would have differed had there not been an increase in bank monitoring due to liberalization. Thus, by comparing this benchmark with the subsample of firms with new long-term loans, we difference away endogeneities associated with the selection of pilot cities and the timing of the pilot program.

[50] We acknowledge the possibility that banking liberalization leads to a general improvement in auditing/lawyer quality in liberalized cities, and banks therefore are able to hire better lawyers and improve their auditing process, which in turn reduces fraud. Our triple-difference results, however, are inconsistent with the idea that improved auditing quality affects fraud through channels unrelated to bank monitoring.

8. Conclusion

Our paper presents a simple model of how bank monitoring may discourage borrower financial misreporting. The model illustrates that banks do not monitor firms so as to detect misreporting per se, but rather to get a better sense of the firm's financial health to decide whether it is worth funding/continuing or not. To maximize expected loan repayment, the optimal level of credit monitoring intensity for banks involves trading off the benefits of early liquidation of bad firms versus marginal costs of monitoring more intensively. Bank monitoring therefore reduces misreporting by decreasing the odds that the fraud produces a private control benefit for management by allowing bad firms to continue operation.

A benefit of our model is that it generates testable implications about the impact of a reduction in monitoring costs on the incidence of misreporting fraud and how this impact may vary depending on borrower asset structure and industry conditions. As bank monitoring costs are often endogenous and unobservable, we take advantage of the unique institutional settings in China, and based on existing evidence, reasonably assume that monitoring costs of Chinese banks are significantly lower in provinces with less developed banking markets, and they decrease subsequently after the exogenous shock of banking liberalization.

Using CSRC enforcement actions concerning financial misreporting of firms listed on mainland China's two stock exchanges from 2000 to 2010, we find that fraud is less likely to be committed and more likely to be detected once committed if the firm is headquartered in a province with greater bank development. We then use the staggered banking liberalization that followed China's entry into the WTO in 2001 to focus on exogenous shocks to banking sector development. Fraud propensity among firms headquartered in a given city drops after foreign banks are allowed to enter that city conducting RMB transactions, with the greatest decreases concentrated among firms that borrow from foreign banks. This is consistent with improved monitoring by foreign banks and, to a lesser extent, by domestic banks that have improved in anticipation of foreign bank entry, both of which in turn reduce the likelihood of financial misreporting. As predicted by our model, fraud reduction is greatest for borrowers in high growth industries and for borrowers with relatively high levels of fixed assets or relatively low levels of working capital.

To the best of our knowledge, we are the first to model whether bank monitoring has a causal impact on the likelihood of corporate financial fraud, whereas the existing literature focuses on the role of stock market participants, owners, auditors, and market regulators. Given the importance of banks in most developed and developing countries—which often exceeds the importance of stock markets per se—and the large negative externalities created by corporate fraud, our work suggests that banking sector development is a critical avenue for reducing such fraud and improving the efficiency of the financial system.

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Table 1
Summary Statistics.

This table reports the distribution of fraud by the calendar year of fraud detection in Panel A, the summary statistics of main variables in Panel B, and compares the mean of these variables between fraud and non-fraud firms one year before fraud detection in Panel C. The sample consists of firms listed on mainland China's stock exchanges during the 2000 to 2010 period. Fraud firms refer to firms that were detected committing financial misreporting by CSRC in a specific year. *Banking Development* is the provincial index of banking industry marketization by Fan, Wang, and Zhu (2011). In Panel A, Columns (1) – (5) report calendar year, total number of firms, number of fraud and non-fraud firms, and the percentage of fraud firms, respectively. In Panel B, Columns (2) to (7) report the number of observations, mean, standard deviation, and the 25th, 50th, and 75th percentile of main variables, respectively. In Panel C, Columns (2) and (3) report the mean values of different characteristics for the fraud and non-fraud firms one year before fraud detection, while Columns (4) and (5) report the differences in means and the *t*-statistics for the two-tailed *t*-tests. The *t*-statistics marked with *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively. All variables are winsorized at the 1% and 99% levels. Definitions of all variables are provided in Online Appendix B.

Panel A: **Distribution of fraud by year**

(1) Year	(2) Total firms	(3) Fraud firms	(4) Non-Fraud firms	(5) % Fraud Firms
2000	925	11	914	1.19%
2001	1,006	49	957	4.87%
2002	1,070	36	1,034	3.36%
2003	1,131	28	1,103	2.48%
2004	1,191	36	1,155	3.02%
2005	1,190	75	1,115	6.30%
2006	1,253	59	1,194	4.71%
2007	1,355	56	1,299	4.13%
2008	1,405	43	1,362	3.06%
2009	1,541	63	1,478	4.09%
2010	1,864	52	1,812	2.79%
Total	13,931	508	13,423	3.65%

Panel B: Summary statistics of main variables

(1) Variable	(2) Obs.	(3) Mean	(4) Std. Dev.	(5) 25%	(6) 50%	(7) 75%
Provincial Development						
Banking Development	13,931	8.42	2.73	6.33	9.04	10.51
Firm Characteristics and Ownership Structure						
Total Assets (billions)	13,931	3.92	9.58	0.84	1.54	3.18
Leverage	13,928	50.65%	27.57%	33.80%	48.80%	62.81%
ROA	13,928	4.82%	8.43%	2.87%	5.37%	8.28%
Sales Growth	12,708	3.41%	53.47%	-2.14%	12.81%	25.59%
Stock Return	12,605	36.72%	94.89%	-27.52%	0.80%	78.88%
Stock Turnover	13,654	2.66	2.34	0.92	1.82	3.78
Largest Shareholder	10,775	37.80%	15.89%	25.17%	35.73%	50.16%
State Ownership	13,745	26.20%	25.69%	0.00%	22.80%	49.88%
Factors Affecting Fraud Dection						
Abnormal Industry Litigation	13,931	0.11	7.57	-0.06	1.44	4.70
Disastrous Stock Return	13,931	0.10	0.30	0.00	0.00	0.00
Abnormal Stock Turnover	13,654	0.00	2.01	-1.32	-0.42	0.73

Panel C: Characteristics of fraud versus non-fraud firms

(1) Characteristics	(2) Fraud firms	(3) Non-Fraud firms	(4) Difference	(5) *t*-statistics
Provincial Development				
Banking Development	7.50	7.83	-0.33	-2.57***
Firm Characteristics and Ownership Structure				
Size	20.77	21.22	-0.45	-9.25***
Leverage	68.63%	49.73%	18.90%	16.02***
ROA	-3.70%	5.07%	-8.77%	-23.65***
Sales Growth	-32.78%	3.52%	-36.30%	-14.64***
Stock Return	23.97%	39.35%	-15.37%	-3.42*
Stock Turnover	2.88	2.39	0.49	4.74***
Largest Shareholder	31.96%	38.49%	-6.53%	-7.82***
State Ownership	22.31%	29.52%	-7.21%	-6.14***
Factors Affecting Fraud Dection				
Abnormal Industry Litigation	3.75	2.03	1.71	7.29***
Disastrous Stock Return	0.18	0.10	0.08	5.41***
Abnormal Stock Turnover	0.62	0.11	0.51	5.19***

Table 2

The probit model of corporate fraud.

This table reports the results from probit regression analyses of corporate fraud. The sample consists of firms listed on mainland China's stock exchanges during the 2000 to 2010 period. The dependent variable is one if a firm is detected committing financial misreporting in a specific year by CSRC, and zero otherwise.

The key explanatory variable, *Banking Development*, is the provincial index of banking industry marketization by Fan, Wang, and Zhu (2011). All explanatory variables are measured at the year before fraud detection and defined in Online Appendix B. All regressions include industry and year dummies. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Dependent variable: whether a firm is detected committing fraud			
Explanatory variables	(1)	(2)	(3)	(4)
Banking Development	-0.063***	-0.052***	-0.066***	-0.065***
	(-6.04)	(-5.06)	(-5.11)	(-6.00)
Size		-0.114***	-0.127***	-0.125***
		(-7.39)	(-4.44)	(-4.67)
Leverage		0.384***	0.362***	0.337***
		(4.26)	(4.95)	(5.46)
ROA		-2.423***	-2.348***	-2.377***
		(-11.94)	(-10.87)	(-8.86)
Sales Growth		-0.108***	-0.095***	-0.101***
		(-3.32)	(-2.40)	(-2.20)
Stock Return		-0.021	-0.031	
		(-0.75)	(-0.69)	
Stock Turnover		0.015	-0.003	
		(0.65)	(-0.14)	
Largest Shareholder			-0.686***	-0.705***
			(-5.60)	(-4.83)
State Ownership			-0.286***	-0.252***
			(-3.08)	(-2.67)
Industry Dummies	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y
No. of Obs.	13,931	11,681	8,340	8,435
No. of Fraud Obs.	508	477	363	369
% of Fraud Obs.	3.65%	4.08%	4.35%	4.37%
Pseudo R-sq	0.031	0.121	0.140	0.138

Table 3

Banking development and fraud: the liberalization of the banking sector after WTO.

This table examines the impact of the liberalization of the banking sector after WTO on corporate fraud. The dependent variable is one if a firm is detected committing financial misreporting in a specific year by CSRC, and zero otherwise. *Financial Liberalization* is an indicator variable that equals one if a firm is located in a city that allows foreign banks to conduct local currency-related business in a specific year (i.e. foreign bank entry), and zero otherwise. The sample consists of firms listed on mainland China's stock exchanges during the period 1990 to 2010. Columns (1)– (3) present the baseline results and matching-city approach results, respectively. Specifically, the probit regression results based on the full sample are reported in Columns (1). Probit regression results in Column (2) are based on the subsample of listed companies in provinces with cities that allow foreign bank entry and cities that do not allow so in a specific year (note, by construction, such subsample only covers the transitional period from 2002 to 2006). Regression results in Column (3) are based on the subsample of listed companies located in either treated cities that allow foreign bank entry or control cities that satisfy the following criteria: (1) they must share a border with the treated city; (2) the difference in annual GDP between the treated city and each control city must be less than one standard deviation of the full-sample GDP distribution; (3) they do not allow foreign bank entry. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Dependent variable: whether a firm is detected committing fraud		
	Baseline	Matching-city approach	
Explanatory variables	(1)	(2)	(3)
Financial Liberalization	-0.263***	-0.303***	-0.294***
	(-3.04)	(-3.05)	(-2.52)
Size	-0.117***	0.034	0.063
	(-4.08)	(0.53)	(0.82)
Leverage	0.320***	0.236	0.242
	(5.81)	(1.12)	(1.12)
ROA	-2.355***	-3.617***	-3.314***
	(-8.82)	(-5.47)	(-5.11)
Sales Growth	-0.113**	-0.145***	-0.178***
	(-2.50)	(-2.83)	(-3.48)
Largest Shareholder	-0.669***	-0.641	-0.836*
	(-5.34)	(-1.15)	(-1.84)
State Ownership	-0.258***	-0.494***	-0.428*
	(-3.23)	(-2.83)	(-1.76)
Industry Dummies	Y	Y	Y
Year Dummies	Y	Y	Y
Province Dummies	Y	N	N
No. of Obs.	8,435	2,319	1,930
No. of Fraud Obs.	369	101	84
% of Fraud Obs.	4.37%	4.36%	4.35%
Pseudo R-sq	0.153	0.246	0.223

Table 4

The impact of banking liberalization: subsample analysis for firms with different types of creditors.

This table compares the impact of banking liberalization on fraud for firms with different types of lenders. The dependent variable is one if a firm is detected committing financial misreporting in a specific year by CSRC, and zero otherwise. *Financial Liberalization* is an indicator variable that equals one if a firm is located in a city that allows foreign banks to conduct local currency-related business in a specific year (i.e. foreign bank entry), and zero otherwise. The sample consists of firms listed on mainland China's stock exchanges during the period 1990 to 2010. Columns (1)– (4) present probit regression results based on the subsamples of firms that have borrowed from foreign banks, and firms that have borrowed exclusively from domestic banks, national domestic banks, and local domestic banks during the sample period, respectively. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Dependent variable: whether a firm is detected committing fraud			
	Foreign banks	Domestic banks	National domestic banks	Local domestic banks
Explanatory variables	(1)	(2)	(3)	(4)
Financial Liberalization	-0.510***	-0.220*	-0.211*	-0.290**
	(-2.94)	(-1.93)	(-1.69)	(-2.09)
Size	-0.265***	-0.0966***	-0.104***	-0.056
	(-2.65)	(-3.68)	(-3.17)	(-1.28)
Leverage	0.699***	0.270***	0.340***	0.085
	(2.77)	(3.59)	(5.24)	(0.68)
ROA	-3.150***	-2.499***	-2.532***	-2.590***
	(-2.63)	(-10.92)	(-10.27)	(-6.56)
Sales Growth	0.090	-0.127***	-0.127***	-0.154***
	(0.58)	(-3.76)	(-3.17)	(-4.82)
Largest Shareholder	-0.484	-0.854***	-0.808***	-0.476
	(-0.81)	(-3.11)	(-3.51)	(-1.36)
State Ownership	-0.417	-0.159*	-0.093	-0.490**
	(-1.53)	(-1.75)	(-0.88)	(-1.99)
Industry Dummies	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y
Province Dummies	Y	Y	Y	Y
No. of Obs.	1,275	6,127	5,335	1,930
No. of Fraud Obs.	43	291	268	155
% of Fraud Obs.	3.37%	4.75%	5.02%	8.03%
Pseudo R-sq	0.242	0.156	0.161	0.223

Table 5
The impact of banking liberalization: subsample analysis for collateral and industry conditions.

This table compares the impact of banking liberalization on fraud for firms with different industry conditions (collateral in Panel A and industry cycles in Panel B). Probit model estimation results based on Model (3) of Table 3 with matching-city approach are presented. The dependent variable is one if a firm is detected committing financial misreporting in a specific year by CSRC, and zero otherwise. *Financial Liberalization* is an indicator variable that equals one if a firm is located in a city that allows foreign banks to conduct local currency-related business in a specific year (i.e. foreign bank entry), and zero otherwise. The full sample consists of firms listed on mainland China's stock exchanges during the period 1990 to 2010. In Panel A, Columns (2)– (7) present probit regression results based on the subsamples of firms within industries of lower and higher levels of asset tangibility ratio (tangible assets divided by total assets), and within industries of lower and higher levels of % fixed assets (fixed assets divided by total assets) and % current assets (current assets divided by total assets), respectively. In Panel B, Columns (2)– (5) present probit regression results based on the subsamples of firms within industries of lower and higher levels of sales growth and number of IPOs, respectively. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A: Collateral

	Dependent variable: whether a firm is detected committing fraud						
	Full Sample	Subsamples based on industry conditions					
		Asset Tangibility		% Fixed Assets		% Current Assets	
		Low	High	Low	High	Low	High
Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Financial Liberalization	-0.294***	-0.279*	-0.319**	-0.11	-0.407***	-0.434***	-0.07
	(-2.52)	(-1.88)	(-2.43)	(-1.47)	(-3.94)	(-4.72)	(-0.93)
Size	0.063	0.132***	0.043	-0.02	0.104***	0.09	0.01
	(0.82)	(3.16)	(0.36)	(-0.15)	-2.64	(1.62)	(0.04)
Leverage	0.242	0.372	0.201	0.09	0.37	0.32	0.18
	(1.12)	(1.18)	(0.58)	(0.22)	(1.41)	(1.59)	(0.36)
ROA	-3.314***	-2.603**	-4.058***	-3.532***	-3.301***	-3.203***	-3.475**
	(-5.11)	(-2.43)	(-6.27)	(-2.67)	(-10.52)	(-9.12)	(-2.51)
Sales Growth	-0.178***	-0.179**	-0.238***	-0.213***	-0.122*	-0.181***	-0.191***
	(-3.48)	(-2.10)	(-4.00)	(-3.01)	(-1.65)	(-3.10)	(-3.19)
Largest Shareholder	-0.836*	-0.551	-1.315***	-2.161***	-0.24	-0.33	-2.100***
	(-1.84)	(-1.13)	(-2.73)	(-17.58)	(-1.54)	(-1.55)	(-39.04)
State Ownership	-0.428*	-0.749***	-0.057	-0.31	-0.478**	-0.493**	-0.31
	(-1.76)	(-4.04)	(-0.11)	(-0.50)	(-2.39)	(-2.38)	(-0.45)
Industry Dummies	Y	Y	Y	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y	Y	Y	Y
No. of Obs.	1,930	941	922	575	1,355	1,388	542
No. of Fraud Obs.	84	35	49	44	40	42	42
% of Fraud Obs.	4.35%	3.72%	5.31%	7.65%	2.95%	3.03%	7.75%
Pseudo R-sq	0.223	0.197	0.254	0.291	0.154	0.159	0.287

Panel B: Industry Cycles

	Dependent variable: whether a firm is detected committing fraud				
		Subsamples based on industry conditions			
	Full Sample	Sales Growth		No. of IPOs	
		Low	High	Low	High
Explanatory variables	(1)	(2)	(3)	(4)	(5)
Financial Liberalization	-0.294***	0.055	-0.885***	-0.162	-0.450**
	(-2.52)	(0.48)	(-3.95)	(-1.34)	(-2.44)
Size	0.063	0.038	0.127***	0.111	0.018
	(0.82)	(0.27)	(5.37)	(1.51)	(0.22)
Leverage	0.242	0.171	0.238	-0.181	0.506
	(1.12)	(0.46)	(1.16)	(-0.67)	(0.99)
ROA	-3.314***	-3.353***	-4.109***	-4.869***	-2.583**
	(-5.11)	(-3.45)	(-10.42)	(-6.35)	(-2.50)
Sales Growth	-0.178***	-0.169***	-0.197	-0.332**	-0.058
	(-3.48)	(-2.90)	(-1.36)	(-2.10)	(-0.55)
Largest Shareholder	-0.836*	-1.277*	-0.270	-0.691	-1.057***
	(-1.84)	(-1.87)	(-0.61)	(-0.96)	(-2.60)
State Ownership	-0.428*	-0.652	-0.441	-0.629***	-0.312
	(-1.76)	(-1.25)	(-1.15)	(-3.01)	(-0.81)
Industry Dummies	Y	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y	Y
No. of Obs.	1,930	978	841	918	962
No. of Fraud Obs.	84	55	29	36	48
% of Fraud Obs.	4.35%	5.62%	3.45%	3.92%	4.99%
Pseudo R-sq	0.223	0.265	0.219	0.282	0.205

Table 6

Test results related to parallel trend assumptions.

This table reports test results related to parallel trend assumptions. Panel A reports placebo test results, whereby we repeat the regression analysis in Column (3) Panel A of Table 3, but pretend that bank liberalization occurred three years before (Column 2), two years before (Column 3), one year before (Column 4), one year after (Column 5), two years after (Column 6), and three years after (Column 7) the actual year. Panel B reports robustness results after controlling for separated linear time trends between pilot and non-pilot cities. *Pilot City Indicator* equals one if a firm is located in one of the pilot cities (i.e., Guangzhou, Zhuhai, Qingdao, Nanjing, Wuhan, Jinan, Fuzhou, Chengdu, Chongqing, Kunming, Beijing, Xiamen, Xian, Shenyang, Shantou, Ningbo, Harbin, Changchun, Lanzhou, Yinchuan, and Nanning) during the banking liberalization process, and zero otherwise. Detailed variable definitions are found in Online Appendix B. Robust standard errors are clustered at the industry and province levels with t-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A: Placebo test results

	Dependent variable: whether a firm is detected committing fraud						
	Actual Year (t)	Placebo 3 Yrs Before (t-3)	Placebo 2 Yrs Before (t-2)	Placebo 1 Yr Before (t-1)	Placebo 1 Yr After (t+1)	Placebo 2 Yrs After (t+2)	Placebo 3 Yrs After (t+3)
Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Financial Liberalization	-0.294***	-0.443	-0.203	-0.200	-0.376***	-0.296*	0.0776
	(-2.52)	(-1.37)	(-0.71)	(-1.00)	(-2.83)	(-1.80)	(0.47)
Size	0.063	0.052	0.051	0.054	0.065	0.0577	0.0464
	(0.82)	(0.71)	(0.69)	(0.72)	(0.93)	(0.82)	(0.60)
Leverage	0.242	0.235	0.238	0.225	0.241	0.232	0.231
	(1.12)	(0.99)	(1.03)	(1.02)	(1.04)	(0.98)	(1.03)
ROA	-3.314***	-3.389***	-3.366***	-3.349***	-3.315***	-3.289***	-3.360***
	(-5.11)	(-5.34)	(-5.30)	(-5.29)	(-5.15)	(-5.27)	(-5.19)
Sales Growth	-0.178***	-0.168***	-0.164***	-0.172***	-0.178***	-0.167***	-0.167***
	(-3.48)	(-3.39)	(-3.44)	(-3.26)	(-3.21)	(-3.38)	(-3.13)
Largest Shareholder	-0.836*	-0.807*	-0.793*	0.054	-0.877**	-0.808*	-0.755
	(-1.84)	(-1.77)	(-1.84)	(0.72)	(-2.04)	(-1.85)	(-1.64)
State Ownership	-0.428*	-0.455*	-0.446*	-0.434*	-0.440*	-0.474*	-0.470*
	(-1.76)	(-1.89)	(-1.79)	(-1.76)	(-1.77)	(-1.88)	(-1.86)
Industry Dummies	Y	Y	Y	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y	Y	Y	Y
Province Dummies	N	N	N	N	N	N	N
No. of Obs.	1,930	1,930	1,930	1,930	1,930	1,930	1,930
No. of Fraud Obs.	84	84	84	84	84	84	84
% of Fraud Obs.	4.35%	4.35%	4.35%	4.35%	4.35%	4.35%	4.35%
Pseudo R-sq	0.223	0.218	0.216	0.218	0.227	0.220	0.214

Panel B: Controlling for separated linear time trends

	Dependent variable: whether a firm is detected committing fraud		
	Baseline	Matching-city approach	
Explanatory variables	(1)	(2)	(3)
Financial Liberalization	-0.182***	-0.237*	-0.263***
	(-2.90)	(-1.77)	(-2.77)
Size	-0.118***	0.03	0.06
	(-3.96)	(0.47)	(0.79)
Leverage	0.320***	0.23	0.24
	(5.85)	(1.14)	(1.14)
ROA	-2.351***	-3.611***	-3.316***
	(-8.87)	(-5.54)	(-5.14)
Sales Growth	-0.111**	-0.144***	-0.179***
	(-2.55)	(-2.83)	(-3.53)
Largest Shareholder	-0.666***	(0.64)	-0.838*
	(-5.22)	(-1.14)	(-1.87)
State Ownership	-0.249***	-0.489***	-0.424*
	(-3.10)	(-2.77)	(-1.78)
Linear Trend * Pilot City Indicator	Y	Y	Y
Industry Dummies	Y	Y	Y
Year Dummies	Y	Y	Y
Province Dummies	Y	N	N
No. of Obs.	8,435	2,319	1,930
No. of Fraud Obs.	369	101	84
% of Fraud Obs.	4.37%	4.36%	4.35%
Pseudo R-sq	0.153	0.246	0.223

Appendix A. Proofs of Results in Text

Proof of Proposition 1. When the condition in part (a) of the proposition holds, the right-hand side of Equation (3) in the main text is positive even for $\mu = 1$, so the optimal choice is $\mu = 1$, which means $\phi(\mu) = (1-\mu)B/\lambda f = 0$. When the condition in part (c) of the proposition holds, the right-hand side of Equation (3) in the main text is negative even for $\mu = 0$, so the optimal choice is $\mu = 0$, which means $\phi(\mu) = (1-\mu)B/\lambda f = B/\lambda f$.

Otherwise, when condition (b) of the proposition holds, Equation (3) holds with equality for a unique choice of μ between 0 and 1, so the results for an interior optimum apply. ■

Proof of Corollary 1. As noted in the text, these comparative statics follow from taking the derivative (or cross-derivative) of the expression for optimal μ in Equation (4) in the text, and from the fact that $\phi(\mu) = (1-\mu)\, B/\lambda f$ when the optimal choice of μ is interior. ■

Proof of Corollary 2. Parts (a) through (c) follow from the definition $\Phi = (1-\theta)\,(1-\beta)\phi(\mu)$ and the expressions for optimal μ and thus $\phi(\mu)$ from Proposition 1.

The results in part (d) of the corollary follow from using Equation (4) in the text to obtain optimal μ, substituting that into $\phi(\mu)$, multiplying by $(1-\theta)\,(1-\beta)$ to get Φ, and then taking the appropriate derivative or cross-derivative.

The results in part (e) follow from setting optimal μ to 0, which means $\phi = B/\lambda f$ and $\Phi = (1-\theta)(1-\beta)B/\lambda f$. The comparative statics results in the corollary follow immediately. ■

Proof of Corollary 3. As noted in the text, $\mu(R)$ is still given by Equation (4); then, R is determined so that expected profits $\Pi(R,\mu(R)) = 0$. It follows that, for any parameter x, the change in monitoring (and thus in fraud $\phi(\mu)$) is given by $d\mu/dx = \partial\mu/\partial x + (\partial\mu/\partial R)\,(\partial R/\partial x)$. From Equation (4), however, it is clear that optimal μ does not depend on the lending rate R. Thus, $\partial\mu/\partial R = 0$, and $d\mu/dx = \partial\mu/\partial x$. It follows that all the comparative statics on the interior maximum case for μ still hold when the lending rate R is chosen competitively to make profits equal to zero. ■

Online Appendix

Online Appendix A. The legal framework for securities markets in China

China has three state laws that constitute the highest legal authority among security-related statutes: the Securities Law, which regulates issuing and trading securities, the Company Law, which regulates the organization and behavior of public and private companies, and the Securities Investment Fund Law, which regulates public and private securities investment fund activities. Article 7 of the Securities Law gives the CSRC the responsibility for exercising centralized and unified regulation over the nationwide securities markets.[51]

Since civil litigation systems in China are relatively immature, CSRC enforcement actions are the main legal mechanism for disciplining Chinese listed firms and their management. The current CSRC enforcement system separates hearings from case investigations to enhance the efficiency and fairness of enforcement actions. The Enforcement Bureau (Chief Enforcement Office), Enforcement Contingent, and the enforcement departments of CSRC regional offices work together in case filing, investigation and implementation of administrative sanctions, while the Administrative Sanction Committee is mainly responsible for hearings and proposing administrative sanction opinions.

In relation to violations of securities laws, CSRC may impose administrative sanctions or ban market entry on the liable entity or individual. CSRC administrative sanctions include the following types of penalties: orders to rectify illegal conduct, warning, fine, and confiscation of illegal income. According to CSRC annual report, in 2013, CSRC received 611 case leads, and probed into 350 cases, among which 41 suspected criminal cases were referred to the judicial authority, and 86 cases were closed within the same year. These cases involve financial

[51] Article 179 of the Securities Law mandates that CSRC shall perform the following regulatory duties for the securities market: (1) to formulate regulations and rules for the regulation of the securities markets and exercise the authority of approval and authorization pursuant to applicable laws; (2) to regulate the issuance, listing, trading, registration, depository and clearance of securities; (3) to regulate securities-related business of issuers, listed companies, securities companies, securities investment fund management companies, securities service institutions, stock exchanges and securities registrar and clearance institutions; (5) to supervise and inspect information disclosure concerning the issuance, listing and trading of securities; (6) to investigate and penalize violations of laws or administrative regulations governing the securities markets; (7) other duties as applied by applicable laws and administrative regulations.

misreporting, tunneling, insider trading, and others. Based on investigation of these cases, the CSRC made 79 decisions on administrative sanctions, and made 21 decisions to bar market entries of 38 individuals, including permanent bars on 25 individuals. Because CSRC enforcement actions have significant negative impact on stock price and may even lead to the replacement of the CEO and board of directors and other executives (Chen, Firth, Gao, and Rui, 2005, 2006; Chen, Cumming, Hou, and Li, 2016), the CSRC has established its credibility as the main legal watchdog against corporate securities fraud in China.

Online Appendix B. Variable definitions

Variable	Definition
Fraud	
Fraud Indicator	An indicator variable that equals one if a firm is detected committing financial misreporting by Chinese Securities Regulatory Commission (CSRC) in a specific year, and zero otherwise.
Detection Period	The time difference between the beginning year of fraud commitment as specified in CSRC enforcement action report and the year of CSRC enforcement action.
Provincial Development	
Banking Development	The NERI index of banking industry development by Fan, Wang, and Zhu (2011). The assessment is based on two factors: banking industry competition as measured by the percentage of deposits held by non-state-owned banks, and banking market efficiency as measured by the percentage of credits allocated to non-state-owned enterprises.
Stock Market Capitalization/GDP	Normalized provincial measure of stock market development, defined as stock market capitalization of listed firms whose headquarters are located within a province divided by provincial GDP.
Total Credit/GDP	Normalized provincial measure of banking sector development, defined as total loan credits divided by GDP.
Per Capita Lawyers	Number of lawyers divided by the total population of a province.
Per Capita Accountants	Number of accountants divided by the total population of a province.
Treaty	An indicator variable that equals one if the province includes treaty ports or leased cities to foreign countries during the Opium War.
Firm Characteristics	
Size	Logged value of total assets.
Leverage	Total liabilities divided by total assets.
ROA	Earnings before interest and tax (EBIT) divided by total assets.
Sales Growth	Percentage change in net sales from last year.
Stock Return	Annual stock return.
Stock Turnover	Annual trading value/ market capitalization.
Ownership Structure	
Largest Shareholder	Percentage of outstanding shares held by the largest shareholder.
State Ownership	Percentage of outstanding shares held by the state.
Foreign Ownership	Percentage of outstanding shares held by Foreign individuals or entities.
Managerial Ownership	Percentage of shares held by top executives.
Other Governance Mechanisms	
CEO Ownership	Percentage of outstanding shares held by the CEO.
CEO Compensation	CEO annual compensation (in 1,000 RMB).
CEO Duality	An indicator variable that equals one if the CEO is also the chairman of the board, and zero otherwise.
Independent Board	Number of independent directors/total number of directors on the board.
Board Size	Total number of directors on the board.
Foreign Auditor	An indicator variable that equals one if a firm hires an auditing firm that is registered or headquartered outside mainland China.

Variable	Definition
Bivariate Probit Model	
Fraud Commitment	An indicator variable that equals one if a firm commits a financial misreporting fraud in a specific year, and zero otherwise.
Fraud Detection	An indicator variable that equals one if a firm is detected committing financial misreporting fraud by CSRC in a specific year conditional on fraud commitment.
Industry Litigation	The logarithm of the total market value of firms subject to CSRC enforcement actions in a specific industry and year.
Abnormal Ind. Litigation	Yearly deviation from the average value of *Industry Litigation.*
Disastrous Stock Return	An indicator variable that equals one if annual stock return is below the bottom 10% of the sample distribution (i.e., <-45%), and zero otherwise.
Abnormal Stock Turnover	Deviation from the average stock turnover for a specific firm.
Natural Experiement	
Financial Liberalization	An indicator variable that equals one if a firm is located in a city that allow foreign banks to conduct local currency-related business in a specific year (i.e. foreign bank entry), and zero otherwise.
Political Connections	
SOE CEO	An indicator variable that equals one if a firm's past or incumbent CEO has worked for a state-owned enterprise, and zero otherwise.
Gov. CEO	An indicator variable that equals one if a firm's past or incumbent CEO has worked as a central or local government officer or has been in the military, and zero otherwise.
CPPCC CEO	An indicator variable that equals one if a firm's past or incumbent CEO has been a deputy to Chinese People's Political Consultative Conference (CPPCC), and zero otherwise.
NPC CEO	An indicator variable that equals one if a firm's past or incumbent CEO has been a deputy to National People's Congress (NPC), and zero otherwise.
CSRC Chair Connected	An indicator variable that equals one if a firm's headquarter is located at the incumbent CSRC chair or vice chairperson's birth city, and zero otherwise.
Gov. Supported Industry	An indicator variable that equals one if a firm is operating in government supported industries according to China's Five-Year Plans for National Economic and Social Development, and zero otherwise.

Online Appendix C. Additional Results

Appendix Table 1

Distribution of fraud by province.

This table summarizes the distribution of fraud by the province in which firms are headquartered. The sample consists of firms listed on mainland China's stock exchanges during the 2000 to 2010 period. Fraud firms refer to firms that were detected committing financial misreporting by CSRC in a specific year. *Banking development* is measured by the provincial index of banking industry marketization by Fan, Wang, and Zhu (2011). Columns (1)– (6) report the provinces in which firms are headquartered, average provincial banking development during the sample period, total number of firms in the sample, the number of fraud and non-fraud firms, and the percentage of fraud firms, respectively.

(1) Province	(2) Banking Development	(3) Total Firms	(4) Fraud Firms	(5) Non-Fraud Firms	(6) % Fraud Firms
Eastern and Coastal Provinces					
Zhejiang	10.56	943	17	926	1.80%
Shanghai	10.11	1,223	33	1,190	2.70%
Guangdong	9.27	1,898	69	1,829	3.64%
Jiangsu	8.83	943	15	928	1.59%
Shandong	8.66	777	20	757	2.57%
Liaoning	8.26	545	22	523	4.04%
Beijing	7.86	864	21	843	2.43%
Fujian	7.51	476	18	458	3.87%
Hebei	7.49	312	11	301	3.53%
Tianjin	7.46	228	10	218	4.39%
Hainan	5.75	202	11	191	5.45%
Total	8.34	8,411	247	8,164	2.94%
Central Provinces					
Henan	7.53	344	10	334	2.91%
Anhui	7.06	441	15	426	3.40%
Shanxi	6.93	218	5	213	2.29%
Hunan	6.76	434	27	407	6.22%
Hubei	6.33	611	23	588	3.76%
Jiangxi	6.04	249	7	242	2.81%
Jilin	5.17	328	15	313	4.57%
Heilongjiang	4.15	263	21	242	7.98%
Total	6.25	2,888	123	2,765	4.26%
Western Provinces					
Chongqing	7.72	288	13	275	4.51%
Ningxia	7.21	108	5	103	4.63%
Shaanxi	7.04	261	10	251	3.83%
Yunnan	6.89	196	6	190	3.06%
Sichuan	6.44	603	36	567	5.97%
Guangxi	5.75	244	11	233	4.51%
Guizhou	5.47	147	6	141	4.08%
Neimenggu	5.23	160	6	154	3.75%
Gansu	5.09	197	17	180	8.63%
Xinjiang	4.85	285	15	270	5.26%
Qinghai	4.12	76	7	69	9.21%
Tibet	2.65	67	6	61	8.96%
Total	5.70	2,632	138	2,494	5.24%
Full Sample	6.76	13,931	508	13,423	3.65%

Appendix Table 2

Distribution of fraud by detection period.

This table summarizes the distribution of fraud by detection period (Panel A) and the descriptive statistics of fraud detection period (Panel B). The sample consists of firms listed on Mainland China's stock exchanges during the 2000 to 2010 period. Fraud firms refer to firms that were detected committing financial misreporting by CSRC in a specific year. *Detection Period* is defined as the time difference between the beginning year of the fraud and the year of CSRC enforcement action.

Panel A: Distribution of fraud by detection period

Detection Period (Year)	Freq.	Percent
0	123	24.21%
1	135	26.57%
2	88	17.32%
3	42	8.27%
4	38	7.48%
5	32	6.30%
6	20	3.94%
7	21	4.13%
8	5	0.98%
9	3	0.59%
10	1	0.20%
Total	508	100.00%

Panel B: Distribution of fraud detection period

	Min.	25%	50%	Mean	75%	Max.	Std.
Detection Period	0	0	1	2	3	10	2.47

Appendix Table 3
Alternative specifications.

This table reports probit regression results of corporate fraud using alternative specifications. The sample consists of firms listed on Mainland China's stock exchanges during the 2000 to 2010 period. The dependent variable is one if a firm is detected committing financial misreporting in a specific year by CSRC, and zero otherwise. Panel A reports the results using independent variables measured at two years before fraud detection. Panel B reports the results using dependent variable measured at the first year of fraud commission as specified in CSRC enforcement action report, while independent variables are measured at one year before fraud commission. *Banking Development* is the provincial index of banking industry marketization by Fan, Wang, and Zhu (2011). Other explanatory variables are defined in Online Appendix B. All regressions include industry and year dummies. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A
Independent variables measured at two years before fraud detection

	Dependent variable: whether a firm is detected committing fraud		
Explanatory variables	(1)	(2)	(3)
Banking Development	-0.0635***	-0.0558***	-0.0550***
	(-4.73)	(-4.08)	(-3.82)
Size		-0.117***	-0.137***
		(-7.37)	(-4.57)
Leverage		0.247**	0.204**
		(2.46)	(2.16)
ROA		-1.631***	-1.729***
		(-10.27)	(-8.25)
Sales Growth		-0.0865***	-0.0641
		(-3.16)	(-1.56)
Largest Shareholders			-0.600***
			(-3.16)
State Ownership			-0.438***
			(-5.46)
Industry Dummies	Y	Y	Y
Year Dummies	Y	Y	Y
No. of Obs.	11,170	10,358	7,019
No. of Fraud Obs.	482	457	330
% of Fraud Obs.	4.32%	4.41%	4.70%
Pseudo R-sq	0.026	0.069	0.098

Panel B

Dependent variables measured at the commission year of fraud

	Dependent variable: whether a firm is detected committing fraud		
Explanatory variables	(1)	(2)	(3)
Banking Development	-0.0418***	-0.0349***	-0.0347***
	(-4.13)	(-3.71)	(-3.47)
Size		-0.106***	-0.0887***
		(-8.86)	(-4.97)
Leverage		0.067	-0.063
		(-0.63)	(-0.63)
ROA		-1.291***	-1.603***
		(-5.00)	(-4.43)
Sales Growth		-0.010	0.024
		(-0.45)	(0.600)
Largest Shareholders			-0.550***
			(-3.13)
State Ownership			-0.404***
			(-4.09)
Industry Dummies	Y	Y	Y
Year Dummies	Y	Y	Y
No. of Obs.	12,766	11,793	8,435
No. of Fraud Obs.	508	482	369
% of Fraud Obs.	3.98%	4.09%	4.37%
Pseudo R-sq	0.022	0.041	0.055

Appendix Table 4

Alternative Measures for financial and institutional development.

This table reports probit regression results of corporate fraud using alternative measures for financial and institutional development. The sample consists of firms listed on Mainland China's stock exchanges during the 2000 to 2010 period. Financial development is the normalized measure of stock market capitalization divided by GDP (Column 1) and total credit divided by GDP (Column 2), the number of accountants and lawyers per capita in Column 3, and whether the province includes treaty ports or leased cities during the Opium War (Treaty) in Column 4. The complete set of explanatory variables is defined in Online Appendix B. All regressions include industry and year dummies. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Dependent variable: whether a firm is detected committing fraud			
	(1)	(2)	(3)	(4)
Explanatory variables	Stock Market Capitalization/GDP	Total Credit/GDP	Per Capita Lawyers and Accountants	Treaty
Institutional Development	-1.324	-6.630***	-0.0391***	-0.179**
	(-0.57)	(-3.04)	(-5.27)	(-2.42)
Leverage	0.331***	0.321***	0.428***	0.327***
	(5.34)	(5.28)	(3.47)	(5.03)
ROA	-2.458***	-2.445***	-2.657***	-2.404***
	(-9.55)	(-9.65)	(-7.86)	(-8.65)
Sales Growth	-0.0959**	-0.0958**	-0.114**	-0.101**
	(-2.11)	(-2.18)	(-2.01)	(-2.15)
Size	-0.133***	-0.129***	-0.0541	-0.130***
	(-5.19)	(-4.84)	(-1.60)	(-4.85)
Largest Shareholder	-0.727***	-0.725***	-0.673**	-0.742***
	(-5.21)	(-5.03)	(-2.54)	(-5.03)
State Ownership	-0.182*	-0.186*	-0.391***	-0.229**
	(-1.92)	(-1.95)	(-2.60)	(-2.30)
Industry Dummies	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y
No. of Obs.	8,405	8,435	5,717	8,435
No. of Fraud Obs.	368	369	256	369
% of Fraud Obs.	4.38%	4.37%	4.48%	4.37%
Pseudo r^2	0.133	0.133	0.161	0.135

Appendix Table 5

Controls for governance.

This table reports probit regression results of corporate fraud after controlling for various governance mechanisms. The sample consists of firms listed on Mainland China's stock exchanges during the 2000 to 2010 period. The dependent variable is one if a firm is detected committing financial misreporting in a specific year by CSRC, and zero otherwise. The complete set of explanatory variables is defined in Online Appendix B. All regressions include industry and year dummies. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Dependent variable: whether a firm is detected committing fraud			
Explanatory variables	(1)	(2)	(3)	(4)
Banking Development	-0.065***	-0.0647***	-0.0716***	-0.0746***
	(-6.00)	(-6.18)	(-6.75)	(-6.02)
Size	-0.125***	-0.125***	-0.136***	-0.138***
	(-4.67)	(-4.65)	(-4.61)	(-3.82)
leverage	0.337***	0.337***	0.362***	0.359***
	(5.46)	(5.50)	(2.94)	(2.74)
ROA	-2.377***	-2.377***	-2.070***	-2.137***
	(-8.86)	(-8.82)	(-11.23)	(-11.58)
Sales Growth	-0.101**	-0.101**	-0.152***	-0.148***
	(-2.20)	(-2.16)	(-3.16)	(-2.99)
Largest Shareholder	-0.705***	-0.705***	-0.575***	-0.562***
	(-4.83)	(-4.75)	(-4.30)	(-3.88)
State Ownership	-0.252***	-0.251**	-0.250*	-0.247*
	(-2.67)	(-2.56)	(-1.95)	(-1.67)
Foreign Ownership		0.018	-0.017	-0.046
		(0.05)	(-0.07)	(-0.17)
CEO Duality			0.608	0.635
			(1.60)	(1.56)
CEO Ownership			-0.009	-0.008
			(-0.68)	(-0.63)
CEO Compensation			-0.020	-0.017
			(-0.35)	(-0.30)
Independent Board				-0.302
				(-0.56)
Board Size				-0.013
				(-1.16)
Foreign Auditor				0.253
				(1.27)
Industry Dummies	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y
No. of Obs.	8,435	8,435	6,171	6,085
No. of Fraud Obs.	369	369	259	256
% of Fraud Obs.	4.37%	4.37%	4.20%	4.21%
Pseudo R-sq	0.138	0.138	0.133	0.136

Appendix Table 6

Fraud commitment versus detection.

This table reports the estimation results from the bivariate probit model of fraud commitment versus detection. Column (1) presents the standard probit estimation results in Model (4) of Table 2, while Columns (2) and (3) report the estimation results of fraud commitment and fraud detection conditional on fraud commitment, respectively. The sample consists of firms listed on mainland China's stock exchanges during the 2000 to 2010 period. Fraud firms refer to firms that were detected committing financial misreporting by CSRC in a specific year. The explanatory variables are defined in Online Appendix B. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Probit	Bivariate Probit	
		Fraud Commitment	Fraud Detection
Explanatory variables	(1)	(2)	(3)
Banking Development	-0.065***	-0.079***	0.101***
	(-6.00)	(-4.48)	(2.92)
Size	-0.125***	-0.275***	0.349***
	(-4.67)	(-5.83)	(3.94)
Leverage	0.337***	0.809***	-0.779***
	(5.46)	(6.33)	(-4.25)
ROA	-2.377***	-3.528***	3.211***
	(-8.86)	(-8.42)	(4.25)
Sales Growth	-0.101***	-0.138**	0.073
	(-2.20)	(-2.31)	(0.84)
Largest Shareholder	-0.705***	-1.196***	3.174***
	(-4.83)	(-3.51)	(3.70)
State Ownership	-0.252***	-0.207	-0.364
	(-2.67)	(-1.03)	(-0.81)
Abnormal Ind. Litigation			0.002
			(0.25)
Disastrous Stock Return			0.172
			(1.23)
Abnormal Stock Turnover			0.138***
			(2.34)
No. of Obs.	8,435	8,063	8,063
No. of Fraud Obs.	369	364	364
% of Fraud Obs.	4.37%	4.51%	4.51%
Log Likelihood	-1306.1	-1262.5	-1262.5

Appendix Table 7

Controls for political connections.

This table reports probit and bivariate probit regression results of corporate fraud after controlling for political connections. The sample consists of firms listed on Mainland China's stock exchanges during the 2000 to 2010 period. Columns (1)– (6) report standard probit regression results where the dependent variable is one if a firm is detected committing financial misreporting in a specific year by CSRC; Columns (7)– (10) report bivariate probit regression results of fraud commitment versus detection. The key independent variable is *Political Connection*, as measured by *SOE CEO* (Column 1), *Gov. CEO* (Column 2), *CPPCC CEO* (Column 3), *NPC CEO* (Column 4), *CSRC Chair Connected* (Columns 5 and 7–8), and *Gov. Supported Industry* (Columns 6 and 9–10). The complete set of explanatory variables is defined in Online Appendix B. All regressions include industry and year dummies. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Probit						Bivariate Probit			
	SOE CEO	Gov. CEO	CPPCC CEO	NPC CEO	CSRC Chair Connected	Gov. Supported Industry	CSRC Chair Connected		Gov. Supported Industry	
							Fraud	Detect|Fraud	Fraud	Detect|Fraud
Explanatory variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Banking Development	-0.0612***	-0.0612***	-0.0611***	-0.0612***	-0.0635***	-0.0656***	-0.069***	0.201**	-0.088**	0.102**
	(-4.80)	(-4.83)	(-4.87)	(-4.86)	(-6.01)	(-6.02)	(-2.93)	(2.13)	(-2.82)	(2.11)
Political Connection	0.0178	0.0028	0.137	-0.011	-0.593***	-0.130**	0.075*	-1.755***	-0.156***	-0.635
	(0.18)	(0.06)	(0.59)	(-0.07)	(-7.78)	(-2.45)	(-1.98)	(-4.96)	(5.04)	(-1.02)
Size	-0.122***	-0.122***	-0.123***	-0.122***	-0.126***	-0.124***	-0.155***	0.946***	-0.275***	0.349***
	(-5.04)	(-5.10)	(-5.02)	(-5.03)	(-4.70)	(-4.65)	(-4.98)	(3.55)	(-4.95)	(3.94)
Leverage	0.342***	0.342***	0.343***	0.342***	0.345***	0.341***	0.600*	-0.464***	0.950***	-0.580**
	(5.57)	(5.56)	(5.64)	(5.56)	(5.65)	(5.59)	(1.65)	(4.31)	(3.73)	(-1.96)
ROA	-2.363***	-2.365***	-2.363***	-2.365***	-2.379***	-2.358***	-2.556***	2.526**	-2.663***	1.639**
	(-8.52)	(-8.78)	(-8.68)	(-8.82)	(-8.83)	(-8.57)	(-7.82)	(2.40)	(-5.35)	(2.07)
Sales Growth	-0.102**	-0.102**	-0.102**	-0.102**	-0.101**	-0.103**	-0.173***	0.0528	-0.142**	0.0333
	(-2.18)	(-2.22)	(-2.22)	(-2.21)	(-2.20)	(-2.18)	(-3.62)	(0.43)	(-2.10)	(0.33)
Largest Shareholder	-0.672***	-0.672***	-0.668***	-0.672***	-0.707***	-0.702***	-1.865***	2.913***	-2.380***	2.665***
	(-3.78)	(-3.76)	(-3.65)	(-3.77)	(-4.92)	(-4.78)	(-3.69)	(3.54)	(-3.85)	(3.63)
State Ownership	-0.261***	-0.259***	-0.259***	-0.259***	-0.249***	-0.236**	1.252***	-1.553***	0.067	-1.110***
	(-2.61)	(-2.66)	(-2.65)	(-2.67)	(-2.66)	(-2.50)	(2.71)	(-2.80)	(0.20)	(-2.81)
Abnormal Ind.								-0.004		0.0204
Litigation								(-0.82)		(0.92)
Disastrous Stock								0.027*		0.122
Return								(1.73)		(1.35)
Abnormal Stock								-0.003		0.122**
Turnover								(-0.26)		(2.09)
Industry Dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
No. of Obs.	8,148	8,148	8,148	8,148	8,435	8,435	8,063	8,063	8,063	8,063
No. of Fraud Obs.	353	353	353	353	369	369	364	364	364	364
% of Fraud Obs.	4.33%	4.33%	4.33%	4.33%	4.37%	4.37%	4.51%	4.51%	4.51%	4.51%
Log likelihood	-1255.6	-1255.6	-1255.6	-1255.6	-1305.0	-1304.2	-1533.2	-1533.2	-1533.7	-1533.7

Appendix Table 8

Descriptive statistics on foreign bank presence.

This table describes foreign bank presence following financial liberalization. The sample consists of loans granted to firms listed on Mainland China's stock exchanges after banking liberalization during the 2000 to 2010 period. Panel A presents the total number and value of foreign loans issued by the top 15 banks in our sample following liberalization. Panel B presents the total number of foreign banks and foreign loans in pilot cities during the period after liberalization. Panel C presents the number of firms with new loans and among them the percentage of firms with new foreign loans during each year after banking liberalization.

Panel A: **Top 15 foreign banks.**

Rank	Bank	Country	Value of Foreign Loans (in *billion* RMB)	No. of Foreign Loans
1	HSBC	U.K.	110.0	518
2	The Bank of East Asia	Hong Kong	30.0	131
3	OCBC Bank	Singapore	24.0	136
4	Standard Chartered Bank	U.K.	24.0	89
5	Citibank	United States	14.0	237
6	Nanyang Commercial Bank	Hong Kong	8.8	93
7	ABN AMRO Bank	Netherlands	8.3	18
8	Chinese Mercantile Bank	Hong Kong	7.8	41
9	Development Bank of Singapore	Singapore	6.4	53
10	Hang Seng Bank	Hong Kong	4.7	72
11	ANZ Bank	Australia	4.5	42
12	Hana Bank	South Korea	4.1	35
13	United Overseas Bank	Singapore	3.6	52
14	BNP PARIBAS	France	3.1	33
15	J.P. Morgan	United States	2.5	15

Panel B: **Distribution of foreign banks and foreign loans by pilot cities.**

City	Liberalization Year	No. of Foreign Banks	No. of Foreign Loans
Shenzhen	2001	33	475
Shanghai	2001	33	144
Beijing	2004	22	103
Tianjin	2001	16	64
Guangzhou	2002	16	44
Zhuhai	2002	11	31
Fuzhou	2003	11	31
Xiamen	2004	8	21
Nanjing	2002	5	17
Wuhan	2002	5	12
Kunming	2004	5	6
Qingdao	2002	4	7
Chengdu	2003	4	7
Shantou	2005	4	6
Shenyang	2004	4	5
Dalian	2001	3	18
Harbin	2005	3	7
Ningbo	2005	3	4
Nanning	2005	3	3
Chongqing	2003	2	4
Jinan	2003	2	3
Changchun	2005	1	7
Xian	2004	1	1
Lanzhou	2005	1	1
Yinchuan	2005	0	0

Panel C: **Change in percentage of firms with new foreign loans.**

Year after Liberalization	No. of Firms with New Loans	% of Firms with New Foreign Loans
0	110	0.91%
1	237	3.38%
2	376	2.66%
3	435	2.30%
4	445	3.15%
5	518	5.60%
6	867	5.42%
7	1,012	9.29%
8	1,327	9.87%
9	1,557	11.69%

Appendix Table 9

Control for Special Economic Zone.

This table examines the impact of banking development (Columns 1–2) and liberalization (Columns 3–4) on corporate fraud after controlling for Special Economic Zones. The sample consists of firms listed on Mainland China's stock exchanges during the 2000 to 2010 period. The dependent variable is one if a firm is detected committing financial misreporting in a specific year by CSRC, and zero otherwise. *Banking Development* is the provincial index of banking industry marketization by Fan, Wang, and Zhu (2011). *Financial Liberalization* is an indicator variable that equals one if a firm is located in a city that allows foreign banks to conduct local currency-related business in a specific year, and zero otherwise. *Special Economic Zone* is an indicator variable that equals one if a firm is located in the Special Economic Zone (i.e., the city of Shenzhen, Zhuhai, Xiamen, and Shantou and Hainan province). Other explanatory variables are defined in Online Appendix B. All regressions include industry and year dummies. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Dependent variable: whether a firm is detected committing fraud			
Explanatory variables	(1)	(2)	(3)	(4)
Banking Development	-0.0647***	-0.0654***		
	(-6.00)	(-5.74)		
Financial Liberalization			-0.263***	-0.279***
			(-3.04)	(-2.93)
Size	-0.125***	-0.124***	-0.117***	-0.116***
	(-4.67)	(-4.66)	(-4.08)	(-4.19)
Leverage	0.337***	0.337***	0.320***	0.318***
	(5.46)	(5.56)	(5.81)	(5.87)
ROA	-2.377***	-2.377***	-2.355***	-2.357***
	(-8.86)	(-8.90)	(-8.82)	(-8.80)
Sales Growth	-0.101**	-0.101**	-0.113**	-0.114**
	(-2.20)	(-2.20)	(-2.50)	(-2.50)
Largest Shareholder	-0.705***	-0.703***	-0.669***	-0.678***
	(-4.83)	(-4.71)	(-5.34)	(-5.60)
State Ownership	-0.252***	-0.248***	-0.258***	-0.256***
	(-2.67)	(-2.73)	(-3.23)	(-3.27)
Special Economic Zone		0.036		0.128
		(0.28)		(0.84)
Industry Dummies	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y
Province Dummies	N	N	Y	N
No. of Obs.	8,435	8,435	8,435	8,435
No. of Fraud Obs.	369	369	369	369
% of Fraud Obs.	4.37%	4.37%	4.37%	4.37%
Pseudo R-sq	0.138	0.138	0.153	0.153

Appendix Table 10

Subsample analysis by firm size.

This table examines the differential impact of banking development (Columns 1 and 3) and banking liberalization (Columns 2 and 4) on corporate fraud by comparing the full sample and the subsample of small firms. The full sample consists of firms listed on Mainland China's stock exchanges during the 2000 to 2010 period. The subsample of small firms consists of firms whose logged value of total assets are below the sample median. The dependent variable is one if a firm is detected committing financial misreporting in a specific year by CSRC, and zero otherwise. *Banking Development* is the provincial index of banking industry marketization by Fan, Wang, and Zhu (2011). *Financial Liberalization* is an indicator variable that equals one if a firm is located in a city that allows foreign banks to conduct local currency-related business in a specific year, and zero otherwise. Other explanatory variables are defined in Online Appendix B. All regressions include industry and year dummies. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Dependent variable: whether a firm is detected committing fraud			
	Full Sample		Subsample of Small Firms	
Explanatory variables	(1)	(2)	(3)	(4)
Banking Development	-0.065***		-0.0942***	
	(-6.00)		(-3.33)	
Financial Liberalization		-0.263***		-0.453**
		(-3.04)		(-2.40)
Size	-0.125***	-0.117***	-0.115*	-0.04
	(-4.67)	(-4.08)	(-1.93)	(-0.45)
Leverage	0.337***	0.320***	0.848***	0.321***
	(5.46)	(5.81)	(4.60)	(3.77)
ROA	-2.377***	-2.355***	-2.911***	-2.115***
	(-8.86)	(-8.82)	(-4.34)	(-10.19)
Sales Growth	-0.101***	-0.113**	-0.082**	-0.109**
	(-2.20)	(-2.50)	(-2.01)	(-2.02)
Largest Shareholders	-0.705***	-0.669***	-0.622*	-0.539
	(-4.83)	(-5.34)	(-1,84)	(-1.44)
State Ownership	-0.252***	-0.258***	-0.290**	-0.443***
	(-2.67)	(-3.23)	(-2.18)	(-3.50)
Industry Dummies	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y
Provincial Dummies	N	Y	N	Y
No. of Obs.	8,435	8,435	3,812	3,735
No. of Fraud Obs.	369	369	247	247
% of Fraud Obs.	4.37%	4.37%	6.48%	6.61%
Pseudo R-sq	0.138	0.153	0.114	0.140

Appendix Table 11

Client of Foreign versus Domestic Banks.

This table compares the characteristics of foreign versus domestic bank clients. The full sample consists of firms listed on mainland China's stock exchanges during the period 1990 to 2010. Probit estimation results based on the full sample and the subsamples following banking liberalization are presented in Columns (1)-(2), and (3)-(4), respectively. The dependent variable is an indicator variable that equals one if a firm is foreign bank client, and zero otherwise. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Dependent Variable: Foreign Bank Client Indicator			
	Full Sample		Subsamples After Liberalization	
Explanatory variables	(1)	(2)	(3)	(4)
Asset Tangibility	0.864**	0.303	0.864**	0.277
	(2.23)	(0.69)	(2.34)	(0.60)
Leverage	0.318***	0.282**	0.294***	0.257**
	(3.06)	(2.35)	(2.76)	(2.08)
Sales Growth	0.0691*	0.0668*	0.105**	0.100**
	(1.83)	(1.88)	(2.41)	(2.41)
ROA	1.430***	1.659***	1.498***	1.744***
	(3.02)	(4.23)	(2.64)	(3.52)
Size	0.170***	0.186***	0.176***	0.198***
	(4.77)	(6.47)	(4.85)	(7.35)
Largest Shareholder	0.513*	0.501*	0.680***	0.669***
	(1.77)	(1.75)	(2.64)	(2.59)
State Ownership	-0.587***	-0.488***	-0.667***	-0.563***
	(-4.83)	(-4.66)	(-4.61)	(-4.05)
Industry Dummies	N	Y	N	Y
Year Dummies	Y	Y	Y	Y
No. of Obs.	7,706	7,706	5,977	5,977
No. of Fraud Obs.	334	334	241	241
% of Fraud Obs.	4.33%	4.33%	4.03%	4.03%
Adjusted R-sq	0.035	0.049	0.043	0.060

Appendix Table 12

Subsample analysis for firms with and without increasing long-term loans.

This table analyzes the impact of banking liberalization and long-term loans. The dependent variable is one if a firm is detected committing financial misreporting in a specific year by CSRC, and zero otherwise. *Financial Liberalization* is an indicator variable that equals one if a firm is located in a city that allows foreign banks to conduct local currency-related business in a specific year (i.e. foreign bank entry), and zero otherwise. The sample consists of firms listed on mainland China's stock exchanges during the period 1990 to 2010. Columns (1)– (3) present probit regression results based on the full sample and the subsamples of firms with and without increasing long-term loan ratios after banking liberalization, respectively. *Long-Term (Short-Term) Loan Ratio* is defined as long-term (short-term) loans divided by total assets. Robust standard errors are clustered at the industry and province levels with *t*-statistics reported in parentheses. Coefficients marked with *, **, and *** are significant at the 0.1, 0.05, and 0.01 levels, respectively.

	Dependent variable: whether a firm is detected committing fraud		
		Subsample analysis	
	Full sample	Firms with increasing long-term loan ratios	Firms without increasing long-term loan ratios
Explanatory variables	(1)	(2)	(3)
Financial Liberalization	-0.289***	-0.380***	-0.0913
	(-3.73)	(-3.39)	(-0.52)
Size	-0.109***	-0.0685**	-0.203***
	(-3.73)	(-2.34)	(-3.27)
Long-Term Loan Ratio	-0.579*	-0.687*	-0.328
	(-1.91)	(-1.79)	(-0.62)
Short-Term Loan Ratio	0.980***	1.221***	0.581
	(5.18)	(6.08)	(1.57)
ROA	-2.249***	-2.028***	-2.601***
	(-9.45)	(-8.42)	(-5.17)
Sales Growth	-0.112**	-0.078	-0.147***
	(-2.43)	(-1.28)	(-3.88)
Largest Shareholder	-0.704***	-0.446***	-1.450***
	(-6.28)	(-2.79)	(-5.26)
State Ownership	-0.174***	-0.240*	0.101
	(-2.69)	(-1.71)	(0.45)
Industry Dummies	Y	Y	Y
Year Dummies	Y	Y	Y
Province Dummies	Y	N	N
No. of Obs.	8,341	5,190	1,930
No. of Fraud Obs.	367	257	110
% of Fraud Obs.	4.40%	4.95%	5.70%
Pseudo R-sq	0.161	0.157	0.215

Appendix Table 13

Economic significance.

This table reports the economic significance of Banking Development, Financial Liberalization, and other control variables from our main specifications. In Panel A, Columns (1) and (2) report the probit regression coefficient estimates from Column (4) of Table 2, and marginal effects estimated at the means of the covariates in the model. Sample means and standard deviations of the explanatory variables are reported in Columns (3) and (4), respectively. In Columns (5) and (6), we present absolute and percentage changes in predicted fraud probabilities if we vary one explanatory variable from its mean to its mean plus one standard deviation, while holding other determinants at the means. Panel B reports the economic significance of Financial Liberalization from three different columns of Table 3. Column (1) reports the marginal effects of financial liberalization estimated at the means of the covariates. Columns (2)– (3) present absolute and percentage changes in predicted fraud probabilities if we vary financial liberalization from zero to one while holding other determinants at the means.

Panel A: Economic Significance from Table 2

	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variables	Probit coefficient estimate	Marginal effects	Mean	Std. Dev.	Absolute change in predicted probability	% change in predicted probability
Banking Development	-0.065	-0.004	8.860	2.734	-0.99%	-34.13%
Size	-0.125	-0.008	21.373	1.116	-0.81%	-27.88%
Leverage	0.337	2.22%	53.62%	27.57%	0.67%	23.13%
ROA	-2.377	-15.70%	4.44%	8.43%	-1.09%	-37.79%
Sales Growth	-0.101	-0.67%	2.73%	53.47%	-0.34%	-11.71%
Largest Shareholder	-0.705	-4.65%	38.21%	15.89%	-0.66%	-22.97%
State Ownership	-0.252	-1.66%	26.87%	25.69%	-0.40%	-13.88%

Panel B: Economic Significance from Table 3

	(1)	(2)	(3)
Explanatory variable	Marginal effects	Absolute change in predicted probability	% change in predicted probability
Financial Liberalization			
Model (1)	-0.016	-1.80%	-44.90%
Model (2)	-0.015	-1.47%	-52.09%
Model (3)	-0.016	-1.63%	-50.15%