FINANCIAL AND REAL EFFECTS OF GOVERNMENT MONITORING: EVIDENCE FROM COMMERCIAL BANK LOANS*

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I use a 2009 redesign of corporate tax enforcement in Ecuador to document that tax enforcement affects firms' cost of capital and real decisions in settings where agency frictions constrain lending. Firms included in a group that was disproportionately monitored, i.e., audited annually by the Ecuadorian tax authority, accessed significantly cheaper new bank debt despite paying more taxes. Additionally, monitored firms increased their investments in human and physical capital. I use a regression discontinuity design to control for selection bias in their decision about which firms to monitor. Finally, I provide evidence that tax enforcement reduces agency frictions between the firm and its lenders, which is reflected in the lower interest rate on new debt. The implications are that (1) credible tax enforcement can play a corporate governance role, and, (2) tax enforcement is an effective and fiscally positive way to stimulate firm investment where agency frictions constrain lending. *JEL Codes:* E22, E24, E26, G21, G38. *Keywords:* Monitoring; Real effects; Credit supply; Banking; Small firms; Emerging market; Large Taxpayer Unit; Corporate governance

I. Introduction

In a frictionless world, banks would have confidence that firms report their true financial state. They could rely solely on firm numbers to make intelligent estimates of the magnitude, timing, and uncertainty of future cash flows. They could then make wise decisions about whether to lend to a company, promoting the efficient allocation of capital. This does not describe the real world.

Firms themselves ultimately bear the resultant cost of enforcing financial contracts by paying a higher price for, or suffering constrained access to, external capital (Jaffee and Russell [1976]; Stiglitz and Weiss [1981]; Gale and Hellwig [1985]; Williamson [1987]).[1] Though they are often willing to voluntarily release information, banks are unlikely to know precisely

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1. Firms consistently name access to external financing as their greatest obstacle to growth. E.g., in World Bank Enterprise Surveys, which tabulates answers to this question from firms in more than 140 countries since 2006 (WBES [2006 - 2017]). In particular, respondents from Ecuador, the setting of this paper, report financial constraint across firm size, industry, and region.

the extent of their informational disadvantage. Caution, and regulation, dictates that they withhold credit when they lack enough credible data to perform a risk assessment.

Effective communication between firms and their lenders benefits from supervision by a credible party outside the relationship that ideally makes the verity of the firm's report independent of its intrinsic incentives to report truthfully; third-party monitors such as ratings agencies, public markets, other banks, and external auditors have all been shown to play this role. Yet developing countries lack many of these firm monitoring mechanisms, and when they do exist they are often less effective. For example, domestic auditors may be poorly policed or lack expertise (Schipper [2000]; Defond and Hung [2004]), while foreign auditors are expensive.[2] Poor oversight is exacerbated by expensive and inefficient courts (Brown, Cookson, and Heimer [2016]; Beck, Degryse, De Haas et al. [2018]). Even in developed countries private firms often do not issue audited financial statements and auditors have signed off on fraudulent filings. In this common situation, the government itself may act as a third-party monitor through its rule-making and enforcement roles, notably via tax enforcement. This is because there exists complementarity between firm actions the bank would prevent, such as to divert firm value toward controlling shareholders, and firm actions to reduce corporate tax liability (Desai, Dyck, and Zingales [2007]).[3] When banks know that government tax authorities are auditing firms to increase tax collection they can rely on those audits to verify—or challenge—financial claims, including firm revenues and profits.[4]

Ecuador offers a clean setting for studying the real and financial effects of government monitoring because a 2009 reform designed to increase tax revenue introduced quasi-random variation in the distribution of tax auditing resources across firms. As a result, some firms were disproportionately monitored, which means in this case they were audited annually by the tax authority independent of whether they appeared to be evading taxes. Otherwise similar firms were less monitored. Moreover, the Ecuadorian tax authority, the Servicio de Rentas Internas (SRI), intentionally designed its selection mechanism to be relatively impervious to firm manipulation. Specifically, they centralized the choice of which firms to monitor and kept the decision rule unpredictable. This minimized concerns about corruption and fraudulent disclosure by firms attempting to orchestrate their monitoring status. Moreover these measures were effective: tax revenue increased and I find no evidence of firm manipulation of the monitoring choice. Thus, I am able to translate the SRI's selection criteria into a fuzzy regression discontinuity design to estimate the effect of government monitoring on the cost of new bank debt for firms added to its program between 2010 and 2015.

I find that banks offer newly monitored firms more loans at a lower price, behaving as if they were receiving new information. Specifically, I find that the average firm monitored by the SRI paid a 17.9% lower average, loan-size weighted, interest rate for new bank debt versus the firms in the control group just to the left of the normalized cutoff. Moreover, the average monitored firm greatly increased the amount of bank debt that it borrowed; banks lent approximately 27% more in total new bank debt, relative to the firm's outstanding debt in the pre-period, to monitored firms than to control. Next, I estimate the effect of SRI monitoring on the investment and employment decisions of firms. I find that the average

2. For example, see Fortin and Rahman [2004] on the auditing industry in Ecuador.

3. Ecuadorian firms usually have concentrated ownership structures, suggesting that conflict between firm insiders and outsiders is the important agency friction. E.g., the average (median) manager in Ecuador over 2012 to 2016 held 51% (55%) of their company. The average (median) Ecuadorian firm over the same period had 7 (2) shareholders (predominately private investors). Ecuador is not unique; in most countries the majority of firms are closely-held (La Porta, Lopez-de Silanes, and Shleifer [1999]).

4. While I will consider the effect of government monitoring on bank lending, the same argument pertains to firm's relationships with outside capital providers other than banks.

treated firm increased employment growth by an additional 17% over the employment growth in control firms just to the left of the cutoffs. When I estimate by firm size quintile, I find that the effect attenuates in firm size. I also find that a significant proportion of the increase is from employees that were not formally working for a prior firm, suggesting this is an upper bound on the "true" employment effects. Moreover, the mean treated firm increased its physical investment. Its value of Property, Plant, and Equipment (PP&E), scaled by total assets, grew by 10% more than for control firms to the left of the cutoff and within MSE-optimal bounds. Non-tangible investment was not significantly impacted by monitoring. The fact that government monitoring affected real firm decisions suggests that bolstering the efficacy, and credibility, of government monitoring is a fiscally responsible mechanism to enhance credit allocation toward productive investment.

What is the mechanism behind these effects? Monitored firms pay a higher effective tax rate and increase the amount of new bank debt that they borrow. Since loan interest is deductible in Ecuador, the main mechanism could be demand for a higher tax shield (Modigliani and Miller [1958]). But in that case we would not expect a lower interest rate after controlling for loan size. On the other hand, we know that firms themselves ultimately bear agency costs in equilibrium through a higher cost of external capital (Jensen and Meckling [1976]; Diamond [1984])). Thus, a lower cost and higher quantity of bank debt is clear evidence that agency frictions between firms and their lenders decreased after firms began to be audited by the SRI each year. By agency friction, I mean hidden type/adverse selection and hidden action/moral hazard frictions. Theoretically we would expect both frictions to decrease from credible government monitoring. Indeed, I find evidence consistent with reduction in both, though results suggest that the major driver was a reduction in hidden action frictions. Specifically, and consistent with theory, the evidence suggests that the main channel is that the bank is pricing in that monitored firms will now be constrained by SRI scrutiny from extracting value from the firm, which could be otherwise used to pay off debt, cushion default risk, or indeed pay taxes (lower bank exposure to firm regulatory risk).

First, I find that the cost of borrowing was lower for firms that had observationally more severe agency frictions ex ante, e.g., younger, smaller, more financially constrained firms and those headquartered outside the major urban centers. Second, I find that new capital not only increased on the intensive but also on the extensive margin—monitored firms were more likely to borrow from new banks and new private investors. Third, I find that it was firms that the tax authority believed were previously diverting more that experienced a larger decrease in the price of new bank debt after they were chosen for the always-monitored group. Banks did not observe SRI proxies for tax evasion, so this suggests that banks also had a good idea about firm type before treatment assignment and were already pricing it in when setting loan interest rates (Artavanis, Morse, and Tsoutsoura [2016]). I find corroborating evidence that supports this conjecture. Firms that are more likely to have manipulated their financial and tax statements prior to being monitored, in that those statements do not follow Benford's Law—the distribution of the first non-zero digits that would theoretically hold for un-manipulated financial statement numbers—also borrow at lower rates than the average treated firm once they begin to be monitored. This is consistent with banks responding to an increase in the cost of firm hidden actions against the interests of its creditors from credible government monitoring by decreasing firm cost of bank capital (Desai, Dyck, and Zingales [2007]; Sufi [2009]).

As counterintuitive as it first appears, increasing tax oversight seems to create transparency regarding business financial reporting. That, in turn, boosts bank confidence in the monitored borrower and, therefore, credit availability. Access to more plentiful but less ex-

pensive loans encourages business growth, including employment. The policy implication for Ecuador and similar developing nations is that better tax oversight can improve credit access and growth agendas. In particular, monitoring might increase transparency for private firms with little transactional history, and thus possibly make loans more available for young and small corporate borrowers. Another implication is that governments could conceivably lower tax rates without loss of revenues if the enhanced oversight brings in funds that previously leaked through fraud or sloppy accounting. To the extent that firms grow as the result of improved access to credit, this would also improve future tax revenues. Further study of this channel, therefore, appears to be worthwhile given the extensive use of LTU programs around the world, and the concurrent benefits to governments, firms, and banks.

The main threat to a causal interpretation of these results is that firms may be able to perfectly manipulate their ranking score so that they can choose whether to be included in the monitored group. If this were the case then precisely those firms that would most benefit from the costly quality signal that the SRI provides, e.g., firms with profitable investments that need more bank financing, would choose to be treated. This would bias estimates away from finding no effect. I find no evidence of bunching in the density of the assignment variable around the cutoffs. This test supports the assumption that firms were not able to perfectly manipulate their treatment status (McCrary [2008]). Moreover, I confirm continuity in variables that affect firms' cost of capital at the treatment cutoff in the year before treatment assignment. These tests provide confidence in my assumption that the potential outcomes of monitored firms did not differ in a manner that is correlated with their cost of bank capital or real firm decisions in the pre-period around the thresholds. Taken together, they support a causal interpretation of the results. I further show robustness along a number of dimensions, including estimation with a variety of controls for credit access and investment, by confirming that results are not sensitive to estimation with parametric versus local linear regression specifications, or to estimation within varying bandwidths. I also find that results hold if I estimate using pooled data or separately for each treatment cohort and that there is no evidence of an effect around placebo cutoffs. I conclude that the main results appear robust and unlikely to be primarily driven by bias in the estimates.

A large literature (beginning with La Porta, Lopez-de Silanes, Shleifer et al. [1997], (1998)) documents a connection between good government institutions and capital market access and cost of capital, both across and within countries (see recent surveys Demirgüç-Kunt and Levine [2018] for the economics literature and Leuz and Wysocki [2016] for the accounting literature). Some papers in this literature find that the financial market benefits of stronger contract and legal enforcement extend to economic outcomes (e.g., Levine [1998], Levine [1999]; Levine, Loayza, and Beck [2000]; Bekaert and Harvey [2003]), while others find limited real effects from stronger contracting institutions (e.g., Acemoglu and Johnson [2005]). This paper utilizes a clean shock to tax enforcement to contribute within-country evidence in support of the link between firm transparency and both cost of capital and economic outcomes. In doing so, it joins a handful of studies that provide identified, microeconomic evidence for specific mechanisms highlighted in the macroeconomic, cross-country analyses, including private property protection (e.g., Claessens and Laeven [2003]; Berkowitz, Lin, and Ma [2015]); investor protection (e.g., McLean, Zhang, and Zhao [2012]; Brown, Martinsson, and Petersen [2013]); and well-functioning courts (Ponticelli and Alencar [2016]; Brown, Cookson, and Heimer [2016]. These two strands of the wider law and economics literature are mutually reinforcing, with the cross-country literature providing external validity and strong evidence that institutions have macro importance, while the micro, within-country evidence isolates particular mechanisms, addresses reverse causality and

other endogeneity concerns, and shows that the relationship with capital and real outcomes is economically meaningful at the firm level.

The closest paper to this one in this literature is Colonnelli and Prem [2019], which considers the effect of government corruption audits of firms with public contracts in Brazil on local economic outcomes. Relative to their paper, I use a government auditing program that did not condition treatment on fraud and that assigned treatment in a manner that produces a natural control group within localities. Moreover, my data allow me to trace out the capital market and real effects at the company level for a wide cross-section of the firm distribution in Ecuador. The mechanism of the effect that I find is a reduction in agency frictions between firms and their lenders from government monitoring, while Colonnelli and Prem [2019] find that of their effects are driven by the ability of corrupt firms to stop making payments to government officials. Nevertheless, that both papers produce local estimates tightly connected to the institutional details of the setting and yet find real effects from government audit programs enhances the external validity of each.

This paper also contributes to the literature on the corporate governance view of taxes following Desai, Dyck, and Zingales [2007], who build a model relating tax enforcement to firms' cost of equity capital via a corporate governance channel in which an increase in enforcement reduces the capacity of firm managers to extract private benefits at the expense of outside shareholders, increasing the equity value of firms. Another close paper in this literature, Guedhami and Pittman [2008], finds a negative association between the intensity of Internal Revenue Service (IRS) audits at the county level and the yield spreads on the 144A bond issuances of privately-held US firms. Their results suggest that government monitoring via tax enforcement is valued even by qualified institutional buyers in the United States, where capital markets and private markets are much more highly-developed than in my setting. Using a setting with quasi-random variation in the probability a firm is monitored by the Ecuadorian tax authority, I provide evidence for the corporate governance role of tax enforcement, directly show that tax receipts are also augmented by monitoring, and find that the magnitude of the effect on firm of cost of capital is meaningful. I provide evidence that the mechanism of the effect is that tax monitoring decreases managers' ability to divert firm resources in a way visible to firm lenders. Finally, I find that in Ecuador tax monitoring also impacts real firm decisions over investment and employment.

This paper also contributes to the literature that examines the effects of size-dependent tax enforcement on firm behavior. This literature focuses on evasion around monitoring thresholds both from theoretical (Keen and Mintz [2004]; Dharmapala, Slemrod, and Wilson [2011]; and Kanbur and Keen [2014]) and empirical perspectives. In particular from the empirical evidence, Aparicio and Oliva [2010] and Aparicio [2012] use the rule by which the Ecuadorian tax authority assigned firms to monitoring in 2008 in the province of Pichincha to show that monitoring increases tax collection, both from monitored firms and their suppliers. Another close paper, Almunia and Lopez-Rodriguez [2018] use the monitoring assignment rule in Spain to examine the effect of tax enforcement on firm tax compliance. They also demonstrate a complementarity between monitoring effort and the quality of firm-level information. This paper provides evidence that there are substantial gains from monitoring of firms by the fiscal authority beyond increasing tax revenues. Moreover, much policy talk debates whether lowering taxes stimulates firm investment, but this paper suggests that enforcing tax payments, even with official rates held constant, can stimulate investment in opaque firms even when firm's effective tax rate is weakly higher after enforcement increases.

Finally, I contribute to the literature on the real effects of credit supply shocks (see survey Güler, Mariathasan, Mulier et al. [2019]). Only a few papers in this literature investigate

the real effects of positive shocks. The existing evidence suggests the effect is context-dependent, with some studies showing positive effects while others show no or negative real effects. Acharya, Eisert, Eufinger et al. [2018] and Giannetti and Simonov [2013] show that an increase in credit availability can lead to zombie lending when it occurs at a time of a general dearth of credit, as was the case during the European debt crisis and after bank bailouts in Japan, respectively. These papers find no effects on firm investment. Bai, Carvalho, and Phillips [2018], Ferrando, Popov, and Udell [2019], and Morais, Peydró, Roldán-Peña et al. [2019] examine positive credit supply shocks that improve firm outcomes, especially for young and small firms. My results add to the evidence that there can be meaningful positive effects of a positive credit supply shock for both firm employment and investment. I providee evidence on the settings where we would expect real effects, namely in markets characterized by high agency frictions between credit suppliers and borrowers, especially where it is difficult for lenders to monitor firm actions or enforce their rights ex post. Moreover, the magnitudes of the effects should be larger where there is widespread financial constraint and a large informal economy, such as in Ecuador in my study or in Mexico as in Morais, Peydró, Roldán-Peña et al. [2019].

II. Institutional setting

II.A. *Tax monitoring in Ecuador*

Ecuador historically had difficulty collecting taxes. For example, in 2010 the tax commissioner, Carlos Carrasco, estimated that income tax evasion was approximately 45% and evasion of the value-added tax (VAT) was approximately 21%.[5] One strategy the Ecuadorian government adopted to strengthen its fiscal capacity was to reform their implementation of an institution internationally known as a "Large Taxpayer Unit," a sub-group within the larger SRI tasked with choosing a subset of firms to allocate more auditing resources to monitoring. Over 90 other countries use some version of this tax monitoring strategy (Bachas, Jaef, and Jensen [2018]).[6] In Ecuador firms monitored by this group were designated contribuyente especiales, or "special taxpayers." Prior to 2009, there was a separate Special Taxpayer Auditing group in each province. Each operated with little direction from the central tax authority, used their own procedures for choosing whom to audit, and most had low reputations for credible enforcement. In 2009, the SRI consolidated the auditing function to one group in the capital Quito and gave it over 70% of auditing resources (see Table I, Column 5). At the same time, a new law went into effect that increased the penalties that the SRI could unilaterally apply for tax evasion.[7]

[Place Table I here.]

The newly consolidated and empowered Special Taxpayer Auditing unit developed a system for choosing which firms to audit with the aim of maximizing tax revenue while limiting firms' ability to manipulate the process. First, The SRI assigned each formal firm an index value that was a weighted sum of firm observables from the year prior. The inputs to the index varied every year, but generally they were proxies for each firm's importance

5. "Carrasco: Evasión fiscal se ubica en un 45%," published in the newspaper El Universo on April 26, 2010. (last accessed online 7/22/2018).

6. What is common across countries is that the auditing sub-group within the country's tax authority uses some form of assignment rule, usually a ranking rule based on firm asset size, along with a cutoff, to identify the most important taxpayers and allocate more tax auditing resources to monitoring them.

7. the "Ley Reformatoria Para La Equidad Tributaria del Ecuador." Violators could be fined, have their business license suspended, and individuals could be imprisoned.

within its supply chain and as a source of tax revenue within its province. For example, in 2010 the index was calculated as:

$$\begin{aligned} Index_{j,t=2010} = {} & weight_1 max\left\{Sales_{j,t=2009}, Costs_{j,t=2009}\right\} \\ & + weight_2 (Num.\ Suppliers\ \&\ Customers)_{j,t=2009} + weight_3 Taxes_{j,t=2009} \end{aligned} \tag{1}$$

Where *Sales* is the firm's percentile rank within its province of its reported sales from 2009; *Costs* is the firm's percentile rank of its reported costs; *(Num. Suppliers & Customers)* is the firm's percentile rank of the number of firms firm j transacts with over the previous year; and *Taxes* is the firm's percentile rank of the firm on total taxes paid through that firm, including taxes withheld at the transaction level owed by the firm's counterparties. Second, each firm's index value was transformed into a percentile ranking within that firm's province of incorporation, hereafter referred to as the "firm ranking variable." Third, a province-specific cutoff was applied.

The cutoff thresholds qualified more firms for auditing than the SRI had resources to effectively monitor. The auditing process the SRI developed was resource-intensive. Firms chosen for auditing were each assigned an auditing team with wide powers to gather information from the firm and firm stakeholders, including suppliers, customers, banks, and employees. Moreover, once a firm was designated a Special Taxpayer it was audited every year. Thus, while all firms had some probability of being audited by the SRI, there was substantially stronger monitoring for firms monitored by the Special Taxpayer Auditing program. The Special Taxpayer Auditing program individually considered all firms with values of the firm ranking variable that were above the cutoff for inclusion in the monitored group.[8]

Finally, the list of all special taxpayers was published on the SRI website and received wide-spread coverage in the major national newspapers. Thus, which firms were actually chosen to be special taxpayers was common knowledge while the cutoffs and firm rankings were not known outside of the Special Taxpayer unit, and indeed the inputs to the index and the province-specific cutoffs, and the proportion of firms chosen from each province, varied every year to prevent firms from intuiting their values over time.[9] I translate these institutional details into a fuzzy regression discontinuity design (RDD), identifying off of a jump in the probability of treatment when the firm's ex-ante ranking value falls just to the right of its province cutoff relative to otherwise similar firms to the left.[10] Before I describe how I implement the RDD, I confirm that the program was effective at raising taxes, i.e., that monitoring was credible, and that firms could not manipulate their ranking values.

II.B. Monitored firms pay more taxes

Were tax audits effective at raising tax revenue? Table I shows us that although the firms monitored by the Special Taxpayer Auditing program were only around 4—5% of all formal firms in the economy, the SRI collected over 80% of all corporate tax revenue through

8. This is analogous to admissions decisions at many U.S. universities where a test score cutoff is applied and then each applicant above that cutoff is considered individually for a limited number of available spots.

9. A consequence is that there is wide variation in the size, age, etc. of special taxpayers in my sample, as a firm important in a small and relatively poor province is smaller than many unmonitored firms headquartered in a large and prosperous one with major urban centers. Indeed the average Special Taxpayer was not among the largest firms in Ecuador if they were ranked nationally on asset size, as is the typical procedure in other LTUs. This is especially true when we note that many of the largest firms in Ecuador are state-owned enterprises, which are not eligible to be special taxpayers.

10. Note that this does not rely on a certainly erroneous assumption of randomness in the SRI's choice of which firms with ranking variables to the right of the cutoff would be audited.

these firms. This effectiveness comes from the combination of careful scrutiny of the most interconnected firms and a tax withholding system. This is key to the effectiveness of the SRI's strategy. The monitored firms were generally "node" firms within their local supply chain. Because Ecuador collects most corporate tax income at the transaction level, by mandating that the purchasing firm withhold a portion of income tax VAT from payment and remit it to government, the SRI could focus monitoring on a minority of firms with many suppliers and ensure they collected most of the corporate tax owed by the rest of the firms in the economy.[11] The monitored firms were motivated to ensure their suppliers paid the correct taxes both because they would have to provide supporting documents for the tax audits and because they received tax credits against their own liabilities.

Moreover, the SRI was able to use the information on other firms' revenues from these transactions to better allocate the remainder of their auditing resources. They summed over the transactions to make a lower-bound estimate of true firm sales and costs for every formal firm in the economy. The SRI then audited firms that reported significantly below the lower-bound estimate (see Carrillo, Pomeranz, and Singhal [2017] for more details). Thus, the Special Taxpayer Auditing program allowed the Ecuadorian tax authority to concentrate its monitoring resources on a few firms that purchased inputs from many other firms, and through them collect much of the taxes owed from the rest. Finally, we can see from Table II that treated firms also payed higher taxes on average, further strengthening the assertion that they were effectively monitored. The average treated firm paid approximately $7,000 more in corporate income taxes per year than control firms just to the left of the cutoff.

[Place Table II here.]

II.C. Firms cannot choose their monitoring status

Was the SRI successful in preventing firms from gaming the process and choosing whether they would be monitored or not? I look for firm manipulation of the assignment decisions by testing for bunching in the distribution of firms by their firm ranking variable. I first pool across all years and all provinces by subtracting the year-province-specific cutoff from each firm's running variable value so that all cutoffs are zero. The left panel of Figure I reports that histogram within a bandwidth of 0.01 on either side of the normalized cutoff. Visual inspection reveals that the distribution of firms is relatively smooth around the cutoff. More formally, McCrary [2008] provides a statistical test of continuity of the density of firms at the cutoff. The right panel of Figure I displays local polynomial density estimates, and associated, point-wise robust confidence intervals around the normalized cutoff. I perform a two-sided test for a discontinuity between the two densities and obtain a p-value of 0.899. Therefore, I cannot reject the null that the estimated test statistic is statistically zero. This is direct evidence supporting the assertion that firms could not perfectly manipulate their rankings to increase their probability of treatment.[12]

[Place Figure I here.]

Note that, although firms were not able to choose if they were included or removed from the program, the Special Taxpayer Auditing program itself did remove firms from the monitoring program when it was decided the firm was no longer important enough to justify the resources expended on monitoring it, or if the firm declared bankruptcy, had its

11. For the majority of firms in Ecuador, 100% of the taxes owed are withheld at the transaction level.
12. See Online Appendix Figure A1 for the same figure, and its associated test statistic, estimated for each treatment cohort separately.

operating license suspended by court order, or was acquired. Panel A and Table III presents the frequency by treatment cohort of entry and exit from the monitoring group, as well as the number that remained in the sample after removal. Since the decision of which firms to remove was not rules-based, it could affect the results. To address this, I count a firm as treated if it is ever selected into the Special Taxpayer monitoring program within my sample, even if it is later removed. Thus, I estimate an average intent-to-treat effect. To the extent that some firms I count as treated are not receiving treatment, this will bias my results towards finding no effect. To make sure there is no selection effect from treated firms dropping out of the sample more often than untreated ones, I test for differential attrition by treatment status in Panel B of Table III. In the full sample, before any data filters are applied, the treated are less likely to drop out than the untreated. Around the threshold we cannot reject the hypothesis of no difference in attrition rates.

[Place Table III here.]

III. Data

The base of my data is the confidential annual selection criteria from the Special Taxpayer Auditing program over the period 2010 to 2015. I observe the value, inputs, and weights of the firm index, firm percentile rankings, the province-year thresholds applied on that ranking, as well as notes on the firm-by-firm inclusion decision for the firms that were above the threshold. I combine this data with balance sheet and income statement data for all formal firms over the period 2009 to 2018.[13] This data is publicly available on the website of the Ecuadorian company regulator (the Superintendencia de Compañías) for all incorporated firms through a company-by-company lookup tool. I accessed the filings from an anonymized database at the SRI that allowed me to merge it with other internal datasets, such as daily inter-firm transaction data, aggregated to the firm-year level, and SRI internal measures of firm tax evasion risk.

I use a common anonymized firm identification to merge with loan-level data that was reported by the banks to the Ecuadorian bank authority (the Superintendencia de Bancos y Seguros) on a quarterly basis.[14] These data represent the universe of new and outstanding commercial bank loans from all banks operating in Ecuador from 2010 to 2018. There were 24 private Ecuadorian banks, two state-owned banks, and 2 foreign banks that lent commercial loans to Ecuadorian firms over my sample period. The only part of the formal lending market that I do not observe are loans from credit unions (cooperativas de ahorro y crédito), which have their own regulator. However, these were not major lenders to the firms around the Special Taxpayer decision cutoffs since they focus on micro loans to small businesses. The data include information on the loan amount, type, intended use, interest rate, term-to-maturity, and the internal bank risk rating at grant. I match this loan origination database to snapshots of bank loan performance at each quarter-end: amount outstanding, amount due, interest paid, any penalty interest or fees applied, a time-varying internal bank loan-level risk rating, the amount of the debt that the bank expects to lose before maturity, bank loan provisions, and, from 2012, the number of days the borrower is late on payments. Interest rates are spreads over the rate on Ecuadorian government bonds with the closest maturity. Both interest rates and maturities are aggregated to the firm-year level, the level

13. Across datasets, a firm is defined at the consolidated level.

14. Banks are the most important source of external financing in Ecuador. The two equity markets in the country (on which are also listed private debt issuances) are small and illiquid

of treatment, by weighting them by the size of the loan, i.e., by computing the weighted interest rate and the weighted average maturity.

I remove firms from the financial sector, state-owned enterprises, non-governmental organizations, foreign-owned subsidiaries, and firms chosen as special taxpayers before 2010. I also prune all firm-years with zero sales, where the firm does not file a tax report, or reported no net income. Since I estimate primarily within narrow bandwidths around cutoffs applied at the upper end of the firm distribution, results are not sensitive to these decisions. A less standard filtering decision is based on the observation that, for the assignment years 2012 to 2015, the SRI used a second ranking metric and province-year cutoff, in addition to the firm ranking variable I described above. The second running variable was based on total tax collected through each firm, in thousands of USD. This second cutoff was also applied at the year-province level. It was not, however, binding for most firms within the ranking variable bandwidths. In other words, there were very few treated firms just to the right of the second cutoff because most paid substantially more taxes than the cutoff. In contrast, the firm ranking variable cutoff was always at the far right tail of its distribution. Therefore, I require that firms included in my estimation sample, both treated and control, pay taxes at or above the tax threshold. I set that threshold at zero for assignment years 2010 to 2011 and use the actual year-province specific tax threshold for assignment years 2012 to 2015 (see the "uni-variate" method for estimating regression discontinuity with more than one threshold in Wong, Steiner, and Cook [2013]).[15]

The final dataset is arranged in stacked cohort panels. I construct a panel for each treatment assignment year, 2010 through 2015, that includes the year before to three years after treatment assignment. I fix the firms in the cohort sample in the year before treatment. I then pool across cohorts by defining time as years from treatment assignment, which I will refer to as $t = 0$, i.e., by imposing event time. Once a firm is treated it never re-appears in a later cohort but a firm that is not chosen can appear in later cohorts. The full, unfiltered dataset comprises 988,529 firm-years, with repetition across cohorts. After applying the above sampling restrictions, my final sample is approximately 180,000 firm-years representing about 22,000 firms.

Table IV reports summary statistics for the years before treatment assignment at event time $t = -1$ (2009 through 2014).[16] From Panel A of the summary table, we see that the average Ecuadorian firm in the years before treatment assignment (2009 through 2014) was comparable to a small or medium-sized enterprise in the U.S. context. The average (median) firm had about 492 (156) thousand USD in assets, approximately 614 (283) thousand USD in sales, and around 24 (9) employees. Keeping in mind that the average firm is quite small is helpful when interpreting the effect sizes and drawing implications from the results. Panel B of Table IV summarizes the financial ratios and indicators actually used by loan officers at the largest private banks in Ecuador. They proxy for firm profitability, liquidity, and default risk. Column 2 reports variable definitions. These observables are included as controls in all regressions and are used in placebo tests that check for predetermined differences in firms across the cutoffs.

[Place Table IV here.]

Finally, Panel C reports summary statistics for the sub-sample of firms that had newly-

15. The direction and significance of the main results, estimated within mean-squared-error-optimal bandwidths, do not change if I do not apply this filter, but the magnitude of the effects increases substantially for specifications using all of the data.

16. Note that Ecuador has used the U.S. dollar as its official currency since 2000. I convert all dollar figures into real, 2016 dollars by using Ecuador's Consumer Price Index.

granted commercial loans from Ecuadorian banks in the year before treatment assignment. The average (median) firm had 1.5 (1) bank relationships. From that bank, the average firm was granted about 3.3 (2) new loans. The average (median) loan size was approximately 104 (17) thousand dollars, carried a 13.4 (12) percent annualized interest rate, and had a loan-to-maturity at origination of 11.6 (6) months.[17] Online Appendix Table A1 displays descriptive statistics for the same variables within the MSE-optimal bounds calculated for the main variable of interest, the average weighted interest rate on new bank debt.

IV. Empirical setting

IV.A. Discontinuity in the probability of being monitored

Figure II shows the fraction of firms chosen to be monitored by the Special Taxpayer Auditing program, in percentile bins of the firm ranking variable. Visual inspection reveals this fraction is discontinuously higher for firms whose percentile ranking as of event time $t = -1$ is higher than the cutoff for their province. The figure also shows the ordinary least squares (OLS) fitted values and 95% confidence intervals of the regression:

$$\begin{aligned} Monitoring_{j,p,c,t=0} = \omega + \pi above_{j,p,c,t=-1} + \\ f(RV_{j,p,c,t=-1} - cutoff_{p,c,t=-1}) + \gamma_{c,p} + \zeta_{j,p,c,t=0} \end{aligned} \tag{2}$$

for firm j, event year t, cohort c, and province p. Where *Monitoring* is an indicator variable that equals one if the firm is assigned to Special Taxpayer monitoring in event year $t = 0$; *above* is an indicator variable that equals one if a firm's running variable value is higher than the province-event year specific cutoff; and $(RV - cutoff)$ is the normalized firm ranking variable. The polynomial *f*, depicted graphically as the smooth line on both sides of the cutoff, controls for any underlying relationship between the fraction of treated firms and the firm ranking variable. $\gamma_{c,p}$ are cohort times province fixed effects. The fixed effects control for general trends in the economy and ensure that all comparisons are of firms within the same province and year, i.e., the firms the SRI ranks them against when computing the firm ranking variable.

[Place Figure II here.]

Table V reports the OLS estimates of the coefficient π and the constant ω of Equation (2) for the pooled sample and for each treatment cohort separately. The coefficient π, which in the plot corresponds to the difference in the vertical axis between the points where the left and right polynomials intersect the cutoff, is a measure of the size of the discontinuity.[18] The pooled sample point estimates imply that ranking at the cutoff increases the probability of being monitored by the Special Taxpayer Auditing program by 24.9%, from a 2.7% probability just to the left of the cutoff. Note that the variation in the jump in the probability of treatment across cohorts comes from the variation in the number of firms the Special Taxpayer Auditing program wants to audit from each province in a given year (see Table III). Ultimately, this was due to the SRI's desire to keep the proportion of monitored firms approximately equal across the period.

17. We can compare these numbers to similar statistics for commercial loans from domestic U.S. banks that are backed by the Small Business Administration to gain context. The average loan size for SBA loans over the period 2009 to 2014 was 120 thousand dollars with a weighted-average effective loan rate of 4.59 percent and had a weighted average maturity at origination of 9.6 months. Source: Board of Governors of the Federal Reserve System (US), E.2 Survey of Terms of Business Lending.

18. Note that, because I remove already-treated from the sample, this is the probability of treatment conditional on not having been chosen for treatment before, and controlling for the ex-ante ranking variable.

[Place Table V here.]

IV.B. The fuzzy regression discontinuity design

In this section I describe how I translate the institutional details above into a fuzzy regression discontinuity design to estimate the causal effect of monitoring by the Special Taxpayer Auditing program on treated firms' cost of bank debt (Imbens and Lemieux [2008]; Lee and Lemieux [2010]; Roberts and Whited [2013]). Intuitively, I compare the outcome of firms whose firm ranking variable value is slightly higher than the cutoff with that of firms whose running variable value is slightly lower than the cutoff. I assign any difference in the cost of borrowing to the monitoring. The identifying assumption is that these two groups were statistically indistinguishable before the campaign. In the fuzzy RD setting, this is equivalent to assuming that the cross-sectional distribution of the unobserved residual is continuous at the firm ranking variable cutoffs. To fix ideas, consider the regression model:

$$Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} = \alpha + \beta monitoring_{j,p,c,t=0} + \epsilon_{j,c,p,t} \tag{3}$$

For firm j, event year t, cohort c, and province p. Y is the outcome, for instance the natural log of the average interest rate on new bank debt over the three years following the event year versus the year before the event year. *Monitoring* is an indicator variable that equals one if the firm is assigned to Special Taxpayer Auditing program monitoring in event year $t = 0$ and $\gamma_{c,p}$ are cohort times province fixed effects.

By substituting the first-stage, Equation (2), into the regression model, Equation (3), and relabeling coefficients and the functional form $\hat{f}$, I obtain the fuzzy regression discontinuity reduced form:

$$\begin{aligned} Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} = {} & \alpha_0 + \beta \times \pi \times above_{j,c,p,t=-1} \\ & + \hat{f}(RV_{j,c,p,t=-1} - cutoff_{c,p,t=-1}) + \gamma_{c,p} + \chi_{j,c,p,t} \end{aligned} \tag{4}$$

Note that, in its simplest form, the fuzzy regression discontinuity setting implements a Wald estimator for β. This estimator is equal to the coefficient on *above* in Equation (4), $\beta \times \pi$, divided by the coefficient on *above* in Equation (2), π. Thus, the fuzzy regression discontinuity procedure is equivalent to instrumenting *Monitoring* by *above*, conditional on $\hat{f}(RV - cutoff)$, (Hahn, Todd, and Van der Klaauw [2001]). I estimate this regression using a two-stage least squares (2SLS) procedure where Equation (2) is the first and Equation (3) is the second stage.[19]

IV.C. The identification assumption

Following Lee and Lemieux [2010], I assume that crossing the cutoff does not affect the firm's cost of bank debt other than through the change in the probability of being monitored by the Special Taxpayer Auditing program (excludability), and that no firm becomes less likely to be chosen to be monitored by the Special Taxpayer Auditing program if their firm ranking variable is above the cutoff than if it were below (monotonicity).[20] Then, β

19. All reported regression results are estimated while applying a triangular kernel function to the firm ranking variable ($max(0, 1 - abs(running\ var.))$), which places more weight on observations closer to the cutoff. This is in keeping with best practice in the non-parametric estimation literature and with the intuition of the regression discontinuity design, i.e., that we are most interested in the effect on firms around the cutoff. Results are similar if I estimate without these weights.

20. Note that the exclusion restriction for the fuzzy regression discontinuity design is not identical to that for the instrumental variables design. In particular, one need not assume the RD design isolates treatment

estimates the causal effect of monitoring on "compliers" of this instrument—a local average treatment effect (LATE). Compliers are firms that are chosen to be monitored because their firm ranking variable value is above their province cutoff.[21]

There are two common tests of the assumption of smooth expected outcomes for treated and control firms around the cutoffs. The first is that firms cannot perfectly manipulate their running variable value such that they can decide whether to be above or below the cutoff. For, were this the case, then we would compare the outcomes for firms that found it optimal to be treated to those that did not and are likely to derive estimates biased away from no effect. I tested this assumption directly in Section II.C. (see Figure I).

The second test is to look for jumps in predetermined variables that also affect the interest rate on bank debt. If we see any effect around the cutoffs before they were even chosen for treatment, i.e., in $t = -1$, that would indicate other variables that impact firm cost of capital were driving the effect, not treatment per se. Therefore, I test for jumps in predetermined firm characteristics that are predictors of the equilibrium interest rate on new bank debt, including the firm indicators and ratios that are used by Ecuadorian loan officers at the two largest private banks in Ecuador as a first screen of firm credit worthiness that are comparable across firms.

Figure III displays plots of the averages for these predetermined covariates as of event time $t = -1$, grouped in percentile bins of the firm ranking variable. Visual inspection suggests that there are no statistical discontinuities in the cross-sectional distributions of any of these variables around the cutoff. Regression results reported in Panel A of Table VI confirm the visual evidence. Panel B also tests for jumps in changes, from two years before treatment assignment to the year before treatment assignment, with the caveat that there is no bank data available for 2008, and so the test does not include the change in the two years before the treatment assignment year 2010 for bank debt variables. There is a marginally significant (at the 10 percent level) jump in firm size from two years to one year before treatment assignment, but otherwise no evidence of discontinuities. A joint test of the equality of all the coefficients is insignificant, with a p-value of 0.388.[22] Note in particular that there is no ex-ante significant difference across the cutoffs in the weighted average interest rate. Together with the balance histogram, this result provides support for the identification assumption.

[Place Figure III here.]

[Place Table VI here.]

V. Effect of government monitoring on cost of bank debt

From the agency cost literature, if we could isolate only the effect of the introduction of an effective third-party monitor we would unambiguously expect to find that the firm's cost of new bank debt decreases. It is less clear the *net* effect of government monitoring in particular will significantly lower the cost of bank debt. Monitored firms pay higher taxes (see Table II) and incur the costs of an intensive audit.[23] Thus, the most obvious alternative hypothesis is

variation that is as good as random. Conditional on the RD assumptions being satisfied, randomized variation is a *consequence* of the firm's inability to perfectly control the assignment variable near the cutoff (Lee [2008]; Lee and Lemieux [2010]).

21. The estimated fuzzy RD coefficients are also local in the sense that the LATE interpretation applies only to the sub-population of compliers with firm ranking variable values close to the cutoff. See Angrist and Pischke [2009], Chapter 6, for details.

22. The main results are robust to controlling for firm size (see below and in the Online Appendix).

23. See Iliev [2010] and Coates and Srinivasan [2014] for evidence that compliance costs from the Sarbanes-Oxley Act of 2002 in the U.S. context were substantial.

that there will be a negative net effect. Moreover, monitored firms may be unable to continue some informal relationships with suppliers or employees (De Paula and Scheinkman [2010]; Kleven, Kreiner, and Saez [2016]), and may operate at a competitive disadvantage versus more lightly monitored competitors (De Fontenay [2016]; Boone, Floros, and Johnson [2016]). Alternatively, there may be no effect if the SRI was not a credible monitor (though we have direct evidence that it was) or if lenders already know firms' true profitability and risk (e.g., Artavanis, Morse, and Tsoutsoura [2016] provide evidence that Greek banks do a good job taking into account borrower misreporting and adjusting their lending terms accordingly). What the net effect is in this context is an empirical question.

Figure IV displays the net effect on the weighted average interest rate on new bank debt over the three years following the year of treatment assignment for all firms who were eligible to be chosen for monitoring, regardless of whether they were chosen, i.e., the reduced-form estimate. This is the effect assuming that the cutoff rule was followed strictly, and thus that every firm to the right of the cutoff received treatment. The figure plots the relationship in percentile bins of the normalized firm ranking variable, along with the fitted values and 95% confidence intervals from the reduced form equation (4). Visual inspection reveals the discontinuity in average borrowing cost induced by monitoring by the SRI: The average cost of bank debt is significantly lower for firms whose firm ranking variable value is right above the cutoff relative to firms just below it; the drop at the cutoff is about 5%. Given the identification assumption, the discontinuity in the cost of bank debt induced by monitoring (i.e., the reduced-form estimate) is attributed to the monitoring treatment, which decreases the ex post interest rate on bank debt for treated firms.

[Place Figure IV here.]

Let us turn to the estimates of the fuzzy regression discontinuity model. As a baseline, Column (1) of Table VII reports the coefficients of a simple OLS estimation of the regression given by equation (3). The coefficient β^{OLS} equals -0.496 and is significant at the 1% level; firms whom the Special Taxpayer Auditing program monitors pay a relatively lower interest rate than firms that do not, on average. Column (2) in Table VII reports the results of the fuzzy regression discontinuity regression, which is analogous to a two-stage least squares estimate, i.e., the ratio of the reduced-form effect around the cutoff divided by the first-stage jump in the probability of treatment. The coefficient β equals -0.331 and is statistically significant at the 1% level. We see that the RD estimate is lower than the OLS estimate (the difference is significant at the 1% level). This confirms our ex ante intuition that the direction of the bias in a naive test of the effect of government monitoring on firm cost of bank capital would be positive.

[Place Table VII here.]

Column (3) shows the fuzzy regression discontinuity specification with predetermined controls that also predict equilibrium bank interest rates, including the firm ratios and indicators used by loan officers at the largest private banks in Ecuador. The magnitude of the point estimate reduces to -0.219 and continues to be highly significant. Column (4) estimates the RD regression within province-year, mean-squared-error optimal bounds (Imbens and Kalyanaraman [2012]). Column (5) reports the RD regression estimate within optimal bounds and including predetermined, firm-level controls. The difference between the coefficients in Column (2) and Column (3) is statistically significant at the 5% level while there is no significant difference between the coefficients in Column (3) and Column (4) or

between Column (4) and Column (5).[24] This indicates that an upward bias enters into the estimates as we move away from the neighborhood of the cutoff and that bias is related to the effect of the firm financial ratios and indicators.

The last, most conservative, model is my preferred specification. I estimate that monitoring by the Special Taxpayer Auditing program decreases the firm's rate on its bank debt, and hence its total interest paid, by approximately 17.9% versus other, unmonitored firms that had similar observables before treatment assignment and with running variables just to the left of the cutoff.[25] This represents an approximate 2 percentage point reduction in the average monitored firm's annualized, nominal interest rate relative to the ex ante average interest rate spread (over the average deposit rate) of 13.41% on new bank debt in the pre-assignment period. To get a sense of the magnitude of this effect, consider the average loan in the year before treatment assignment which was for \$104,010 with a one-year maturity. Then SRI's monitoring causes the firm whose firm ranking variable is equal to the cutoff value to pay approximately \$1,570 less in interest payments. This is a meaningful reduction given the average (median) firm earnings (EBITDA) of 58 (26) thousand in the year before assignment (see Table IV). Online Appendix Table A2 displays the 2SLS estimates of the effect of monitoring by the Special Taxpayer Auditing program separately in event-time years out from the treatment assignment date ($t = 0$). We see that the effect shows up in the first year after treatment assignment and is sustained, though it slightly smaller in magnitude, and significant at the 10 percent level, for new bank borrowing up to three years out from treatment assignment. Thus, the net effect of government monitoring in this setting was to lower the cost of new bank debt.[26]

V.A. Robustness analyses

The finding that government monitoring lowers the average treated firm's cost of bank debt is robust to estimation separately by each treatment cohort (see Online Appendix Table A3). We do observe that the size of the effect decreases with each cohort. I also lose power, as one-third of my treated sample are chosen in the 2010 cohort, but this suggests that, while they could not perfectly manipulate their ranking, firms around the cutoff may have anticipated that their probability of being chosen at some point had risen, potentially from observing province peers who were chosen. To the extent that this is the case, some of my control firms received a (weaker) form of the treatment, which would tend to bias estimates towards finding no effect.

Additionally, the treatment effect does not change if I control for the shape of the relationship between the firm fundamentals variable and the average interest rate via parametric estimation of different polynomial transformations f of the normalized firm ranking variable while estimating over the full support instead of within optimal bandwidths (see Online Appendix Table A4). The main results are also robust to different choices of bandwidth (Online Appendix Table A5). Moreover, the results are robust to estimation with cohort

24. Note that the comparison between Column (3) and (4) is a rough-and-ready test as they are estimated on different samples.

25. Compare this to the finding in Guedhami and Pittman [2008] in the U.S. context that an increase in the probability of an IRS audit in a firm's district decreases the at-issue yield spread on new 144A debt by 5% of the average spread, El Ghoul, Guedhami, and Pittman [2011] find that the same monitoring shock decreases equity financing costs by 11%. Datta, Iskandar-Datta, and Patel [1999] estimates that bank cross-monitoring reduces the cost of arm's-length debt by around 19%. Mansi, Maxwell, and Miller [2004] find that higher auditor quality and tenure are associated with an 18% narrower spread on new debt financing relative to the mean spread (30% for below-investment grade firms).

26. Yet, for the average monitored firm, the magnitude of the increase in tax payments was larger than the interest savings, though we shall see below that monitored firms also were able to borrower substantially more debt, suggesting that on balance the average treated firm was better off.

and province, rather than cohort times province, fixed effects (see Online Appendix Table A6). I also find that the estimated effect is unlikely to be caused by concurrent changes in other determinants of the interest rate. I control for those that I can observe directly in Table VII, where their correlation with firm average cost of bank capital is economically small. Finally, I re-estimate around placebo cutoffs to the left and right of the true cutoffs, and I do not find significant effects around any of them (see Online Appendix Figure A2).

VI. Mechanism

What is driving the effect of tax enforcement on the cost of bank capital? A lower cost of capital paired with higher levels of borrowing suggests that agency frictions between monitored borrowers and their lenders decrease. The evidence I report below suggests the main mechanism is a reduction in hidden action frictions. For example, when a firm is chosen for annual SRI auditing, the cost of diverting resources from both the government and creditors increases. In Ecuador, the speed and severity of the expected penalty also increases, since, unlike a bank, the SRI can unilaterally apply fees, suspend the firm's license to operate, etc. A firm's cost of capital could even decrease if all of its reports to its bank were entirely truthful both before and after it became a Special Taxpayer if government monitoring increases the credibility of the reports.

VI.A. A Tax shield mechanism

I have documented that firms paid higher taxes and that their average interest rate spread on new bank debt narrowed. Since interest on debt is deductible from corporate taxes in Ecuador, these results are consistent with treated firms demanding a higher tax shield (Modigliani and Miller [1958]). Do firms increase the amount that they borrow?

In Table VIII I report the effect of government monitoring on loan terms beyond the cost of new debt. Column (1) of Table VIII reports that there were significant jumps in the average size of new bank debt for newly monitored firms to the right of the cutoff. This is intuitive—the price of bank debt has declined so the demand should increase. The magnitude of the increase is large. After firms began to be monitored by the Special Taxpayer monitoring group, the average amount of total new bank debt increased by 27% relative to control firms. Interpreted versus the pre-period mean amount of new bank debt, \$216,700, the average treated firm borrowed around 60 thousand more new bank debt after being chosen for the monitored group. From Column (2) we see that this increase is also reflected in a higher number of loans borrowed, which increases by 42% versus for control firms. Interpreted relative to a pre-period average of 3.1 new loans that would be about two more loans. And from Column (3) of Table VIII we learn that the new bank debt of monitored firms was also for a longer term-to-maturity versus for control firms just to the left of the cutoff. New debt in the three years after a firm is chosen for monitoring has a loan-size weighted average maturity approximately six months longer than the pre-period weighted loan-to-maturity on new bank debt of 11.6 months.

[Place Table VIII here.]

Given these facts, we expect that part of this increase in the amount of debt borrowed is likely to be an increase in demand for a tax shield. However, a lower average interest rate, controlling for the size of the new bank loans, is not consistent with this being the main driver of the cost-of-capital results. While banks may indeed have been happy to lend more to meet increased demand by treated firms, it is not clear why they would do so at

a lower, per-unit cost, if nothing else had changed. On the contrary, if the firm must pay higher taxes, then the bank's estimate of their default risk should increase. Moreover, as we shall see, the average firm increases its investments in human and physical capital and this is driven by firms that borrowed new bank debt, suggesting that there is significant borrowing for investment purposes.

VI.B. An agency-cost mechanism

A lower cost of capital paired with higher levels of borrowing suggests that banks lowered their credit risk assessments of monitored borrowers. Three tests presented in this section support the reduction of agency cost as the main mechanism behind the reduction in the cost of new bank debt for treated firms. First, I show that treated firms access new, relatively uninformed sources of capital. Second, I show that firms that have higher ex-ante agency cost frictions have a larger decrease in their interest rate on new bank debt than the average treated firm. Third, I find that the decrease in borrowing cost is strongest for firms that were more likely to be evading taxes and manipulating their financial statements ex ante, suggesting that banks know which firms will be most constrained by government monitoring.

VI.C. The monitored access uninformed capital

If SRI monitoring reduces agency frictions between firms and creditors then treated firms should gain access to capital from new investors that were at an informational disadvantage relative to existing investors before monitoring. The majority of firms in my sample borrow from only one bank and have few, if any, private investors, but there is enough variation that we can test for an extensive margin. Table IX provides evidence for this prediction. From Column (1), we see that monitored firms are significantly, 73%, more likely to start borrowing from a bank they have never borrowed from before, albeit from a low pre-period base rate of 4.36%. Column (2) demonstrates that monitored firms also attracted new private investors, defined as a firm or individual that bought a minority stake in the firm and has not invested in the firm beforehand. Specifically, the number of new private investors grew by 34%, versus a 2% growth in new investors from two to one year before treatment assignment.

[Place Table IX here.]

VI.D. Firms with higher ex-ante agency frictions are most impacted by monitoring

Next, I use the heterogeneity in my treated sample to test the hypothesis that firms with more severe proxies for the magnitude of agency cost are more affected than the average treated firm. For example, small, younger, and more informationally opaque treated firms should be more affected than the average treated firm. Firm size and firm age in particular are variables that are closely related to information opacity across time periods and countries (Hadlock and Pierce [2010]), and they are widely used as proxies for the severity of agency frictions in the literature. Table X reports the 2SLS estimates of local linear regressions of Equation (3) where treatment is interacted with predetermined proxies for information opacity.[27] In the first three rows of Table X, firm size is proxied by ln(total assets), ln(sales), and ln(employment), where employment is the number of employees reported by the firm

27. Continuous characteristics are standardized so that the main effect has the intuitive interpretation of the effect of treatment at the average value of the characteristic in the sample and the interaction term represents the effect of increasing the characteristic for the treated by one standard deviation point. Note that to retain enough power to perform subsample analyses all specifications in Table X are over the entire support of the running variable, include the square of the running variable as an additional control, i.e., are estimated as a polynomial RDD specification, and include cohort times province fixed effects.

to the Ecuadorian social security administration (the Banco del Instituto Ecuatoriano de Seguridad Social). All three measures, and firm size measured using sales with significance at the 1% level, suggest that the difference in the borrowing rate between monitored and unmonitored firms narrows as firm size increases, i.e., smaller firms are more affected by the monitoring treatment relative to larger firms.[28] In the fourth row we find that the effect of the monitoring treatment is diminishing with firm age. This is consistent with firms that have larger agency frictions ex ante, because they have less information available overall and because they are under less scrutiny from other sources, having a larger decrease in agency frictions due to being chosen for annual auditing by the SRI.

[Place Table X here.]

I next examine other proxies for how difficult it is for a firm to unilaterally send a credible report to their lender. Row five of Table X displays the interaction of the treatment dummy with an indicator variable that takes the value of one if the firm is headquartered in either Quito or Guayaquil, the two major cities in Ecuador and the centers of financial services. We see that the effect is larger for firms that are not headquartered in one of these two cities. The cost of monitoring is likely to be higher for firms that operate in more remote areas of the country. Finally, I ask whether the relationship varies based on bank heterogeneity. Row six of Table X reports the interaction of treatment with *Bank size*, the within-year rank of all banks by total outstanding assets, where each observation represents the range of the largest bank that the firm borrowed from in that year. The largest bank overall has rank 1 and the smallest of 24, so that bank size is decreasing in the measure. The estimates suggest that firms that borrow from larger banks receive a larger reduction in their borrowing cost after entering the Special Taxpayer monitoring program than the average treated firm. This is consistent with the intuition that larger banks specialize more in rules-based lending based on hard data, and less on relationship lending, than smaller banks (Berger, Klapper, and Udell [2001]; Berger and Udell [2002]). The combined evidence is consistent with a larger effect of monitoring on firms tending to have more severe agency frictions ex ante.

VI.E. Firms likely to divert taxes or commit fraud before treatment are more affected by treatment

The evidence thus far is most consistent with the main channel of the effect coming from a reduction in agency friction costs between borrowing firms and their banks. Recall that by "agency friction," I mean both hidden type and hidden action frictions. Theoretically, we would expect both frictions to decrease from credible government monitoring. Indeed, the evidence so far is consistent with a reduction in both. I use an additional source of data to provide evidence that the more important driver in this setting was a reduction in hidden action frictions.

Note that a maintained assumption of these tests is that firm propensity to evade taxes is correlated with firm propensity to act strategically against all stakeholders. This is indicative of hidden action frictions because both the tax regulator and the creditor are concerned that the firm will take action to hide or remove firm value at the time when tax or interest payment is due. But both types of evasion are inherently tricky to measure. As a first pass, row seven of Table X considers a widely used measure of the ease of evasion that is visible to both the SRI and the banks: The amount of physical inputs that the firm uses to operate,

28. The effect of treatment for the firms with the interacted characteristic (e.g., larger firms) is estimated as the sum of the coefficient on the main effect *Monitoring* and on the coefficient on the interaction effect *Monitoring* $\times$ *Characteristic*.

e.g., factories, machines, real estate, etc. Intuitively, a firm that uses more physical inputs will have a harder time hiding assets as they are easier for outsiders to observe.[29] We see that if the firm has a ratio of tangible assets to employees that is greater than its industry's median value, then the firm is less affected by treatment.

Next, I use two measures of tax evasion that are internal to the SRI. The first is a score that ranges from 0 to 3.65, where a lower score indicates a lower probability that the firm will not pay taxes.[30] When interacting this internal tax evasion measure with treatment status, in row eight of Table X, I find that government monitoring has a larger impact on firms whose actions are most likely to change in response to being monitored. The second measure, used in row nine of Table X, is a proxy for the seriousness of the firm's tax evasion. Specifically, it is an indicator that equals one if the SRI is highly confident that the firm is under-reporting revenue egregiously because the firm reported more than \$10,000 less than the SRI's internal estimated lower bound on true revenue.[31] Again we see that firms that are more likely to be evading taxes in the pre-period have a larger decrease in their average weighted interest rates on new bank debt after being chosen for the monitored group than the average treated firm.

Since banks cannot observe the SRI's tax evasion proxies, these results suggest that the SRI's and the banks' beliefs about which firms are more likely to engage in diversionary behavior are correlated, and that the bank takes into account that these firms will be constrained in these actions by being in the monitored group when setting the cost of new bank debt for those firms. To proxy for information on evasion the bank can directly, I use the observation that the first non-zero digit of firm financial, income, and tax statements that are not labels (e.g., an ID) or summary measures should follow a theoretical distribution posited by Benford's law. The use of Benford's Law to screen for tax fraud rests on the observation that the number manipulator's intuition is that the leading digits of a randomly chosen number follows a uniform distribution, i.e., that every first digit from one to nine, is equally likely to occur. However, Benford's Law states that, for random numbers that follow a smooth and symmetric distribution, the frequency of leading digits follows a base-10 logarithmic distribution wherein the frequency of each digit decreases from one to nine. That the first digits of financial statement line items should follow Benford's law has been confirmed in a U.S. context (Nigrini [1996]; Amiram, Bozanic, and Rouen [2015]). In Online Appendix A2 I proved more information about Benford's Law and report evidence that the financial statement numbers in my dataset conform to Benford's Law in the aggregate.

I use two firm-year level measures that proxy for departure of firm information from the theoretical distribution. The first is a continuous measure, the mean absolute deviation (MAD) statistic, which is calculated as:

$$MAD = \frac{\sum_{i=1}^{9} \mid OP - EP \mid}{K} \tag{5}$$

where i indexes the nine possible digits, OP is the observed proportion of each digit while EP is the expected proportion for each digit based on Benford's Law. A low value of MAD indicates that the observed digit proportions closely approximate the actual values.

29. For instance, it is typically simpler to estimate the true size and profitability of a manufacturing firm versus an equivalently-sized firm in a service industry. Note that this result is consistent with those of the prior section that finds smaller effects for firms with lower agency frictions relative to the average treated firm.

30. Specifically, a score within [0,1.45) indicates low risk, or an estimated 100% receipt of taxes owed on time, a score in [1.45,2) indicates a medium low risk, in [2,2.44) medium high risk, and in [2.44,3.65] high risk, or close to 0% expected probability the firm will file.

31. This lower bound is calculated using third-party data (suppliers, credit cards, etc.). See Carrillo, Pomeranz, and Singhal [2017] for more details.

We can see from row 10 of Table X that firms whose first-digit distribution depart further from Benford's Law have a lower interest rate on new bank debt.

The MAD measure has the advantage of being scale independent, i.e., it does not depend on the number of line items used. This makes it robust in that it does not require the econometrician to choose which line items to include. However, there are no objective, statistically valid cutoff scores for the MAD to test conformity to Benford's Law against. Thus I also calculate a chi-squared statistic for the first digits of each set of firm-year statements:

$$\chi^2 = \sum_{i=1}^{9} \frac{(OP - EP)^2}{EC} \tag{6}$$

Like the MAD, the higher the χ^2 statistic, the more the data deviate from Benford's Law. Unlike the MAD, there are well-established critical values against which to test, so that I can estimate a yes-or-no answer to the question of whether the observed distribution is statistically different from the expected by running a χ^2 test. However, the χ^2 statistic is not scale invariant. As the dataset (number of first digits) becomes large, the calculated chi-square will lead us to over-reject the null. In my sample, the χ^2 test rejects the null of no difference from the Benford's Law distribution for approximately one-third of the firm-year observations (34%) at the 5% level of significance or better. This reduces to approximately 16% for firms within the MSE optimal bandwidths.[32] I construct an indicator that is one when the χ^2 test rejects the null of no difference from Benford's Law in that firm year. Results are reported for row 11 of Table X. Consistent with previous tests I find that firms whose first-digit distribution are statistically different from Benford's Law at the 5% level or better have a lower interest rate on new bank debt. Note that I am not claiming that all of the firms who have a statistically different first-digit distribution than Benford's Law committed fraud. Differences could be due to measurement error, for example. But the results of these tests are consistent with those using the SRI's measures for tax fraud.

In summary, the results of Table X, taken together, confirm that firms that are observationally more opaque, and which were more likely to be manipulating their publicly-reported and tax statement numbers before treatment, are also those firms whose cost of borrowing from banks is most reduced by beginning to be intensively and publicly monitored by the SRI.

An open question worthy of brief note is why firms do not appear to have access to a private information intermediary that can provide the same level of monitoring as the SRI. I have no data to answer this question. But I will speculate briefly on a few common scenarios consistent with this setting would since it is important when considering the broader implications of this paper. Information asymmetry may give the bank pricing power, which is a hypothesized cost of relationship lending (Rajan [1992]). Thus, any third-party would have to be both credible enough and visible enough to allow the firm access to other sources of external financing, which would place pressure on the original bank to lower its price on new debt. While high-quality auditors exist in Ecuador (all the "big 4" auditing firms have offices there), the cost of such an audit may outweigh the benefits, especially for the smaller, younger, more opaque and financially constrained firms whose borrowing costs I found were most affected by beginning to be monitored by the government. Moreover, each individual firm is ex ante trading off the loss of the ability to evade taxes and other external scrutiny with the benefits of government monitoring. Treatment clearly changes this tradeoff. Finally, while most firms in my sample are family firms with an owner-manager, the average firm

32. Compare this to 14% in Amiram, Bauer, and Frank [2018] for U.S. listed firms and the estimate of Dyck, Morse, and Zingales [2013] that the probability of a firm committing fraud is 14.5% per year.

has minority owners as well.[33] Thus, the private benefits of any diversion may be higher for the managers relative to its benefits for the firm as a whole, preventing them from taking the optimal action for the firm before being chosen for monitoring.

Finally, the SRI has some important advantages over monitors. It can quickly and unilaterally apply penalties when it catches firm evasion. It can observe information, such as how important each firm is to its whole supply network, that may not be visible in its entirety to private actors. And, maybe most importantly, the SRI is independently motivated to catch evasion because of its objective to increase tax revenue, i.e., it has "skin in the game." Thus, there is less of an issue about who monitors the monitors, which seems to be where private external auditors fall short in the Ecuadorian context. Clearly, then, these results are most relevant to economies with predominantly private, closely-held, relatively small firms in environments with high agency frictions and with weak alternative governance institutions.

VII. Real effects of government monitoring

Does monitoring from tax enforcement affect firm investment in human and physical capital? In a country where the majority of firms report themselves as financially constrained, investment should respond to an increase in the supply of bank debt (Almeida and Campello [2007]; Banerjee and Duflo [2014]). But even in the case where the firm is not constrained, a key input has become cheaper and thus the firm may invest in projects with a lower hurdle rate than formerly. Moreover, firms that do not need to borrow in the post-assignment period may still adjust their labor and capital inputs if they believe that being monitored has increased their ability to borrow in the future, decreasing the precautionary savings motive (Aghion, Farhi, and Kharroubi [2012]).

Table XI reports tests for effects of government monitoring on firm employment and physical investment. The coefficients measure the growth in firm investment in human and physical capital from the year before treatment assignment to the average over three years after treatment assignment for monitored firms to the right of the cutoff and inside optimal bandwidths versus for the unmonitored to the left of the cutoff and within the optimal bandwidths.[34] Column (1) of Table XI reports that the effect on firm new employment growth of being chosen for the SRI monitoring program is 0.35, significant at the 5% level. relative to the growth in the number of new employees versus the average control firm to the left of the cutoff. Column (2) of Table XI adds the same predetermined, firm-level controls as were used in the main tests reported in Table VII. The effect remains positive and significant, at the 10% level, but the size of the effect reduces to a 0.161.[35]

[Place Table XI here.]

33. The average (median) Ecuadorian firm had 7 (2) shareholders (predominately private investors) over the period 2012 to 2016. Though the average (median) manager in Ecuador over 2012 to 2016 held 51% (55%) of their company.

34. As for the analyses on borrowing costs, these results are net effects, as we also might expect that the costs of government monitoring to the firm would reduce both employment and investment on the margin.

35. Compare this to Siemer [2019] who finds in the U.S. contact that financing constraints reduce employment growth in small firms by 5 to 10 percentage points relative to large firms. Benmelech, Frydman, and Papanikolaou [2019] find that a 5 percentage point larger employment cut by firms with maturing debt vis-'a-vis without during the Great Depression. If we consider the reduced form, which is 0.038, as the aggregate employment effect, we can compare this to -0.015 in Hochfellner, Montes, Schmalz et al. [2015], comparing German firms based in regions with Landenbanken that sustained trading losses from exposure to US banks during the financial crisis of 2008. We can see that effects of a positive credit shock in Ecuador are larger than those estimated in developed country contexts and in response to negative credit shocks. This is expected given the relatively larger frictions and smaller firms in Ecuador as well as stickiness in employment.

In addition to increasing credit supply for the average firm, government monitoring makes it more difficult to hide informal employees. So does this increase represent new jobs or are firms disclosing previously informal employees?[36] I use matched employee-employer data from individual tax returns reported by the firm to the SRI to help distinguish between the two stories. I classify a new employee as "newly appearing" if they do not appear to be an employee of any prior firm before since 2007, when the data begin. From Column(3) of Table XI we find that a substantial proportion of the employment effect appears to be either new entrants to the labor market or, very likely, formerly informal employees. The implication is that the connection between a positive credit supply shock and employment, and this monitoring mechanism in particular, is likely heavily dependent on the formality of the economy. Though the extant empirical evidence looking at the effect of positive credit supply shocks on employment is small, this is consistent with the existing, mixed, evidence: Morais, Peydró, Roldán-Peña et al. [2019] find that an increase in credit supply following a loosening of monetary policy abroad raised employment in small Mexican firms, but not meaningfully for large ones, while Acharya, Eisert, Eufinger et al. [2018] find that the ECB's bank bailout programs during the European debt crisis did not significantly affect employment.

The estimated coefficient of the effect size in the growth of firm employment is large while representing a relatively small number of extra employees. This highlights the small size of the average firm in my sample (the average (median) firm employed 24 (9) in the year before treatment assignment). Figure V graphs the effect of government monitoring on new employment by firm size quintile, where firm size is measures by the number of employees. We can see there is a lot of heterogeneity in the effect across firm size, with medium and large firms still increasing new employment growth on average but with a much lower magnitude. For example, a firm in the top quintile (5) by firm size, which has an average (median) of 160 (52) employees, increases new employment growth by 1.3% more than control firms in the same quintile (significant at the 10% level) in the three years following induction into the SRI's Special Taxpayer monitoring program.

[Place Figure V here.]

Panel B of Table XI reports the effect of government monitoring on firm investment. Column (1) reports that monitored firms increase their investments in physical capital, as proxied by the log change of property, plant, and equipment scaled by total assets. Specifically, physical investment grew approximately 10% more in the three years after treatment assignment over the average for control firms to the left of the cutoff. The effect size and significance do no significantly change when the model includes firm controls, reported in Column (2).[37] Once again we may wonder if this is true new investment or whether firms

36. Both would have real effects. However, the policy implication differs between the two. In the case where employees hired away from other firms these are actual new positions. Presumably their former employers will hire others to take their place or the new one replaces one cut by the former employer. In the case where the employee does not appear as employed at any previous firm either they were hired out of the portion of the population that was previously unemployed or they were formalized, i.e., they had previously worked for the firm but were not reported as employees. The latter would still be a real effect, for example the employees would be able to obtain social security benefits, access formalized financing, pay taxes, etc., but the implications and external validity are distinct.

37. Compare this to Cingano, Manaresi, and Sette [2016], who find an elasticity of 0.18 using exposure of a firm's lenders to the inter-bank market during the 2008 financial crisis; De Jonghe, Dewachter, Mulier et al. [2020] find an elasticity of 0.085 using the same inter-bank lending shock but in Belgium. Lemmon and Roberts [2010] find that below-investment grade firms reduce their net investment by 5% of assets more than their unrated counterparts in the USA and Campello, Graham, and Harvey [2010] find using survey data that the average constrained firm planned to cut back on investment by around 9% during the 2008 crisis, and this estimated cut was stable across firm respondents from US, European, and Asian firms.

are reporting increased investment because the cost of hiding it has risen or because they are seeking to increase the expenses they can deduct from taxes. If this were the case, especially the latter story, we would expect that reported intangible assets would also increase. However, from the second column of Panel B in Table XI we can see that this is not the case for the average firm. Moreover, the fact that treated firms increase their investment and employment at the same time suggests that the effects are not a mechanical result of an increased regulatory burden.

I have found that there are meaningful real effects at the firm level. Does this matter in the aggregate? Given the size of the effects, and the importance of the firms, it is reasonable to hypothesize that it does. However, it is difficult to estimate this using local effects estimated over a sub-population with ranking variables around the SRI's cutoff. It is likely that the short term real affects, particularly employment, would be modest, especially if new employees are mostly from formalization. Moreover, we might expect an attenuation of the effect in the hypothetical world were the SRI audited every firm every year. Spillover effects are likely important to the general equilibrium impact of government monitoring but are ex ante ambiguous: Negative effects on directly competing firms could lead to lower employment. Conversely, there could be positive effects on suppliers, which could lead to higher overall employment (Alfaro, Garcia-Santana, and Moral-Benito [2019]). Moreover, in the long-run there could be substantial benefits, both from new tax revenue and as monitored firms' investments pay off, but also by indirect and hard-to-measure channels like stronger rule-of-law norms and further incentives for firms connected to monitored firms to formalize due to third-party reporting. Certainly the government monitoring mechanism is worth further empirical investigation, including in other contexts outside of Ecuador that provide similar experiments due to size-dependent tax auditing thresholds.

VIII. Conclusion

This paper provides evidence that an unexpected side effect of government monitoring is an economically meaningful increase in monitored firms' access to bank credit. Specifically, Ecuadorian banks decreased the interest charged on new loans, and increased the total amount of credit, to those "special taxpayer" firms that the Ecuadorian SRI audited annually. Moreover, monitored firms became more likely to borrow from a bank they had never borrowed from before, so the risk pool was spread around a greater number of financial institutions. They were also more likely to attract investments from new private investors. Finally, despite paying higher taxes, the average monitored company stepped up investment in human and physical capital. The data supports a hidden-action mechanism. The corporate governance effects of tax enforcement are valuable to firm investors, which update their beliefs on firms' abilities to divert firm resources going forward, making firm actions more predictable under the monitoring regime.

To be clear, tax audits are not cheap. To monitor 5% of Ecuadorian firms, the tax authority uses around 70% of its auditing resources each year. Still, these companies also provide more than 80% of corporate tax revenues. Corporate costs also rose. It remains to be seen if the results of this natural experiment scale. However, in my research setting, the benefits appear to outweigh the costs with Ecuadorian regulators reporting companies requesting to be included in the auditing program.

To apply what we have learned from this study more generally, we must consider the external validity of these results. The estimates are for firms within a narrow bandwidth around the choice thresholds and for compliers, i.e., a local average treatment effect. How-

ever, over 90 other countries use specialized auditing groups similar to Ecuador's Special Taxpayer monitoring program to monitor the most important taxpayers (Bachas, Jaef, and Jensen [2018]) and for these the local results are directly relevant. This is especially so since the tax effects I document are in line with estimates from other economies using a ranking rule and cutoff to assign extra auditing resources, and so we would expect that there would also exist the indirect effects on access and cost of financing I document here. Additionally, these other settings could provide ample opportunities to consider the effects of differential government monitoring on firm investment in other settings, potentially using the same identification strategy.

The policy implication for Ecuador and similar developing nations is that tax oversight can improve credit access and growth agendas. In particular, monitoring might increase transparency for private firms with little transactional history, possibly making loans more available for young corporate borrowers. The benefits may be substantial in that credible monitoring is arguably a public good that could otherwise be underproduced. Moreover, policy that targets agency costs directly may increase access to credit with fewer distortions than alternative government options, such as directing credit to favored groups. Finally, a monitoring intervention is fiscally positive since it reduces firm diversion for both outside investors and the government.

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IX. Tables and Figures

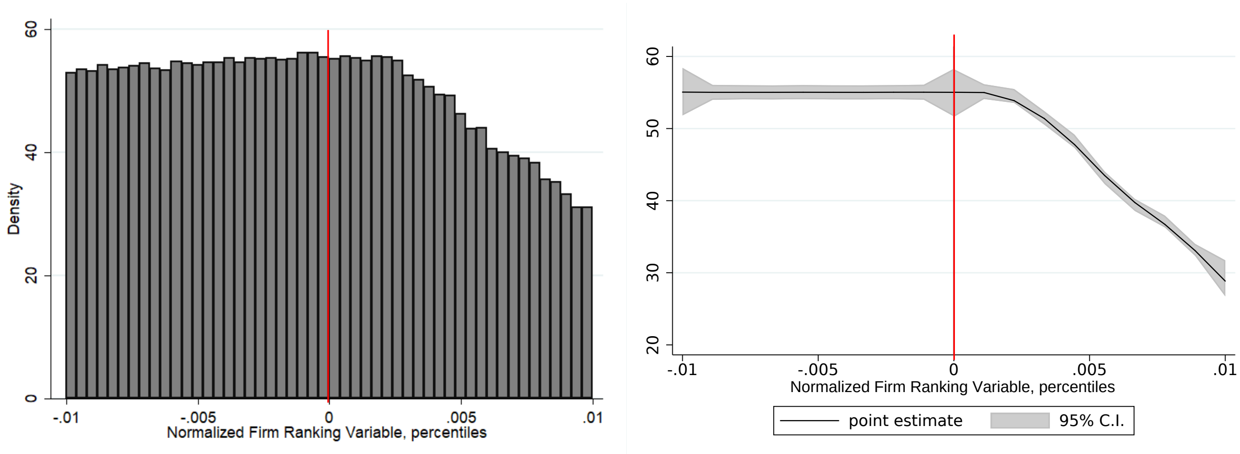


Figure I
Tests for evidence of firm manipulation of the ranking variable.

The left figure shows the histogram for the pooled sample of the normalized firm ranking variable around the cutoff at zero (the vertical line). The right figure displays the local polynomial density estimates (the solid line), and associated, point-wise, robust confidence intervals (shaded area) on either side of the pooled cutoff (the vertical line). It is generated using the algorithm and methodology of McCrary [2008], as well as the code provided by the author. The two-sided p-value of the test of a discontinuity in the density around the cutoff is 0.899. We therefore cannot reject the null hypothesis that firms do not manipulate their within-province ranking around either side of the province cutoff. See Online Appendix Figure A1 for the same figure and test produced for each assignment cohort.

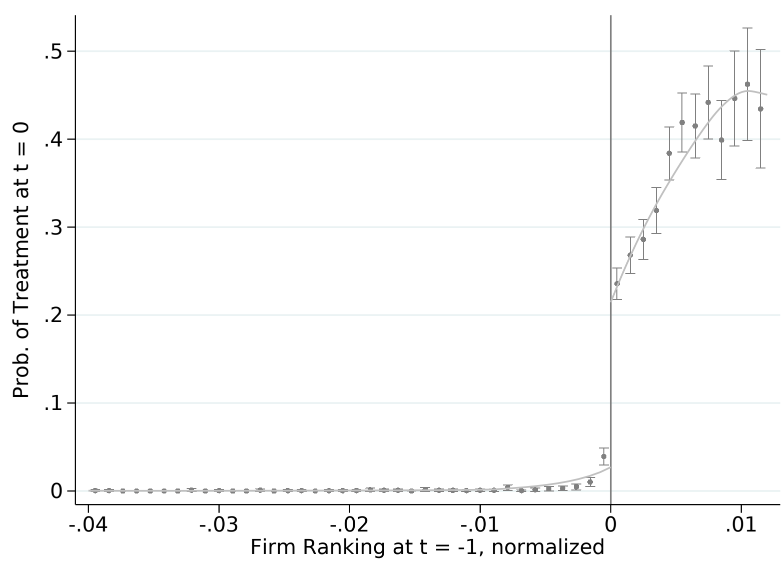
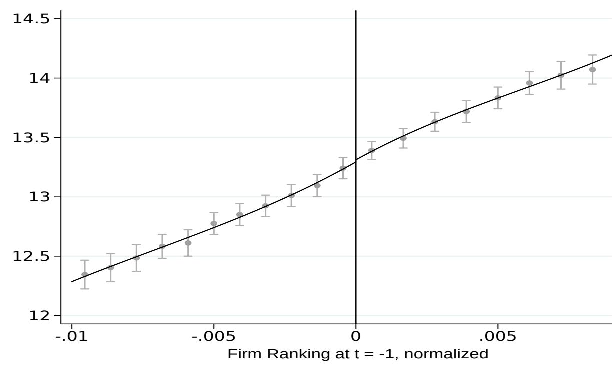
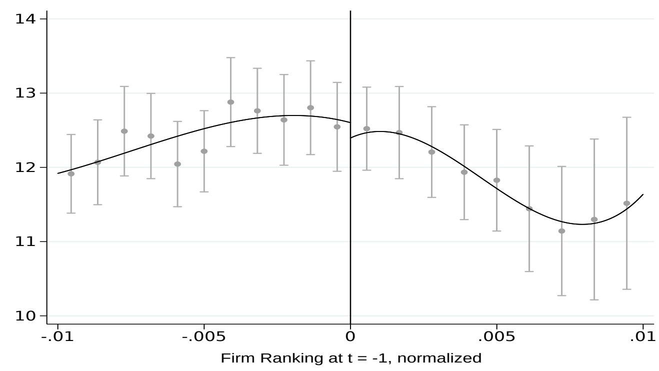


Figure II
Probability of selection into SRI monitoring as a function of the firm ranking.

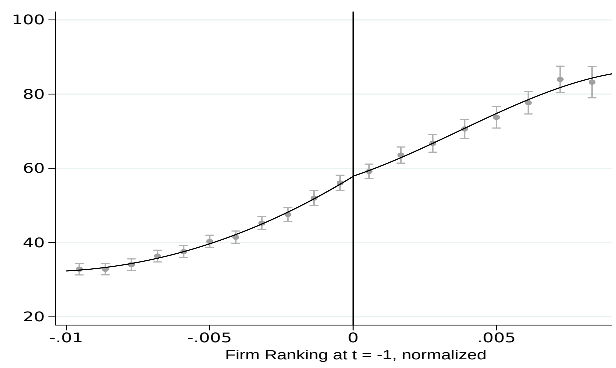
The figure shows the estimated discontinuity in probability of being chosen for monitoring by the SRI's Special Taxpayer monitoring program in percentile bins of the normalized firm ranking variable, and the fitted values and 95% confidence intervals from the regression model: $monitoring_{j,p,c,t=0} = \omega + \pi above_{j,p,c,t=-1} + f(RV_{j,p,c,t=-1} - \text{cutoff}_{p,c,t=-1}) + \gamma_{c,p} + A'X + \zeta_{j,p,c,t=0}$, where *monitoring* is an indicator that equals one if the firm is chosen for monitoring in the event year ($t = 0$) and zero otherwise, *above* is an indicator that equals one if j's normalized firm ranking variable value is greater than the cutoff at zero (represented by the black vertical line) and zero otherwise, *f(RV - cutoff)* is a 3rd degree polynomial of the normalized firm ranking variable, X contains predetermined, firm-level controls, and $\gamma_{c,p}$ are cohort times province fixed effects. Standard errors are clustered at the industry level (243 industries).



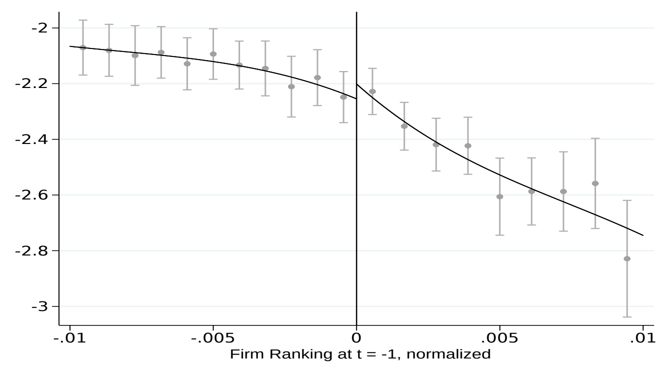
(a) Firm size (ln(total assets))



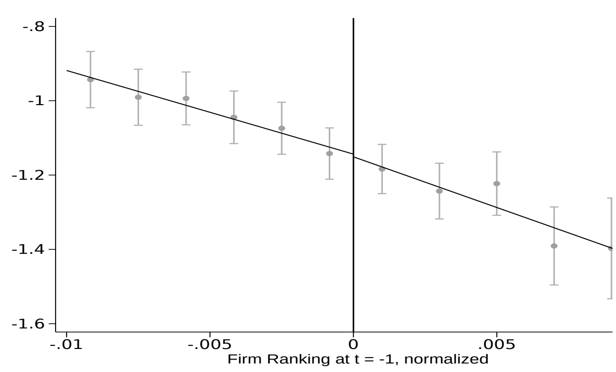
(b) Firm age (years)



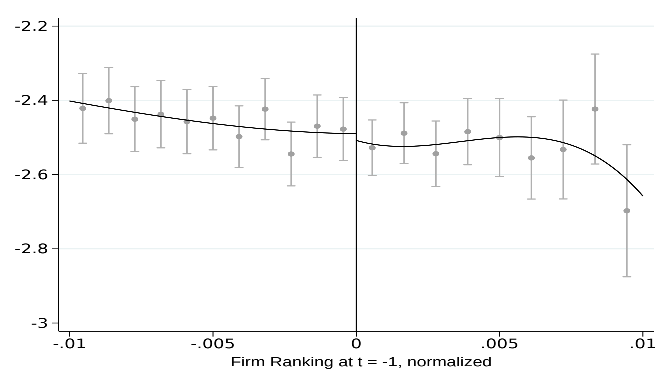
(c) Sales volatility



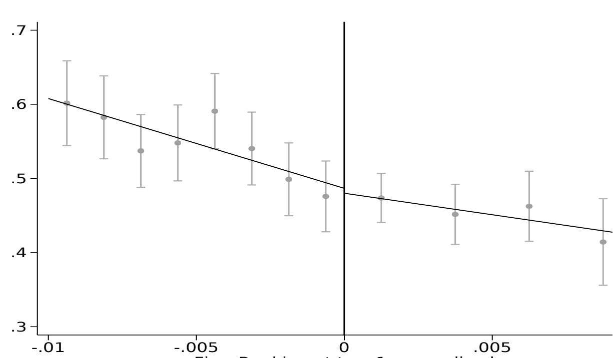
(d) ln(Tangible asset ratio)



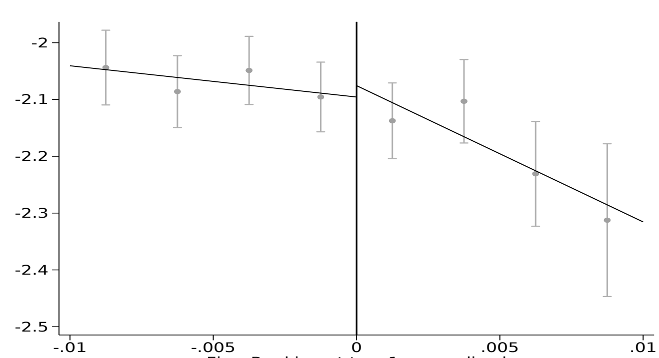
(e) ln(Interest expense ratio)



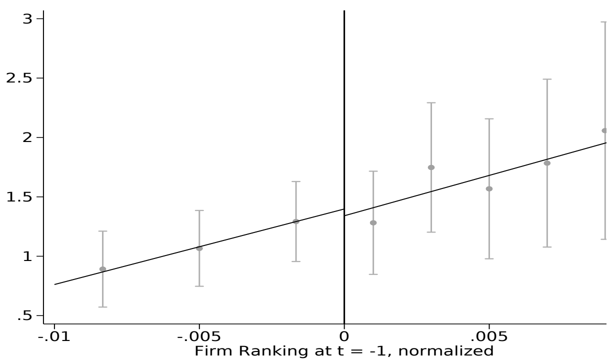
(f) ln(ROA)



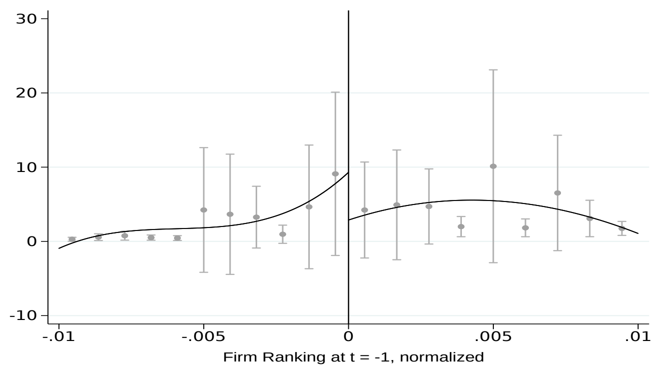
(g) ln(Current ratio)



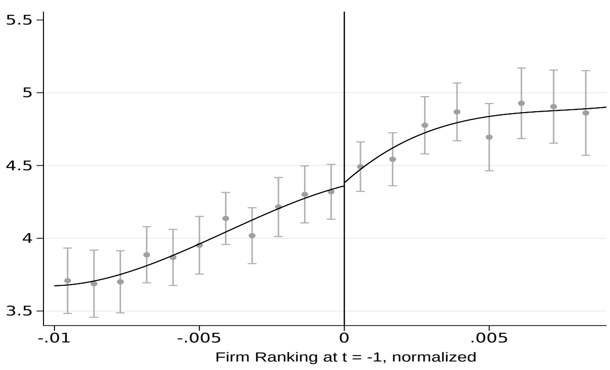
(h) ln(Debt-to-asset ratio)



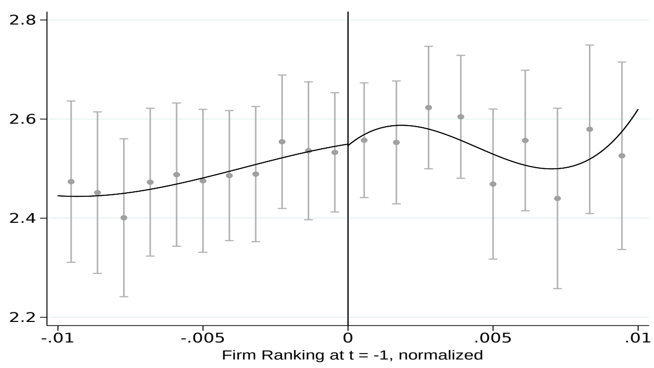
(i) ln(Val. bank debt write-downs)



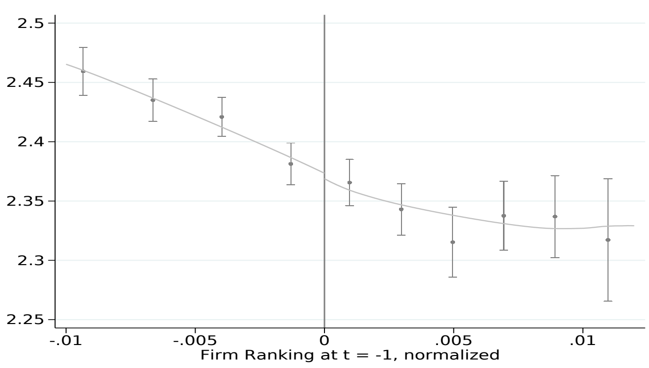
(j) Avg. days late loan payments



(k) ln(Val. outstanding bank debt)



(l) ln(Avg. term-to-maturity bank debt)



(m) ln(Avg. interest rate bank debt)

Figure III

Tests for ex ante discontinuities in determinants of the cost of new bank debt.

The figures test for discontinuities in the pre-determined outcomes ($t = -1$) in bins of the firm ranking variable. They also plot the fitted values and 95% confidence intervals from the reduced-form regression model $Y_{j,c,p,t=-1} = \alpha_0 + \beta \times \pi \times above_{j,c,p,t=-1} + \hat{f}(RV_{j,c,p,t=-1} - cutoff_{c,p,t=-1}) + \gamma_{c,p} + A'X + \chi_{j,c,p,t=-1}$. *Above* is an indicator variable that equals one if the firm's normalized firm ranking variable value is greater than the pooled cutoff at zero (represented by the vertical line), *f(RV - cutoff)* is a 3rd degree polynomial of the normalized firm ranking variable, X contains predetermined, firm-level controls, and $\gamma_{c,p}$ are cohort times province fixed effects. See Table IV for variable definitions. Standard errors are clustered at the industry level (243 industries).

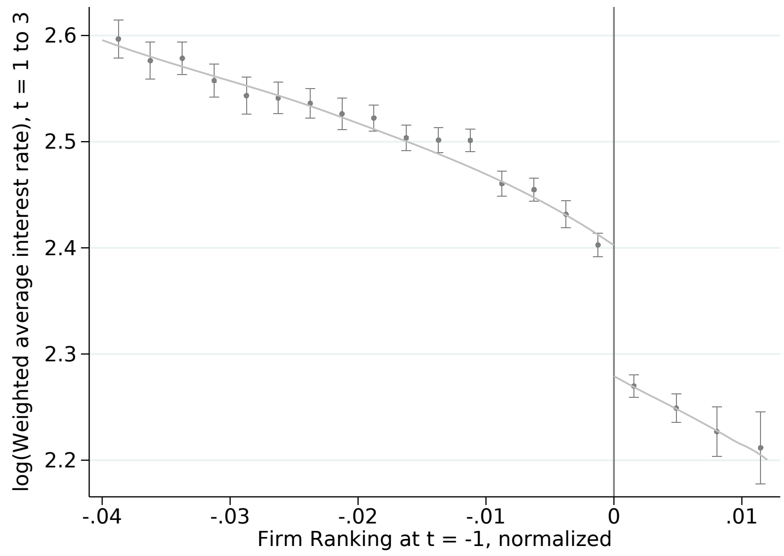


FIGURE IV
The reduced-form effect of monitoring by the SRI on firm cost of new bank debt.

The figure shows the effect on the weighted average interest rate spread on new bank debt over the three years following the event year ($t = 0$) for all firms eligible to be chosen for auditing by the Special Taxpayer monitoring program, regardless of whether they were chosen, i.e., the reduced-form estimates, in percentile bins of the normalized firm ranking variable, and the fitted values and 95% confidence intervals from the regression model $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} = \alpha_0 + \beta \times \pi \times above_{j,c,p,t=-1} + (RV_{j,c,p,t=-1} - cutoff_{c,p,t=-1}) + \gamma_{c,p} + B'X + \chi_{j,c,p,t}$. Y is the natural log of the weighted average interest rate spread, *above* is an indicator variable that equals one if the firm's ranking variable, *RV - cutoff*, is greater than the cutoff (the black vertical line). X is a matrix of firm-year level controls, and $\gamma_{c,p}$ are cohort times province fixed effects. Standard errors are clustered at the industry level (243 industries).

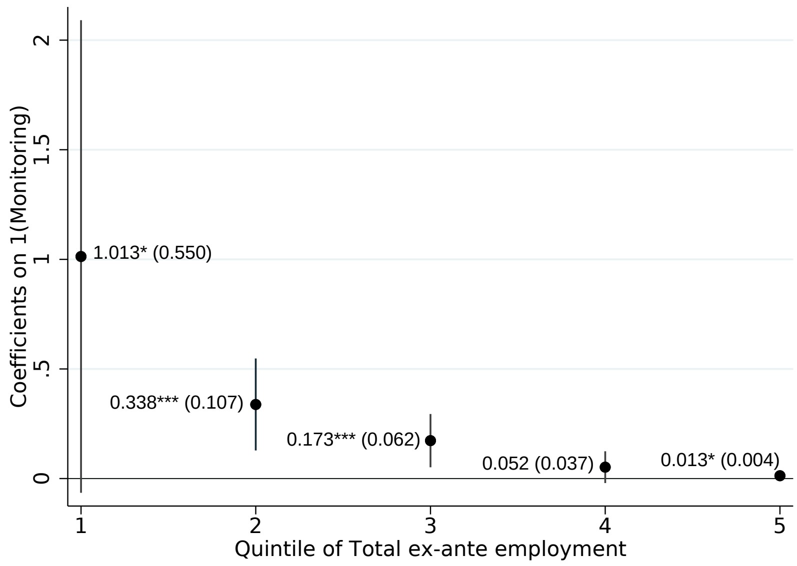


FIGURE V
Effect of monitoring by the SRI on firm employment, by firm size quintiles.

Coefficients are from the model: $\frac{\text{New employees}}{\text{Total employees}_{-1}}_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} - \frac{\text{New employees}}{\text{Total employees}_{-1}}_{j,c,p,t=-1} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_{c,p} + \epsilon_{j,c,p,t}$ of the log change in new employment on *Monitoring*, an indicator variable that equals one if the firm is chosen in event year $t = 0$ for the Special Taxpayer monitoring program, which is instrumented by the discontinuity in the probability of treatment around the cutoff. Estimations include cohort $\times$ province fixed effects and triangular kernel weights. The entire support of the data is used, controlling for the underlying relationship between the change in employment and the running variable and the square of the running variable (polynomial specification). 95% confidence intervals are shown and each point is labeled with the corresponding coefficient and standard error (in parenthesis). Standard errors are clustered at the industry level (243 industries). *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

TABLE I

Firms included in the Ecuadorian Special Taxpayer monitoring program as a share of all firms, of total auditing resources, and of tax revenue

Year	Num. monitored by SRI	Num. firms	Share monitored firms (%)	Share auditing resources (%)	Share tax revenue (%)
2010	7,753	124,619	6.22	70.4	84.30
2011	7,572	140,562	5.39	74.3	90.99
2012	7,420	155,525	4.77	85.2	90.39
2013	7,232	166,891	4.33	87.7	90.31
2014	7,018	178,108	3.94	89.3	88.58
2015	7,197	222,824	3.23	83.3	85.44

Notes. Included firms are those that reported positive sales to the Ecuadorian tax authority and that submitted a tax filing. Monitored firms are those that are annually audited by the Special Taxpayer auditing unit and that were chosen for that always-audited group over the period 2010 to 2015. Source: Servicio de Rentas Internas del Ecuador.

TABLE II

Effect of government monitoring on firm tax payment

$ln(Outcome_{Avg.\ t=1\ to\ t=3}) - ln(Outcome_{-1})$

Outcome	Taxes paid
Monitoring_0	0.583***
	(0.177)
Constant	-0.023
	(0.031)
Observations	57,661
Cohort × province FE	Yes
Triangular kernel weights	Yes

Notes. Estimates are from local linear regressions estimated on either side of the normalized cutoff of zero within optimal bandwidths, in a minimized mean-squared error sense, and with triangular kernel weights that put more weight on observations close to the cutoff. The regression is $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} - Y_{j,c,p,t=-1} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_{c,p} + \epsilon_{j,c,p,t}$. The outcome is the log difference in the average corporate tax payment in the three years after, versus the year before, treatment assignment. *Monitoring* is a treatment indicator variable that equals one if the firm is chosen in event year $t = 0$ (2010 to 2015) to be included in the Special Taxpayer monitoring program. Treatment is instrumented by the discontinuity in the probability of treatment around the cutoff. Estimations include cohort × province fixed effects and triangular kernel weights that assign more weight to observations close to the cutoffs. Standard errors are clustered at the industry level (243 industries). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

TABLE III
Tests for differential attrition by treatment status

Panel A: Tabulation of entry and exit of firms into the Special Taxpayer monitoring program

Year	Num. monitored	Num. new to monitoring	Num. removed from monitoring	Removed & remain in regression sample
2010	7,753	1,233	1	1
2011	7,572	380	8	8
2012	7,420	741	403	372
2013	7,232	609	410	378
2014	7,018	467	417	379
2015	7,197	581	204	186

Panel B: Proportion remaining in the sample by three years after the treatment-assignment year ($t = 0$)

		Bandwidth around normalized cutoff (c = 0)		
	Full sample	0.01	0.02	0.05
Treated	0.778	0.796	0.780	0.779
Untreated	0.666	0.801	0.772	0.769
Difference	0.112	-0.005	0.008	0.009
Observations	988,529	35,334	55,203	114,616
Pr(Diff. = 0)	0.000	0.287	0.250	0.168

Notes. Column 2 of Panel A reports the number of all firms in the Special Taxpayer monitoring program over the period 2010—2015. Column 3 of Panel A reports the number of newly treated firms that were chosen for the program that year. Column 4 of Panel A reports the number of firms that the SRI removed from the monitoring program that year and Column 5 of Panel A the number that were removed by the SRI from the program and yet remain in my sample. Panel B reports the proportion remaining in the sample by three years after the treatment-assignment year ($t = 0$). Note that the sample was created by pooling across stacked treatment event cohorts, each with its own panel from the year before treatment assignment to three years afterwards. Thus, a firm can be counted in the control group more than once, e.g., if it is a potential firm within the specified bandwidth that is not chosen multiple years in a row. Results are similar if we look at each cohort separately (unreported).

TABLE IV
Summary statistics of pre-determined variables ($t = -1$)

Panel A: Firm characteristics in $t = -1$

Variable	Units	Obs.	Mean	Median	Std. Dev.	Min.	Max.
Firm age	Years	178,231	11.43	9.00	9.88	0.00	113.00
Total assets	Thousands 2016 USD	178,231	491.79	155.89	650.56	3.44	68,590.17
Total sales	Thousands 2016 USD	178,048	613.50	283.35	824.39	0.00	4,282.91
Num. employees	Number	88,073	24.29	9.00	69.70	1.00	5,866.00
Num. new employees	Number	88,073	7.87	2.00	19.34	0.00	141.00
Num. new switching employees	number	88,073	4.62	1.00	12.51	0.00	91.00
Num. newly appearing employees	number	88,073	3.05	1.00	7.19	0.00	50.00
Corporate income tax paid	Thousands 2016 USD	178,231	12.43	3.48	33.81	0.07	504.77
EBITDA	Thousands 2016 USD	178,231	58.48	26.35	113.90	-151.41	626.50

Panel B: Firm financial ratios and indicators in $t = -1$

Variable	Units	Obs.	Mean	Median	Std. Dev.	Min.	Max.
Firm sales growth$_{t=-2 \text{ to } t=-1}$	YOY growth rate	178,048	0.07	0.04	0.10	-0.29	1.02
Sales volatility	Standard deviation	178,048	26.83	14.10	31.55	0.00	123.44
ROA	Net income/Total assets	178,048	0.13	0.07	0.55	-3.36	3.08
Current ratio	Current assets/Current liabilities	178,231	2.87	1.43	5.01	0.16	32.92
Debt-to-asset ratio	Total debt/Total assets	178,048	0.16	0.02	0.25	0.00	1.42
Tangible asset ratio	PP&E/Total assets	178,048	0.31	0.19	0.57	0.00	1.00
Intangible asset ratio	Intangible assets/Total assets	178,231	0.02	0.00	0.94	0.00	0.95
Interest expense ratio	Interest expense/Gross profit	178,048	0.02	0.00	0.05	0.00	0.33

Panel C: Firms with new commercial bank debt in $t = -1$

Variable	Units	Obs.	Mean	Median	Std. Dev.	Min.	Max.
Bank relationships	Number	34,391	1.45	1.00	0.74	1.00	4.00
New bank loans	Number	34,391	3.26	2.00	3.39	1.00	18.00
Value new bank loans	Thousands 2016 USD	34,391	104.01	16.59	263.38	0.90	1,565.30
Avg. interest rate, new bank loans	Percent	34,391	13.41	12.00	5.52	3.35	27.98
Avg. maturity, new bank loans	Months	34,391	11.62	5.98	14.38	0.08	60.43
Loans with write-downs	Percent	34,391	0.08	0.00	0.38	0.00	3.00
Avg. days late on bank loans	Days	34,391	8.14	0.00	88.93	0.00	1,131.50

Notes. *Firm age* is years from incorporation date. *Num. employees* is reported to the Social Security Administration. Switching employees are those that were recorded as working for another company in the previous year. Newly appearing employees were not recorded as having worked for any prior company since 2007. *EBITDA* is earnings before interest, taxes, depreciation, and amortization. *Firm sales growth* is year-over-year growth in the 3-year moving average of monthly sales. *Sales volatility* is the 3-year moving standard deviation of monthly sales. *ROA* is return on assets (net income/total assets). *Current ratio* is the ratio of current assets and current liabilities. *Debt-to-asset ratio* is total debt divided by total assets. *Tangible asset ratio* is the ratio of the value of property, plant, and equipment (PP&E) to total assets. *Interest expense ratio* is interest expense divided by gross profit. The bank sample conditions on firms having new bank debt at $t = -1$. *Num. bank relationships* is the number of unique banks with which the firm has outstanding bank debt. *Avg. interest rate* is the average, annualized interest rate at issuance, weighted by loan size. *Avg. maturity* is the average number of months-to-maturity at issuance, weighted by loan size, in months. Continuous variables are winsorized at the 1% and 99% levels. Currency values are in thousands of 2016 USD.

TABLE V

Effect of firm position in the Special Taxpayer monitoring program's internal ranking index on the probability of treatment assignment

$1(Monitoring)_{t=-1}$

	Pooled	2010	2011	2012	2013	2014	2015
Above	0.249***	0.299***	0.235***	0.231***	0.272***	0.331***	0.156***
	(0.007)	(0.011)	(0.018)	(0.011)	(0.022)	(0.023)	(0.012)
Constant	0.002***	0.000	0.001***	0.019***	0.006	0.081***	0.063***
	(0.000)	(0.000)	(0.000)	(0.005)	(0.010)	(0.019)	(0.013)
Observations	179,541	83,114	86,041	3,885	2,453	1,230	2,818
Cohort × province FE	Yes	No	No	No	No	No	No
Province FE	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table shows the effect of a firm's position in the ranking by the firm ranking value versus the (normalized) cutoff on the firm's probability of being chosen for the Special Taxpayer monitoring program in event year $t = 0$. The table reports the constant ω and the coefficient π of the regression model: $Monitoring_{j,p,c,t=0} = \omega + \pi above_{j,p,c,t=-1} + f(RV_{j,p,c,t=-1} - cutoff_{p,c,t=-1}) + \zeta_{j,p,c,t=0}$, where *Monitoring* is an indicator that equals one if the firm is chosen for the Special Taxpayer monitoring program in the event year ($t = 0$), *above* is an indicator that equals one if j's normalized firm ranking variable value is greater than the pooled cutoff at zero, and *f(RV - cutoff)* is a 3rd degree polynomial of the normalized firm ranking variable. Columns (2) through (7) estimate the jump in the probability of treatment around the cutoff for each treatment cohort separately. Standard errors are clustered at the industry level (243 industries). *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

TABLE VI

Tests for jumps in predetermined covariates around the firm ranking variable cutoffs (normalized to zero)

Panel A: $ln(Outcome)_{t=-1}$

Outcome	Effect	Standard Error	P Value
Firm size (ln(Total assets))	0.61	0.68	0.365
Firm age	-4.32	5.49	0.431
Sales volatility	0.06	0.05	0.227
Sales	-0.44	1.02	0.666
ln(Tangible asset ratio)	0.43	0.75	0.568
ln(Interest expense ratio)	0.26	0.99	0.79
ln(ROA)	-0.03	0.61	0.967
ln(Current ratio)	-0.04	0.34	0.912
ln(Debt-to-asset ratio)	-0.32	0.53	0.548
ln(Val. bank debt write-downs)	-0.56	1.04	0.586
Avg. days late loan payments	-12.44	31.29	0.691
ln(Value outstanding bank debt)	-0.03	1.46	0.981
ln(Avg. term-to-maturity bank debt)	-0.31	0.98	0.754
ln(Avg. interest rate bank debt)	-0.04	0.20	0.837

Panel B: $ln(Outcome)_{t=-1} - ln(Outcome)_{t=-2}$

Outcome	Effect	Standard Error	P Value
Firm size	0.78*	0.41	0.059
Sales volatility	38.62	56.56	0.495
Sales growth	-2.5	2.54	0.326
ln(Tangible asset ratio)	-0.02	0.71	0.983
ln(Interest expense ratio)	0.86	3.14	0.785
ln(ROA)	-0.01	0.08	0.885
ln(Current ratio)	0.32	0.50	0.519
ln(Debt-to-asset ratio)	1.17	1.64	0.473
ln(Val. bank debt write-downs)	-0.65	1.35	0.636
Avg. days late loan payments	-6.38	6.90	0.355
ln(Val. outstanding bank debt)	6.42	4.28	0.133
ln(Avg. term-to-maturity bank debt)	3.63	3.08	0.239
ln(Avg. interest rate bank debt)	1.94	5.08	0.703

Notes. Estimates are from local linear regressions within MSE-optimal bandwidths. All specifications include triangular kernel weights and cohort times province fixed effects. Panel A presents tests on levels in the year before treatment assignment. Data on borrowers' days behind payment on outstanding bank debt are from 2012. Panel B presents changes from the year before to two years before treatment assignment, with the caveat that I do not have bank loan data before 2009. *Num. loans with write-downs* are the number of loans per company-year for which a bank has indicated that it will not receive 100% payment at maturity. *Avg. days late on bank loans* is the average number of days that outstanding loans are late on an interest payment. See Table IV for other variable definitions. Standard errors are clustered at the industry level (243 industries). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE VII

Effect of government monitoring on firm cost of bank credit

$ln(Interest\ rate\ new\ bank\ debt)_{Avg.\ t=1\ to\ t=3}$

	OLS	Regression Discontinuity			
		poly. = 2	poly. = 2	MSE bounds	MSE bounds
Monitoring_0	-0.496***	-0.331***	-0.219***	-0.223***	-0.197***
	(0.019)	(0.044)	(0.051)	(0.059)	(0.044)
Firm size_{-1}			-0.040***		-0.033***
			(0.003)		(0.003)
$\text{Sales volatility}_{-1}$			0.000		0.000
			(0.000)		(0.000)
Sales growth_{-1}			-0.002		-0.003
			(0.002)		(0.002)
$\text{ln(Tangible asset ratio)}_{-1}$			0.002		-0.001
			(0.002)		(0.001)
$\text{ln(Interest expense ratio)}_{-1}$			0.000		0.000
			(0.000)		(0.000)
ln(ROA)_{-1}			-0.0002***		-0.0003
			(0.000)		(0.000)
$\text{ln(Current ratio)}_{-1}$			-0.000		-0.000
			(0.000)		(0.000)
$\text{ln(Debt-to-asset ratio)}_{-1}$			0.050***		0.060***
			(0.020)		(0.018)
$\text{ln(Loan maturity)}_{-1}$			-0.001***		-0.001***
			(0.000)		(0.000)
$\text{ln(Loan write-downs)}_{-1}$			0.007***		0.010***
			(0.000)		(0.002)
Constant	2.820***	2.751***	2.964***	2.475***	2.70***
	(0.013)	(0.019)	(0.041)	(0.017)	(0.022)
Observations	179,541	179,541	179,541	35,530	35,530
F Statistic		120.169	85.051	84.500	83.993
Cohort × province FE	Yes	Yes	Yes	Yes	Yes
Triangular kernel weights	No	Yes	Yes	Yes	Yes

Notes. Estimates are of the equation: $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_{c,p} + C'X + \epsilon_{j,c,p,t}$ of the outcome Y, the natural log of the weighted average interest rate spread on new bank debt over the three years following the event year, on *Monitoring*, which equals one if the firm is chosen in event year $t = 0$ for the Special Taxpayer monitoring program. Estimations include cohort × province fixed effects and triangular kernel weights. Column 1 reports OLS results while Columns 2 and 3 report fuzzy RD estimates using all data and with second-degree polynomials of the running variable. Columns 4 and 5 estimate the fuzzy RD specification via local linear regression within province-year, MSE-optimal bounds of the firm ranking variable around the zero cutoff. See Table IV for control variable definitions. Standard errors are clustered at the industry level (243 industries). The reported F statistic is the Kleibergen-Paap rk Wald F statistic on a test for weak instruments. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

TABLE VIII

Effect of government monitoring on the size and term-to-maturity of new bank loans

$ln(Outcome)_{Avg.\ t=1\ to\ t=3}$

Outcome	Val. new bank debt/ Total debt$_{t=-1}$	Num. new bank loans	Avg. maturity new loans
Monitoring	0.236***	0.517***	0.545***
	(0.020)	(0.156)	(0.176)
Firm size$_{-1}$	0.069***	0.128***	0.022
	(0.007)	(0.028)	(0.019)
Sales volatility$_{-1}$	-0.000	-0.000*	-0.001***
	(0.000)	(0.000)	(0.000)
Sales growth$_{-1}$	0.017*	0.013	0.028
	(0.001)	(0.030)	(0.18)
ln(Tangible asset ratio)$_{-1}$	0.210***	0.167***	0.163**
	(0.026)	(0.064)	(0.067)
ln(Interest expense ratio)$_{-1}$	-0.000***	-0.073	-0.000
	(0.000)	(0.067)	(0.000)
ln(ROA)$_{-1}$	0.002***	0.053*	0.010***
	(0.000)	(0.031)	(0.004)
ln(Current ratio)$_{-1}$	0.000	0.004*	-0.000
	(0.000)	(0.002)	(0.000)
ln(Debt-to-asset ratio)$_{-1}$	-0.045**	-0.727***	0.000
	(0.019)	(0.068)	(0.021)
ln(Val. bank debt write-downs)$_{-1}$	-0.004***	0.001	-0.006***
	(0.001)	(0.001)	(0.001)
ln(Avg. bank debt maturity)$_{-1}$	-0.008***	-0.011***	
	(0.001)	(0.001)	
Constant	1.094***	0.400***	1.324***
	(0.093)	(0.035)	(0.248)
Observations	36,258	29,093	28,793
Cohort × province FE	Yes	Yes	Yes
Triangular kernel weights	Yes	Yes	Yes

Notes. Estimates are from local linear regressions, estimated around the normalized cutoff zero within MSE-optimal bandwidths, of the equation: $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_{c,p} + A'X + \epsilon_{j,c,p,t}$ of the outcome Y, the natural log of the outcome over the three years following the event year, on *Monitoring*, an indicator variable that equals one if the firm is chosen in event year $t = 0$ for the Special Taxpayer monitoring program, which is instrumented by the discontinuity in the probability of treatment around the cutoff. Estimations include cohort × province fixed effects and triangular kernel weights that assign more weight to observations close to the cutoffs. See Table IV for outcome variable definitions. Standard errors are clustered at the industry level (243 industries). *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

TABLE IX

Monitored firms access less-informed financing sources

Outcome	1(New bank)	ln(New investors)
Monitoring_0	0.727***	0.337***
	(0.077)	(0.115)
Constant	0.071***	1.259***
	(0.004)	(0.051)
Observations	58,378	50,732
Cohort × province FE	Yes	Yes
Triangular Kernel Weights	Yes	Yes

Notes. Estimates are from local linear regressions, estimated around the normalized cutoff zero within MSE-optimal bandwidths, of the equation: $Y_{j,c,p,t} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_{c,p} + \epsilon_{j,c,p,t}$ of the outcome Y on *Monitoring*, an indicator variable that equals one if the firm is chosen in event year $t = 0$ for the Special Taxpayer monitoring program, which is instrumented by the discontinuity in the probability of treatment around the cutoff. Estimations include cohort × province fixed effects and triangular kernel weights that assign more weight to observations close to the cutoffs. *1(New bank)* is an indicator variable that equals one if the firm has never borrowed from that bank before up to that year. For this outcome, a probit model is estimated. *ln(New investors)* is the log change in the number of new private investors, defined as a firm or individual that buys a minority stake in the firm but that has not invested in the firm before. Standard errors are clustered at the industry level (243 industries). *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

TABLE X

The treatment effect is larger for monitored firms that had more severe asymmetric information, and those that were more likely to be evading taxes and manipulating their statements before monitoring assignment

$ln(Interest\ rate\ new\ bank\ debt)_{Avg.\ t=1\ to\ t=3}$

Characteristic$_{-1}$	Baseline		Monitoring$_0$ × Characteristic$_{-1}$		Constant		Obs.
1. Size (assets)	-0.442***	(0.101)	0.035	(0.107)	2.624***	(0.010)	178,231
2. Size (sales)	-0.467***	(0.111)	0.160***	(0.033)	2.601***	(0.011)	178,231
3. Size (employment)	-0.561***	(0.063)	0.055	(0.045)	2.555***	(0.017)	88,073
4. Firm age	-0.534***	(0.052)	0.055***	(0.020)	2.681***	(0.015)	178,231
5. 1(Major urban center)	-1.511***	(0.117)	0.074**	(0.031)	2.639***	(0.016)	178,231
6. Bank size	-1.921***	(0.210)	0.495***	(0.174)	2.583***	(0.017)	34,391
7. 1(Above median capital intensity)	-1.865***	(0.208)	0.709***	(0.237)	2.688***	(0.017)	178,231
8. Tax payment risk score	-1.434***	(0.122)	-0.458***	(0.172)	2.647***	(0.011)	40,420
9. 1(Revenue < lower bound - \$10,000)	-0.263***	(0.092)	-0.526*	(0.318)	2.499***	(0.014)	35,530
10. Benford's law: MAD	-1.100***	(0.153)	-0.273**	(0.109)	2.653***	(0.010)	78,498
11. Benford's law: 1(χ^2 test rejects null)	-0.879***	(0.070)	-0.490***	(0.156)	2.571***	(0.010)	78,498

Notes. Estimates are from polynomial regressions, estimated around the normalized cutoff zero, of the equation: $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} = \alpha + \beta monitoring_{j,p,c,t=0} + \delta monitoring_{j,p,c,t=0} \times characteristic_{j,t=-1} + \mu characteristic_{j,t=-1} + n.r.v._{j,p,c,t=-1} + n.r.v._{j,p,c,t=-1}^2 + \gamma_{c,p} + \epsilon_{j,c,p,t}$ of the outcome Y, the natural log of the weighted average interest rate spread on new bank debt over the three years following the event year, on *Monitoring*, an indicator variable that equals one if the firm is chosen in event year $t = 0$ for the Special Taxpayer monitoring program and zero otherwise, which is instrumented by the discontinuity in the probability of treatment around the cutoff, and the interaction of this variable with $characteristic_{j,t=-1}$, a predetermined firm proxy for firm tax evasion. Estimations include cohort × province fixed effects and triangular kernel weights that assign more weight to observations close to the cutoffs. *Size* is the natural log of firm size, proxied by the value of its assets, its sales, or the number of its employees. *1(Major urban center)* is equal to one if the firm is incorporated in Quito or Guayaquil. *Bank size* is the within-year ranking by bank total outstanding assets, where the largest bank has rank 1. *1(Above median capital intensity)* equals one if the firm's ratio $\frac{Tangible\ assets}{employees}$ is greater than the median value of its industry. *Tax payment risk score* is an internal, firm-level, annual SRI score that ranges from 0 to 3.65, where a lower score indicates a lower risk of filing late or not at all. *1(Reported revenue < lower bound - \$10,000)* is an indicator equaling one when a firm reports more than \$10,000 less than the lower bound revenue that the SRI estimated for that firm using third-party data (suppliers, credit cards, etc.). *MAD* is the Mean Average Deviation of the divergence between the distribution of the first digits of the line items of firm balance, income, and tax statements and that proposed by Benford's Law. *1(χ^2 test rejects null)* is an indicator that is one when the firm's distribution of first digits from its statements diverge significantly, at the 5% level, from the distribution hypothesized by Benford's Law. Standard errors are clustered at the industry level (243 industries). *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

TABLE XI

Effect of treatment assignment in 2010—2015 over the three years post-treatment on real firm outcomes

$ln(Outcome_{Avg.\ t=1\ to\ t=3}) - ln(Outcome_{-1})$

Panel A: Firm Employment			
Outcome	New employees/Total employees$_{-1}$	New employees/Total employees$_{-1}$	Newly appearing employees/Total employees$_{-1}$
Monitoring$_0$	0.350**	0.161*	0.075***
	(0.160)	(0.103)	(0.012)
Constant	0.203***	1.904***	0.827***
	(0.004)	(0.049)	(0.052)
Observations	35,369	35,369	35,369
Cohort × province FE	Yes	Yes	Yes
Controls	No	Yes	Yes
Triangular kernel weights	Yes	Yes	Yes
Panel B: Firm Investment			
Outcome	PP&E/Total assets$_{-1}$	PP&E/Total assets$_{-1}$	Intangible assets/Total assets$_{-1}$
Monitoring$_0$	0.100***	0.115***	-0.027
	(0.029)	(0.032)	(0.081)
Constant	1.118***	2.061***	0.791***
	(0.078)	(1.045)	(0.011)
Observations	23,250	23,250	23,250
Controls	No	Yes	Yes
Cohort × province FE	Yes	Yes	Yes
Triangular kernel weights	Yes	Yes	Yes

Notes. This table reports the effects of monitoring by the SRI on proxies for firm labor & capital. Estimates are from local linear regressions, estimated around the normalized cutoff zero within MSE-optimal bandwidths, of the equation: $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} - Y_{j,c,p,t=-1} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_{c,p} + \epsilon_{j,c,p,t}$ of the outcome Y, the natural log of the average outcome over the three years following the event year less the log value the year before, on *Monitoring*, an indicator variable that equals one if the firm is chosen in event year $t = 0$ for the Special Taxpayer monitoring program, which is instrumented by the discontinuity in the probability of treatment around the cutoff. Estimations include cohort × province fixed effects and triangular kernel weights that assign more weight to observations close to the cutoffs. *Num. employees* is the number of employees reported by the firm to the Ecuadorian Social Security Administration. "Appearing" employees are those not recorded as working as a formal employee of another company before being employed at the firm. *PP&E* is property, plant, & equipment scaled by the value of the firm's total assets before treatment assignment. Standard errors are clustered at the industry level (243 industries). *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

I. Online Appendix

A1 Additional descriptive statistics and robustness tests

TABLE A1
Summary statistics of pre-determined variables ($t = -1$) that are within the MSE-optimal bandwidths corresponding to the weighted average interest rate on new bank debt regressions

Variable	Units	Obs.	Mean	Median	Std. Dev.	Min.	Max.
Panel A: Firm characteristics in $t = -1$							
Firm age	Years	35,530	11.96	9.00	10.32	0.00	113.00
Total assets	Thousands 2016 USD	35,530	1,107.02	600.35	1,480.11	68.92	68,590.17
Num. employees	Number	30,037	46.34	20.00	106.78	1.00	5,866.00
Num. new employees	Number	88,073	15.25	5.00	28.22	0.00	139.00
Num. new switching employees	number	88,073	9.17	2.00	18.40	0.00	91.00
Num. newly appearing employees	number	88,073	5.60	2.00	10.19	0.00	50.00
Corporate income tax paid	Thousands 2016 USD	35,530	28.41	14.40	222.23	1.04	33,267.62
Panel B: Firm financial ratios and indicators in $t = -1$							
EBITDA	Thousands 2016 USD	35,530	135.17	85.95	166.88	-151.41	626.50
ROA	Net income/Total assets	35,530	0.11	0.08	0.41	-3.36	3.08
Sales	Thousands 2016 USD	35,530	1,538.94	1,219.46	1,030.27	0.00	4,282.91
Firm sales growth$_{t=-2 \text{ to } t=-1}$	YOY growth rate	32,452	0.27	0.19	0.37	-1.10	1.85
Sales volatility	Standard deviation	35,530	55.32	43.23	38.68	0.00	123.44
Current ratio	Current assets/Current liabilities	35,530	3.51	1.43	9.95	0.16	32.92
Debt-to-asset ratio	Total debt/Total assets	35,530	0.16	0.06	0.22	0.00	1.42
Working capital ratio	(Current assets - current liabilities)/Gross Profit	35,530	0.66	0.20	3.00	-14.24	30.17
Tangible asset ratio	PP&E/Total assets	35,530	0.21	0.11	0.25	0	1
Intangible asset ratio	Intangible assets/Total assets	35,530	0.07	0.00	0.60	0.00	0.95
t Interest expense ratio	Interest expense/Gross profit	35,530	0.02	0.00	0.04	0.00	0.16
Panel C: Firms with new commercial bank debt in $t = -1$							
Bank relationships	Number	12,677	1.57	1.00	0.83	1.00	4.00
New bank loans	Number	12,677	4.50	3.00	4.40	1.00	18.00
Value new bank loans	Thousands 2016 USD	12,677	216.70	53.23	378.16	0.900	1,565.30
Avg. interest rate, new bank loans	Percent	12,677	10.89	11.22	3.89	3.35	27.98
Avg. maturity, new bank loans	Months	12,677	11.88	6.78	14.46	0.08	60.43
Loans with write-downs	Percent	12,677	0.09	0.00	0.44	0.00	3.00
Avg. days late on bank loans	Days	12,677	3.58	0.00	60.23	0.00	1,131.50

Notes. Continuous variables are winsorized at the 1% and 99% levels. Currency values are in thousands of 2016 USD. See IV for variable definitions.

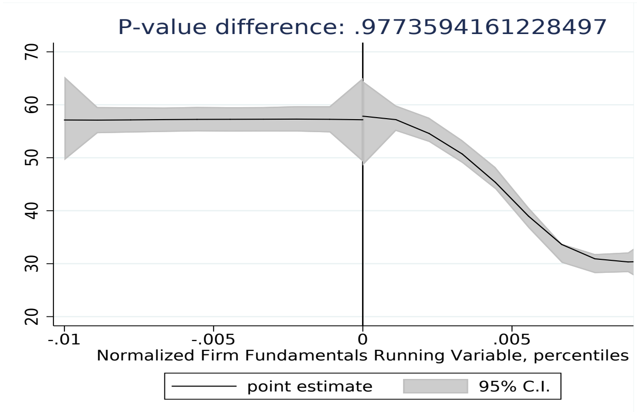
TABLE A2

Dynamics of the effect of government monitoring on firm cost of bank credit

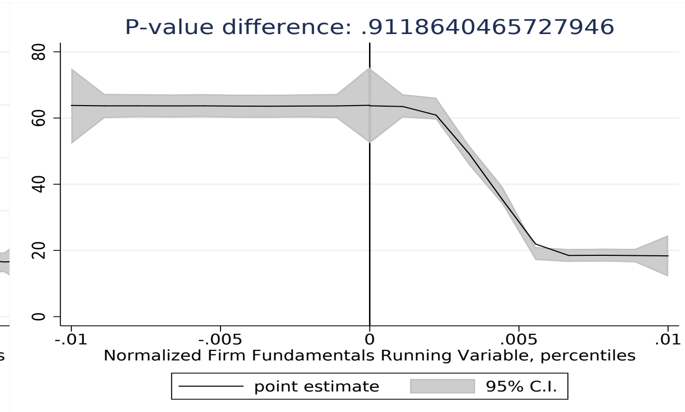
$ln(Interest\ rate\ new\ bank\ debt)_t$

Years out from assignment year	1	2	3
Monitoring	-0.282***	-0.169***	-0.274*
	(0.097)	(0.065)	(0.163)
Firm size$_{-1}$	-0.059***	-0.063***	-0.056***
	(0.005)	(0.007)	(0.008)
Sales volatility$_{-1}$	0.002***	0.002***	0.001
	(0.001)	(0.001)	(0.001)
Sales growth$_{-1}$	0.010	0.012	0.018
	(0.050)	(0.065)	(0.063)
ln(Tangible asset ratio)$_{-1}$	-0.039***	-0.065***	-0.041*
	(0.011)	(0.019)	(0.022)
ln(Interest expense ratio)$_{-1}$	0.000***	0.000***	0.005*
	(0.000)	(0.000)	(0.003)
ln(ROA)$_{-1}$	0.0003	0.002	-0.002
	(0.003)	(0.003)	(0.003)
ln(Current ratio)$_{-1}$	-0.000	0.000	-0.000***
	(0.000)	(0.000)	(0.000)
ln(Debt-to-asset ratio)$_{-1}$	0.109***	0.019	0.013
	(0.038)	(0.084)	(0.054)
ln(Val. bank debt write-downs)$_{-1}$	0.008***	0.014***	0.006
	(0.002)	(0.003)	(0.005)
ln(Avg. bank debt maturity)$_{-1}$	-0.008***	-0.003	-0.008***
	(0.002)	(0.002)	(0.003)
Constant	2.093***	1.994***	1.906***
	(0.059)	(0.088)	(0.100)
Observations	14,750	13,274	10,376
F Statistic	47.986	58.181	60.445
Cohort × province FE	Yes	Yes	Yes
Triangular kernel weights	Yes	Yes	Yes

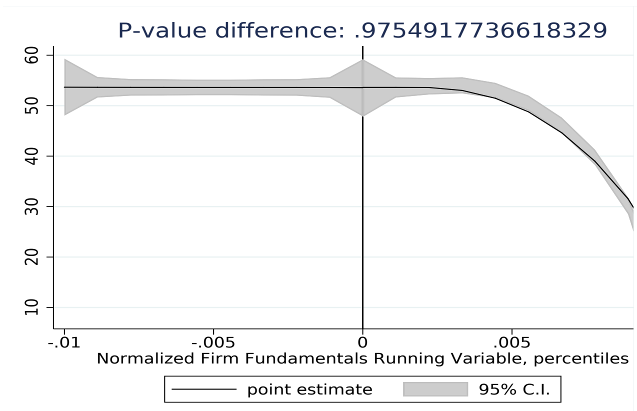
Notes. Estimates are from local linear regressions, estimated around the normalized cutoff zero within MSE-optimal bandwidths, of the equation: $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_{c,p} + A'X + \epsilon_{j,c,p,t}$ of the outcome Y, the natural log of the weighted average interest rate spread on new bank debt over the three years following the event year, on *Monitoring*, an indicator variable that equals one if the firm is chosen in event year $t = 0$ for the Special Taxpayer monitoring program, which is instrumented by the discontinuity in the probability of treatment around the cutoff. Estimations include cohort × province fixed effects and triangular kernel weights that assign more weight to observations close to the cutoffs. See Table IV for outcome variable definitions. Standard errors are clustered at the industry level (243 industries). The reported F statistic is the Kleibergen-Paap rk Wald F statistic on a test for weak instruments. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.



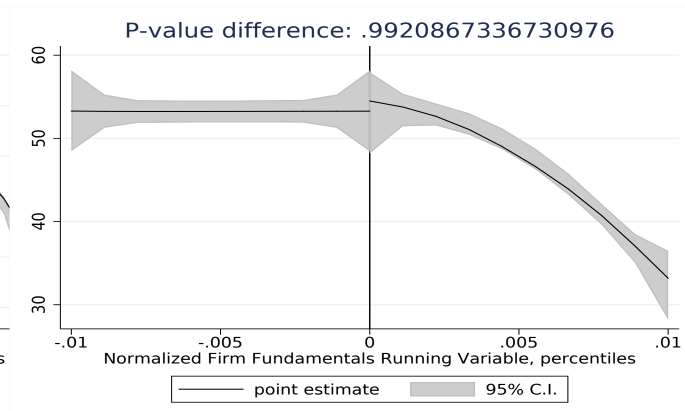
(a) 2010



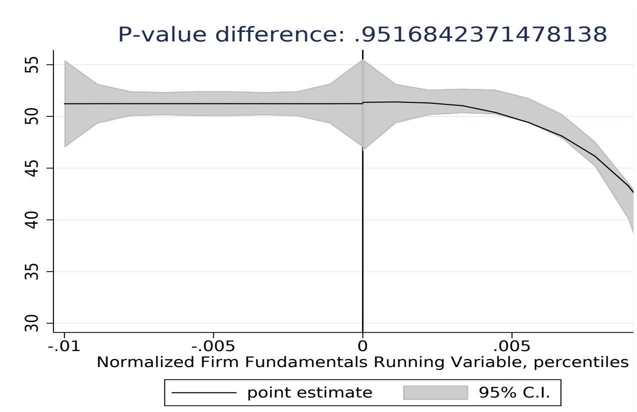
(b) 2011



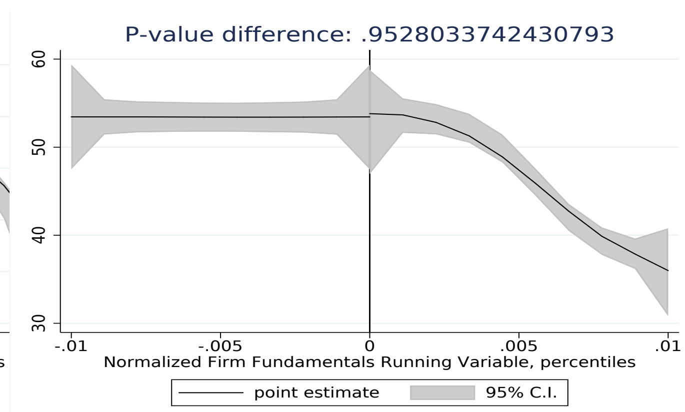
(c) 2012



(d) 2013



(e) 2014

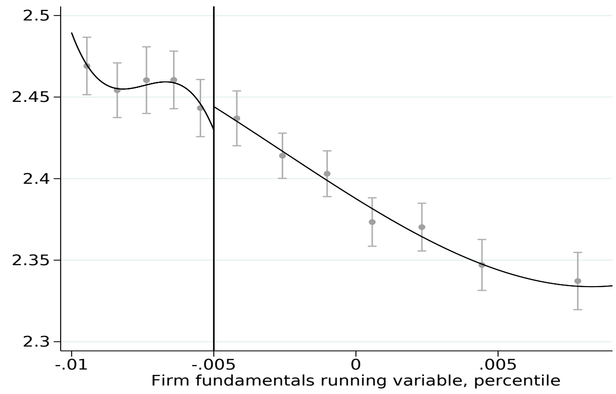


(f) 2015

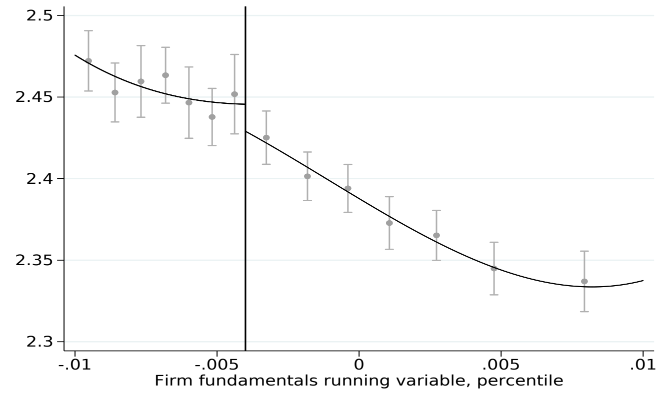
Figure A1

Tests for firm manipulation of the normalized firm ranking variable around the normalized treatment cutoff at zero, by year.

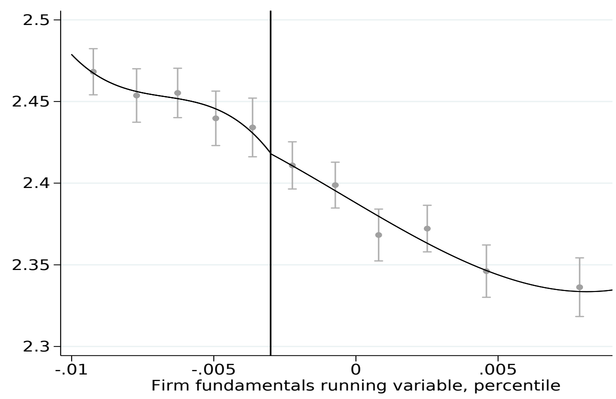
For each year, the figure shows the local polynomial density estimates (the solid line), and associated, point-wise, robust confidence intervals (shaded area), on either side of the pooled cutoff (the vertical line). It is generated using the algorithm and methodology of McCrary [2008], as well as the code provided by the author. Each figure reports the p-value of a test for a break in the estimated density across the threshold. See Figure I for the same figure produced for the pooled sample.



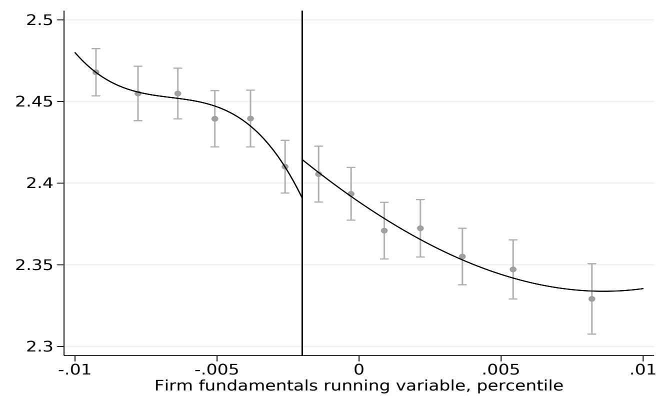
(a) c = -0.005



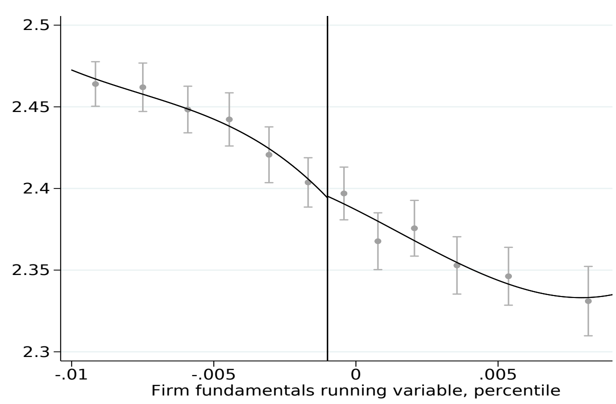
(b) c = -0.004



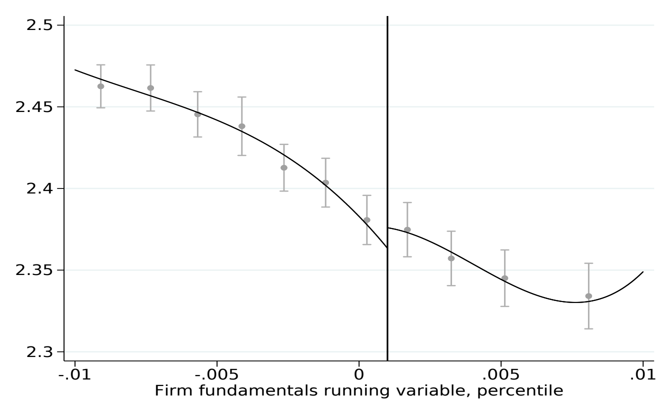
(c) c = -0.003



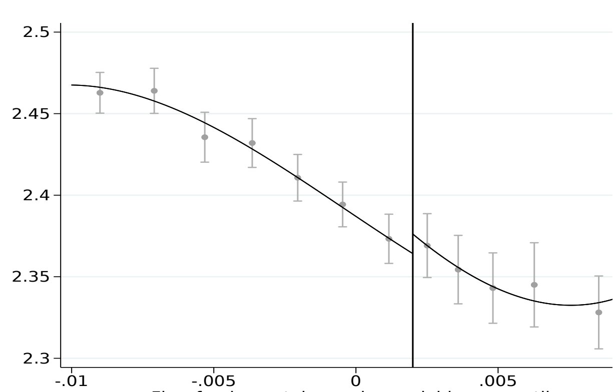
(d) c = -0.002



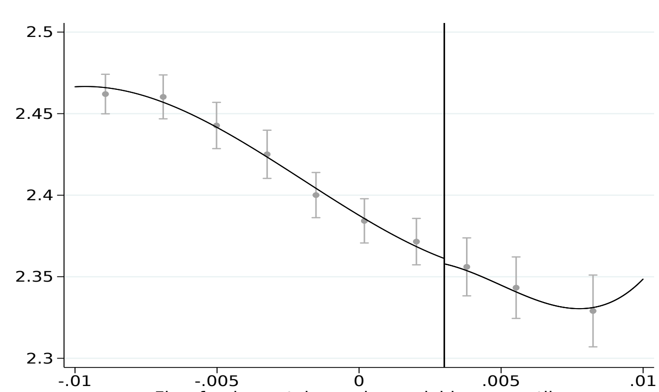
(e) c = -0.001



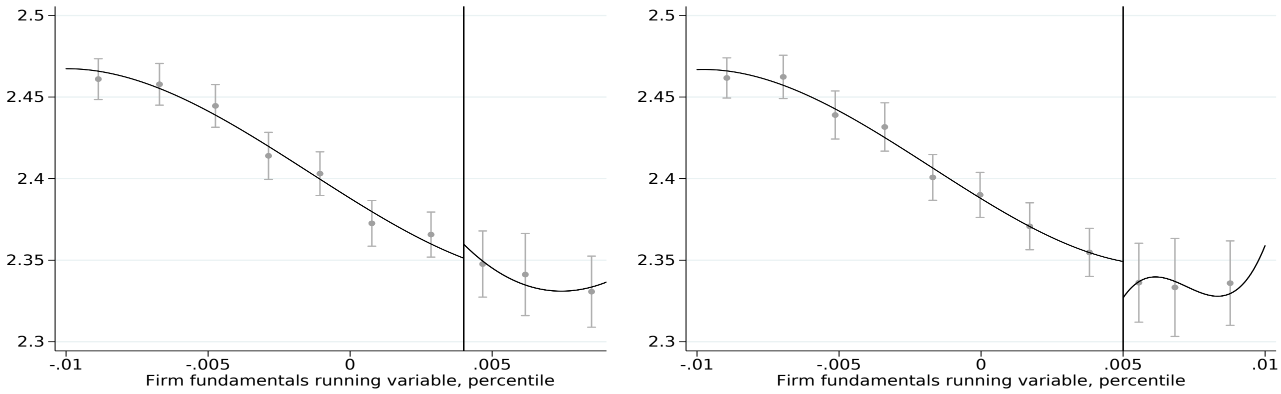
(f) c = 0.001



(g) c = 0.002



(h) c = 0.003



(i) c = 0.004

(j) c = 0.005

Figure A2

Effect of monitoring on firm cost of bank debt around placebo cutoff values

The figure shows the effect on the average interest rate on bank debt over the three years following the event year ($t = 0$) for all firms who were eligible to be chosen for the Special Taxpayer monitoring program, regardless of whether they were chosen, i.e., the reduced-form estimates, in percentile bins of the firm ranking variable, and the fitted values and 95% confidence intervals from the regression model $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} = \alpha_0 + \beta \times \pi \times above_{j,c,p,t=-1} + (RV_{j,c,p,t=-1} - cutoff_{c,p,t=-1}) + \gamma_{c,p} + \chi_{j,c,p,t}$. Y is the natural log of the average interest rate on new bank debt over the three years following the event year, *above* is an indicator variable that equals one if the firm's normalized fundamentals running variable value is greater than the pooled cutoff at zero (represented by the black vertical line), *f(RV - cutoff)* is a 3rd degree polynomial of the normalized firm ranking variable, and $\gamma_{c,p}$ are cohort times province fixed effects. Standard errors are clustered at the industry level (243 industries). In each panel, *cutoff* is varied around the true cutoff of zero, to check for discontinuities other than the one actually used by the SRI. The figures show no effect, strengthening the identification assumption that the cutoff induces quasi-random variation in treatment. It also provides supporting evidence for the assumption of no other discontinuous policy change around the cutoff.

TABLE A3

Effect of monitoring on firm cost of bank credit by each assignment year

$ln(Interest\ rate\ new\ bank\ debt)_{Avg.\ t=1\ to\ t=3}$

Event Year	2010	2011	2012	2013	2014	2015
Monitoring_0	-0.329***	-0.381***	-0.066***	-0.027	-0.075***	-0.080***
	(0.017)	(0.031)	(0.016)	(0.025)	(0.027)	(0.015)
Constant	2.595***	2.572***	2.476***	2.392***	2.406***	2.377***
	(0.015)	(0.017)	(0.023)	(0.036)	(0.029)	(0.023)
Observations	83,114	86,041	3,885	2,453	1,230	2,818
Province FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Column 2 reports the number of all firms in the Special Taxpayer monitoring program over the period 2010—2015. Column 3 reports the number of newly treated firms that were chosen for the program that year. Column 4 reports the number of firms that the SRI removed from the monitoring program that year and Column 5 the number that were removed by the SRI from the program and yet remain in my sample.This table reports the effects of monitoring by the SRI on the cost of bank credit for each treatment cohort. Estimates are from local linear regressions, estimated around the normalized cutoff zero within MSE-optimal bandwidths, of the equation: $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} - Y_{j,c,p,t=-1} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_p + \epsilon_{j,c,p,t}$ of the outcome Y, the natural log of the average interest rate on new bank debt over the three years following the event year, on *Monitoring*, an indicator variable that equals one if the firm is chosen in event year $t = 0$ for the Special Taxpayer monitoring program and zero otherwise, which is instrumented by the discontinuity in the probability of treatment around the cutoff. Estimations include province fixed effects and triangular kernel weights that assign more weight to observations close to the cutoffs. Standard errors are clustered at the industry level (243 industries). *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

TABLE A4

The effect of government monitoring on firm bank borrowing cost is robust to varying the polynomial specification of the firm ranking variable

$ln(Interest\ rate\ new\ bank\ debt)_{Avg.\ t=1\ to\ t=3}$

Polynomial	1	2	3	4
Monitoring_0	-0.415***	-0.424***	-0.516***	-0.263***
	(0.045)	(0.080)	(0.164)	(0.101)
Constant	2.813***	2.756***	2.720***	2.384***
	(0.018)	(0.019)	(0.020)	(0.015)
Observations	179,541	179,541	179,541	179,541
Cohort × province FE	Yes	Yes	Yes	Yes
Triangular kernel weights	Yes	Yes	Yes	Yes

Notes. Estimates are from polynomial regressions over the full support of the running variable, estimated around the normalized cutoff zero within MSE-optimal bandwidths, of the equation: $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} - Y_{j,c,p,t=-1} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_{c,p} + \epsilon_{j,c,p,t}$. This corresponds to Equation (3), the 2SLS estimates. The outcome Y is the natural log of the average interest rate on new bank debt over the three years following the event year, on *Monitoring*, an indicator variable that equals one if the firm is chosen in event year $t = 0$ for the Special Taxpayer monitoring program and zero otherwise, which is instrumented by the discontinuity in the probability of treatment around the cutoff. Estimations include cohort × province fixed effects and triangular kernel weights that assign more weight to observations close to the cutoffs. The 2SLS estimate shown is the ratio of the reduced form estimate (Equation (4)) to the first stage (Equation (2)). It is the polynomial of the standardized firm ranking variable, $f(RV - cutoff)$, included in Equation (4) and Equation (2), that is varied in this table. Standard errors are clustered at the industry level (243 industries). *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

TABLE A5

The effect of government monitoring on firm bank borrowing cost is robust to varying the estimation bandwidth

$ln(Annualized\ interest\ rate\ new\ bank\ debt)_{Avg.\ t=1\ to\ t=3}$

	.5*MSE BW	2*MSE BW	Common MSE BW	Common CER BW
Monitoring_0	-0.315***	-0.359***	-0.2918***	-0.296**
	(0.103)	(0.091)	(0.092)	(0.115)
Observations	20,028	50,088	47,415	38,780
Cohort × province FE	Yes	Yes	Yes	Yes
Triangular kernel weights	Yes	Yes	Yes	Yes

Notes. Estimates are from local linear regressions, estimated around the normalized cutoff zero within varying bandwidths, as indicated by the column headings. The estimating equation is $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_{c,p} + \epsilon_{j,c,p,t}$ of the outcome Y, the natural log of the average interest rate on new bank debt over the three years following the event year, on *Monitoring*, an indicator variable that equals one if the firm is chosen in event year $t = 0$ to for the Special Taxpayer monitoring program and zero otherwise, which is instrumented by the discontinuity in the probability of treatment around the cutoff. Estimations include cohort × province fixed effects and triangular kernel weights that assign more weight to observations close to the cutoffs. *MSE BW* is the optimal bandwidth in a minimized mean-squared error sense, as proposed in Imbens and Kalyanaraman [2012]. *CER BW* is the optimal bandwidth in a minimized coverage-error rate sense, as proposed in Calonico, Cattaneo and Farrell [2018]. *Common* denotes imposing one, common bandwidth across all provinces and all years, as opposed to separately estimating it within each province and cohort. Standard errors are clustered at the industry level (243 industries). *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

TABLE A6

Effect of government monitoring on firm cost of bank credit, with cohort and province fixed effects

$ln(Interest\ rate\ new\ bank\ debt)_{Avg.\ t=1\ to\ t=3}$

	RD (p = 2)	RD (MSE bounds)
Monitoring_0	-0.450***	-0.294***
	(0.085)	(0.090)
Constant	2.479***	2.482***
	(0.020)	(0.014)
Observations	179,541	29,866
Cohort and province FE	Yes	Yes

Notes. This table reports the effects of monitoring by the SRI on the cost of bank credit by estimating the regression: $Y_{j,c,p,Avg.\ over\ 3\ years\ after\ t=0} = \alpha + \beta monitoring_{j,p,c,t=0} + \gamma_c + \gamma_p + \epsilon_{j,c,p,t}$ of the outcome Y, the natural log of the average interest rate on new bank debt over the three years following the event year, on *Monitoring*, a treatment indicator variable that equals one if the firm is chosen in event year $t = 0$ (2010 to 2015) for the Special Taxpayer monitoring program, and cohort and province fixed effects. Column 1 reports the fuzzy RD estimates over the support with second-degree polynomials of the running variable. Column 2 repeats the model of Column 1 but includes firm-level controls and estimates via local linear regression within MSE-optimal bounds of the firm ranking variable around the normalized cutoff of zero. Standard errors are clustered at the industry level (243 industries). The reported F statistic is the Kleibergen-Paap rk Wald F statistic on a test for weak instruments. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

A2 Firm financial statements in aggregate conform to Benford's Law

Benford's Law is a mathematical property that states that the probability that a number has any given first, non-zero digit, d, is:

$$P(d) = Log_{10}(d+1) - Log_{10}(d), \text{ d = 1,2,...,9}$$

This equation predicts the following distribution:

Digit	1	2	3	4	5	6	7	8	9
Proportion	0.3010	0.1761	0.1249	0.0969	0.0792	0.0669	0.0580	0.0512	0.0458

Benford's Law was first posited by astronomer Simon Newcomb in 1881. A variety of data has been shown to follow Benford's Law if unmanipulated. Examples include the returns of stock indices (Ley 1996), survey data, tax statements (Nigrini [1996]), and financial statements (Amiram, Bozanic, and Rouen [2015]).

The following figure shows that the observed first-digit distribution of firm statements for Ecuadorian firms in aggregate from 2009 to 2018 closely conforms to the predicted Benford distribution. Indeed the difference is not statistically significant.

Figure A3

Aggregate Benford's Law Analysis. The X-axis shows the first digits and the Y-axis shows the proportions. The dotted line represents the expected proportions of Benford's Law and the bars represent the actual proportions of the first non-zero digit of the line items of firm balance sheet, income, and tax statements of domestic Ecuadorian firms from 2009 to 2018.