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THE DISTRIBUTIVE IMPACT OF REFORMS IN CREDIT ENFORCEMENT: EVIDENCE FROM INDIAN DEBT RECOVERY TRIBUNALS

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THE DISTRIBUTIVE IMPACT OF REFORMS IN CREDIT ENFORCEMENT: EVIDENCE FROM INDIAN DEBT RECOVERY TRIBUNALS

BY ULF VON LILIENFELD-TOAL, DILIP MOOKHERJEE, AND SUJATA VISARIA[1]

It is generally presumed that stronger legal enforcement of lender rights increases credit access for all borrowers because it expands the set of incentive compatible loan contracts. This result relies on an assumption that the supply of credit is infinitely elastic. In contrast, with inelastic supply, stronger enforcement generates general equilibrium effects that may reduce credit access for small borrowers and expand it for wealthy borrowers. In a firm-level panel, we find evidence that an Indian judicial reform that increased banks' ability to recover nonperforming loans had such an adverse distributive impact.

KEYWORDS: Credit enforcement, general equilibrium effect, inelastic credit supply, distributive impact.

1. INTRODUCTION

WHEN IT COMES TO BANKRUPTCY LAW and the enforcement of credit contracts, economists often disagree with popular opinion. It is commonly believed that giving lenders strong rights to appropriate collateral in the event of default makes small borrowers vulnerable and unable to participate in credit markets. However the general view among economists is that weak lender rights have adverse ex ante incentive effects: if lenders cannot seize collateral in the event of default, borrowers cannot credibly commit to repay their loans. This increases lending risk and results in higher interest rates and reduced access to credit. This adverse effect is considered to be particularly strong for poor borrowers who have less collateral to post and therefore are credit-constrained to begin with. More generally, it is argued that strong enforcement enlarges the effective set of contracts available, allowing a Pareto improvement.

This view has found some support in the data. There is evidence that across countries, weak investor protection is correlated with thinner debt markets La Porta, López-de-Silanes, Shleifer, and Vishny (1997, 1998). Across U.S. states,

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lower borrower liabilities are associated with greater exclusion of poor borrowers from credit markets (Gropp, Scholz, and White (1997)). Visaria's (2009) analysis of microlevel data on loans taken by Indian firms shows that stricter enforcement of lenders rights improves the repayment behavior of delinquent borrowers. She also showed that, on average, interest rates rose but by less for larger loans which are more directly affected by the reform.

With the exception of Gropp, Scholz, and White (1997), most existing evidence comes from analysis of the average borrower, and ignores borrower heterogeneity. In this paper, we analyze how the impact of stronger credit enforcement varies with borrowers' wealth levels. We argue that the traditional theoretical argument for stronger enforcement overlooks its potential general equilibrium effects. The standard theoretical argument is valid when bank credit is infinitely elastic. However, we show that when the supply of credit is inelastic, stronger enforcement of lender rights can increase interest rates and reallocate credit from poor to wealthy borrowers. This provides a rationale for the popular belief that stronger enforcement hurts poor borrowers and benefits lenders and wealthy borrowers, even after ex ante incentive effects are taken into account. In practice, the effects depend on the elasticity of credit supply in the economy being studied, which is an empirical parameter. Our model shows that the macroeconomic and efficiency impacts of strengthening contract enforcement are also ambiguous in general and, again, depend on the supply elasticity of credit.

There are good reasons to believe that the supply of bank credit may be less than perfectly elastic. As a sizeable literature emphasizes, information asymmetries cause lenders to rely on expertise, local knowledge, and long-term relationships with borrowers to effectively screen and monitor borrowers (Fisman, Paravisini, and Vig (2010), Gordon and Bovenberg (1996), James and Smith (2000), Petersen and Rajan (1995)). It follows that to expand their lending operations, banks must employ more loan officers, develop expertise and relationships, and invest in bank infrastructure. Banks also need to reorganize their operations as the relative profitability of serving different client bases changes. All of this takes time and involves significant adjustment costs. Therefore, at least in the short run, credit supply cannot be expanded quickly at a constant marginal cost. In the intermediate or long term, the supply curve is likely to be more elastic. However it may still fail to be perfectly elastic if there is an underlying scarcity of lending personnel with the requisite skills and local information.

Inelastic credit supply generates a general equilibrium effect that can be explained as follows (Lilienfeld-Toal and Mookherjee (2008)).[2] Consider a re-

[2] Lilienfeld-Toal and Mookherjee (2008)'s work on personal bankruptcy law reform provides further details about the microfoundation of the Walrasian model we use here. In particular, they showed that the model characterizes stable contract allocations in a market where lenders and borrowers are matched. Note, however, that reform of bankruptcy exemption limits is funda-

form that increases the probability that the lender will seize a defaulting borrower's secured assets. This improves the credit-worthiness of borrowers and shifts outward the incentive-constrained aggregate demand function for credit. We call this the partial equilibrium (PE) effect. Note that each firm's PE effect depends on its assets; the outward shift of a firm's demand function is directly proportional to its secured assets, so wealthier borrowers experience a larger PE effect. However, because the supply of credit is inelastic, not all of this increased demand can be met at the current interest rate. The result is an increase in the interest rate and in profits earned by lenders. This general equilibrium (GE) effect has a negative effect on the volume of credit. We thus have two opposing effects: a PE effect that increases credit access proportionately more for large borrowers and a GE effect that decreases credit access for all borrowers uniformly. For wealthier borrowers, the larger PE effects can overwhelm the GE effect and cause a net increase in their access to credit. For the poorest borrowers, the GE effect can overwhelm the relatively small PE effect and cause lower access to new credit. As a result, if the supply of credit is sufficiently inelastic, the reform in enforcement may redistribute credit. In practice, the redistribution may be progressive or regressive, depending on parameter values and the size distribution of firms. Thus it is ultimately an empirical matter.[3]

To investigate this issue empirically, we analyze the distributive effect of the Indian legal reform studied previously in Visaria (2009). In the 1990s, the Indian government set up new specialized institutions called debt recovery tribunals (DRTs) to reduce delays in debt recovery suits and strengthen the rights of lenders to recover the assets of defaulting borrowers. Plausibly exogenous interruptions during the roll-out process caused the DRTs to be established at different times in different states, allowing us to exploit state–time variation to identify the effect of the reform. We show that the reform had regressive effects: it caused new long-term borrowing and fixed assets to increase for large borrowers, but decrease for small borrowers. This is consistent with the general equilibrium effects in our theoretical model. We also find a significant rise in interest rates for all categories of borrowers that, as we discuss later, allows us to discriminate between our hypothesis and alternative explanations.

mentally different from enforcement reform: lower exemption limits increase borrower liability equally for all borrowers, whereas stronger enforcement increases liability disproportionately for wealthier borrowers. Therefore, lower exemption limits increase relative credit access for poor borrowers, while stronger enforcement à la debt recovery tribunals reduces it. The model we discuss here is also simpler because it abstracts from ex ante moral hazard for borrowers.

[3]As we show in Section 2, the effect could be progressive if the firm size distribution has relatively few small firms and a significant number of mid-sized credit-constrained firms as well as large firms not subject to credit constraints. In that case, the largest firms borrow less due to higher interest rates, whereas the mid-sized firms borrow more because their credit constraints are relaxed.

The empirical analysis primarily relies on a firm-level panel data set called Prowess, that is collected and distributed by the Centre for Monitoring the Indian Economy (CMIE). This data set contains detailed information on both financial and real variables for all publicly listed Indian firms as well as a large proportion of other large registered firms in the Indian economy. This allows us to examine the effect of the reform on firms' borrowing, fixed assets, profits, and wage bill. Consistent with the general equilibrium effects we model, we find that the reform was associated with reduced borrowing for the firms with the smallest tangible assets and increased borrowing for those with the largest tangible assets. We find parallel effects on plants and machinery. Effects on profits are similar, but these estimates are not always statistically significant. We also find a significant increase in interest rates on new borrowing for all categories of borrowers. All of these results are robust to controls for borrower-specific heterogeneity, changing trends, and time effects that vary by state or by size class of the firm, as well as controls for state-specific policy toward small enterprises.

We find corroborating evidence from the credit supply side using an independent data set distributed by the Indian central bank. In line with our prediction that the reform increased the demand for bank credit, we find that DRTs caused an increase in state-level bank lending. Also, consistent with the idea that DRTs increased the relative profitability of lending to firms with more tangible assets, we find a reallocation of lending away from rural areas, where consumers and farmers would be primary borrowers, toward urban and metropolitan areas, where most businesses would borrow. This shift to lending more to businesses would have necessitated reallocating loan officers and bank business into existing urban and metropolitan branches, and a correspondingly slower branch expansion in suburban and rural areas. In other words, banks would tend to grow more on the "intensive" margin (business per branch, especially urban branches) and less on the "extensive" margin (number of branches). We find evidence that the growth of bank branches, particularly rural branches, slowed down after DRTs were set up. Consistent with the hypothesis of an inelastic supply curve, profits rose significantly in response to DRTs for banks with greater presence in urban and metropolitan areas in the early 1990s.

We also find evidence consistent with adjustment costs in expanding and reorganizing lending operations; credit expansion became larger over time after DRTs were established. It is possible that over a long enough horizon, banks would be able to train their staff, gather local knowledge, and develop business relationships with new borrowers, and the supply curve of credit would become elastic. Thus the GE effect might be mitigated over time. To examine this question, we examine how the effect of DRTs varies as time since DRT establishment increases. We can differentiate the impact 1 year, 2 years, 3 years, and longer after DRTs were set up. We find that interest rates rose the year immediately following a DRT and this increase was moderated the year after.

However, it continued to be positive and significant even 3 years or more later. The same is true for the effects on credit for firms in the first quartile, for whom the GE effect dominates. Thus we find no evidence that the supply of credit became fully elastic within 3 years after the reform. We note, however, that with a long enough time horizon, these bottlenecks to credit expansion might be resolved and our empirical results may not extend.

Next, we examine competing explanations for these distributional effects. First, consider the argument that a short-run shortage of trained personnel might have led to the distributive effects in lending. If lending to large firms requires lower staff time per rupee of loan, then banks might respond to this shortage by substituting supply of credit away from small firms and toward large firms. While it is certainly possible that such substitution occurred, it does not predict that interest rates increase. For this reason we do not think it can be an alternative explanation for our findings.

Second, we consider the argument that the general equilibrium effects arise not in the credit market, but in another input market, such as labor. In this argument, DRTs would increase firms' production levels and demand for labor, and therefore increase the wage rate. If small firms were more labor intensive, then there would be a distributive effect on firm output and profits. We reject this argument for two reasons: because we find no evidence for a wage rate increase in the data and because this labor-market mechanism cannot explain why borrowing contracted for small firms.

Finally we reject an alternative explanation for our results based on the idea that stronger lender rights reduce the insurance value of default (Bolton and Rosenthal (2002), Gropp, Scholz, and White (1997), Perri (2008), Vig (2007)). If, contrary to our model, firms wrote incomplete credit contracts with banks, then their interest payments would not be state-contingent. This would induce small borrowers to default when their projects failed, so as to limit their losses. When a reform such as the DRTs made default more costly, these small borrowers would have lowered their ex ante demand for credit because of aversion either to risk or to deadweight default costs.[4] However, this argument implies that stronger enforcement reduced the risk borne by lenders, which should have caused interest rates to decrease. In contrast, as noted above, interest rates rose after DRTs were established.[5]

We therefore conclude that the hypothesis of GE effects operating through the credit market provides a parsimonious explanation of our empirical results, unlike hypotheses based on GE effects operating through other factor markets

[4]If loan contracts were state-contingent, insurance could be provided directly by lowering interest repayment obligations in adverse states of the world. If so, borrowers would not lower their demand for credit ex ante in response to stronger lender rights.

[5]To be sure, this is not evidence against the incomplete contracting approach per se, but rather against the particular version described by earlier authors that did not include any general equilibrium effects. We cannot reject a version that incorporates both incomplete contracts and general equilibrium effects in the credit market.

or explanations that rely on the insurance value to borrowers of weak lender rights.

The paper proceeds as follows. Section 2 develops the theoretical model. Section 3 describes the judicial reform that we study. Section 4 describes the data that we employ and is followed in Section 5 by the empirical specifications. Section 6 presents the main empirical results. In Section 7, we provide empirical evidence that allows us to differentiate between competing alternative explanations for our results. Finally, Section 8 concludes.

2. THE MODEL

Our approach is based on the theoretical model for competitive credit markets developed in Lilienfeld-Toal and Mookherjee (2008), and differs from conventional models. We treat the price of credit as the rate of profit earned by lenders, rather than the interest rate. We model the "demand" for credit as the solution to an optimal contracting problem, where the expected utility of borrowers is maximized—subject to repayment incentive constraints—and a participation constraint that ensures lenders a rate of profit equal to the going rate, which all agents in the market take as given. The convenience of this formulation is that a change in enforcement institutions does not shift the supply curve of credit: corresponding changes in incentive constraints shift the effective demand curve in a manner that can be derived by performing comparative statics of the optimal contracting problem. Hence changes in default risk faced by lenders appear on the demand rather than supply side in our analysis, in contrast to conventional models in which they appear on both demand and supply sides.

Consider an economy populated by risk-neutral borrowers, differentiated by (collaterizable) fixed assets W, distributed according to cumulative distribution function (c.d.f.) G over support $[\underline{\Omega}, \overline{\Omega}]$. Each borrower seeks to invest in a project of size $\gamma \geq 0$. This requires up-front investments of $\gamma \cdot I$. The project generates returns of $y \cdot f(\gamma)$, where $y \in \{y_s, y_f\}$ is a borrower-specific productivity shock and f is an increasing, continuously differentiable, S-shaped function with $\frac{f(\gamma)}{\gamma}$ rising until $\gamma = b$ and falling thereafter, for some $b \geq 0$. Hence $f'(\gamma)$ is rising over some initial range $(0, b')$ and falling thereafter, where $b' < b$. We assume the borrower does not have any liquid wealth to pay for the up-front investments. In contrast to Lilienfeld-Toal and Mookherjee (2008), we simplify by abstracting from project moral hazard: the probability of success ($y = y_s$) is given and denoted e. It is useful to introduce

$$\bar{y} \equiv e \cdot y_s + (1 - e) \cdot y_f. \tag{2.1}$$

2.1. *Credit Contracts*

A loan contract stipulates the amount borrowed ($\gamma \cdot I$), and the amount T_k to be repaid in state $k \in \{s, f\}$. For simplicity, the realization of the state is costlessly verifiable.[6] We assume *contracts are complete* (CC) in the sense that the repayment obligation T_k can vary with the state $k \in \{s, f\}$. One can think of the payment T_s as corresponding to the stated interest rate that the borrower is expected to repay in the event of success. In the event of failure (state f), the borrower defaults on the obligation and this is followed by a mutually agreed upon adjustment of the borrower's repayment in accordance with his/her ability to pay. The two parties can anticipate in advance what this adjustment will be.

Each borrower has the option of not honoring the loan agreement ex post. For simplicity, we suppose that the borrower either decides to repay the entire interest obligation or none of it.[7] Should the borrower default, lenders can take the borrower to court, and thereafter expect to seize a fraction (θ) of ex post assets owned by the borrower. Ex post assets equal $W + \nu \cdot y_k \cdot f(\gamma)$, where $1 - \nu$ is the fraction of the firm's returns diverted by the entrepreneur. We treat ν as a parameter and assume that it is small; in particular, that it satisfies the condition $\nu < I/(\bar{y} \cdot \theta \cdot f'(b'))$. This limits the extent to which the returns from the project itself can serve as collateral; the borrower's assets remain the primary source of collateral.

This formulation also assumes for simplicity that the assets financed by the loan cannot be seized by the lender: for instance, the loan finances working rather than fixed capital. This is inessential; later we consider an extension where the loan finances the purchase of fixed assets, a fraction of whose value can also be appropriated by the lender if they go to court.

The enforcement institution is represented by θ, incorporating delays and/or uncertainties in the legal process. Enforcement is affected by judicial reforms such as debt recovery tribunals. The main focus is thus on the effects of raising θ.

Should the entrepreneur honor the loan agreement, he obtains ex post utility $W + y_k \cdot f(\gamma) - T_k$ in state $k \in \{s, f\}$. In contrast, utility in case of disagreement in state $k \in \{s, f\}$ is given as

$$(1 - \theta) \cdot [W + \nu \cdot y_k \cdot f(\gamma)] + (1 - \nu) \cdot y_k \cdot f(\gamma) - d, \tag{2.2}$$

[6]The model can be extended to incorporate costs incurred by lenders in verifying the state ex post. Indeed, it is possible that DRTs lowered the costs of state verification, which would also generate partial and general equilibrium effects analogous to those studied here.

[7]The extent of default depends on how penalties are graduated with the size of the default. If they are linear in the extent of default, the optimal extent of default will not be interior: it will be either zero or total. If instead the penalty is convex in the size of the default, partial default may be optimal. Our model can be extended to incorporate this, but the qualitative features would be unaffected. Increasing enforcement would lower the optimal extent of default ex post, thus raising borrower credibility ex ante, which would enable borrowers to borrow more if they are credit-constrained. This would generate PE and GE effects that operate in opposing directions, and the greater the assets that the borrower can pledge as collateral, the larger the PE effect would be.

where d is an additional deadweight loss incurred by the borrower (for example, reputation loss or legal costs).

We assume d is fixed and independent of W. The reader can check that the theory extends straightforwardly to allow d to increase in W: for example, $d = \underline{d} + \delta \cdot W$ for some $\delta > 0$ and $\underline{d} > 0$. (Indeed, it is possible that DRTs raise both $\underline{d}$ and δ, the reputational costs of default. Raising δ is similar to raising θ.) What is important is the assumption that there is a component of reputational cost that applies also to firms with zero assets (i.e., $\underline{d} > 0$). This is a natural assumption to make, since this is what allows new firms to enter and raise credit despite having no assets.

The borrower will honor the agreement in state k if and only if[8]

$$T_k \leq \theta[W + \nu \cdot y_k f(\gamma)] + d. \tag{2.3}$$

It is a standard result that with complete contracting, the loan agreement will always be honored, so the parties never actually go to court. This is because if they do, a Pareto-improving outcome can be generated with a revised loan agreement which lowers the repayment obligation in the failure state, so that the borrower is provided the incentive to honor the agreement. This avoids the deadweight losses associated with going to court. Hence the parties do not go to court on the equilibrium path. The enforcement institution affects the actual contract by determining the ex post outside option of the borrower, which affects the incentive constraints.[9]

2.2. *Supply*

We consider a "competitive" supply of loans, represented by an upward sloping supply curve $L_s(\pi)$ of loanable funds, where π denotes the lender's expected return per rupee loaned.[10] We assume that for there to be some supply

[8]Here we are abstracting from liquidity constraints that may prevent the borrower from repaying. To ensure that the borrower can make payments, we need the condition $T_k \leq W + y_k f(\gamma)$, that is, the ex post return from the project is sufficient to cover the repayment amount. This constraint will not bind once condition (2.3) holds, provided $d < (1-\theta)W + (1-\theta\nu)y_k f(\gamma)$, that is, if d is not too large relative to W. For sufficiently small W, however, the liquidity constraint may bind. In that case, condition (2.3) will not bind and increasing θ will not affect repayments. This complicates the theory slightly, but strengthens our main conclusion: a range of small firms may experience no PE effect at all, since for them the liquidity constraint rather than the incentive constraint will bind.

[9]The model could be modified to allow some asymmetric information or costs of state verification so that with some probability the parties will actually go to court and incur costs of state verification. The current model can be viewed as a limiting case of such a setting where the extent of asymmetric information or costs of state verification are vanishingly small.

[10]Consider the following microfoundation for the supply function of credit. A given lender incurs a loan monitoring (screening/collection) cost of c per rupee loaned, which has to be subtracted from the gross rate of return π on loans to obtain the net profit. Each lender is capacity

of credit, lenders must be assured a return that is at least as large as a nonnegative lower bound α, that is, $L_s = 0$ if $\pi < \alpha$ and $L_s > 0$ if $\pi > \alpha$. To avoid a vacuous analysis, assume that $\bar{y} \cdot f(b)/b > I(1+\alpha)$, that is, some projects will be funded in the absence of any enforcement problems.

The elasticity of this supply function plays a key role. We treat this as an empirical matter. According to one view, globalized financial markets guarantee an infinitely elastic supply of capital to any given economy, in which case $L_s = \infty$ for $\pi \geq \alpha$. In that case, the profit rate will be pegged at α always. We refer to this case as involving *no GE effects*. An alternative view emphasizes that financial intermediaries need local knowledge to monitor loans and argues that this local knowledge is in limited supply. In that case, financial markets are not perfectly integrated and the supply curve $L_s(\pi)$ has a finite elasticity. A limiting case of this is when the supply curve is perfectly inelastic: $L_s = \bar{L}$ for any $\pi \geq \alpha$. In either of these cases, the equilibrium profit rate π will be endogenously determined.

2.3. *Demand*

As a benchmark, we start with the *first-best* demand $\gamma^F(\pi)$, which solves

$$\max_{\gamma}[\bar{y}f(\gamma) - \gamma I(1+\pi)] \tag{2.4}$$

with $\bar{y} \equiv ey_s + (1-e)y_f$.

However, the *first-best* is not always implementable due to the no-default incentive constraint. The relevant demand thus takes these constraints into account:

DEFINITION 1: In a π-incentive compatible loan contract, a borrower with assets W demands credit $\gamma(W, \theta, \pi)$, which solves

$$\max_{\gamma, T_s, T_f} e[y_s f(\gamma) + W - T_s] + (1-e)[y_f f(\gamma) + W - T_f] \tag{2.5}$$

subject to

$$T_k \leq \theta[W + \nu y_k f(\gamma)] + d, k = s, f \tag{2.6}$$

and

$$eT_s + (1-e)T_f \geq \gamma I(1+\pi). \tag{2.7}$$

constrained and a lender with monitoring cost c has capacity to lend up to $L(c)$. Monitoring costs are distributed according to a given distribution $H(\cdot)$ over c. Hence, if the going rate of return on loans is π, lenders are only willing to lend if $c \leq \pi$. As a result, $L_s(\pi) \equiv \int_0^{\pi} L(c)\, dH(c)$.

Aggregate incentive compatible demand for credit is then given as $Ł_d(\theta, \pi) = \int \gamma(W, \theta, \pi)\, d\mu(W)$, where $\mu(W)$ denotes the distribution of W in the population of firms.

If we add up constraints (2.6) and (2.7), it becomes clear that a project size γ is implementable if and only if

$$\theta[W + \nu\bar{y}f(\gamma)] + d \geq \gamma I(1 + \pi). \tag{2.8}$$

Condition (2.8) reduces to the existence of a credit ceiling. To see this, note that it can be rewritten as

$$\theta \cdot W + d \geq \gamma I(1 + \pi) - \theta \cdot \nu\bar{y}f(\gamma). \tag{2.9}$$

The assumption that $\nu < I/(\bar{y} \cdot \theta \cdot f'(b'))$ implies that the right-hand side of equation (2.9) is increasing in project size γ. In other words, since the returns on the project do not serve as a substantial source of collateral (due to the low value of ν), larger project scales are more difficult to implement. A borrower with given wealth W will face a credit ceiling uniquely defined by the value of γ that solves the equality version of (2.9). We denote this project size ceiling by $\gamma^H(W, \theta, \pi)$. It is increasing in W and θ, and decreasing in π.

To characterize the optimal demand for credit, the following definitions are useful:

DEFINITION 2:

First-best asset threshold is $W^F(\pi) \equiv \{\gamma I(1 + \pi) - d\}/\theta - \nu\bar{y}f(\gamma^F)$.

Maximum project size is $\gamma^H(W, \theta, \pi)$, which solves $\theta[W + \nu\bar{y}f(\gamma)] + d = \gamma I(1 + \pi)$.

Minimum project size $\gamma^L(\pi)$ is the smallest solution to $\bar{y} \cdot f(\gamma)/\gamma = I \cdot (1 + \pi)$.

Minimum viable asset threshold $W_L(\pi, \theta)$ solves $\gamma^H(W, \theta, \pi) = \gamma^L(\pi)$.

At a given profit rate π, it is clear that a firm operates and gains access to a loan only if its maximum project size γ^H exceeds the minimum viable project scale γ^L. This translates into a wealth threshold W_L below which borrowers are excluded from the credit market altogether, which we call the minimum viable asset threshold.

Among the borrowers with wealth greater than W_L, those with sufficiently high wealth (we call this the first-best asset threshold, W^F) operate at a scale equal to the first-best scale and are not rationed. The remaining borrowers, who have assets between W_L and W^F, obtain a loan but are rationed with regard to the scale of the loan.

This leads us to the incentive-constrained demand function for loans.

LEMMA 2.1: *The incentive-constrained demand function for credit is*

$$\gamma(W, \pi; \theta) = \begin{cases} 0, & \text{if } W < W_L(\pi, \theta), \\ \gamma^H(W, \theta, \pi), & \text{if } W_L(\pi, \theta) < W < W^F(\pi), \\ \gamma^F(\pi), & \text{if } W > W^F(\pi). \end{cases} \tag{2.10}$$

2.4. *Market Equilibrium*

Next, we solve for the market equilibrium so as to determine the equilibrium profit rate. We consider a competitive market for loan contracts and use a standard Walrasian equilibrium notion, where the profit rate is determined by the equality of aggregated supply and incentive-constrained demand.

DEFINITION 3: An incentive-constrained Walrasian allocation is a credit allocation in which each borrower receives his incentive-constrained demand corresponding to a profit rate π^* that has the property that the supply of loans at π^* equals incentive-constrained demand at π^* aggregating across all borrowers.

Along the lines of Lilienfeld-Toal and Mookherjee (2008), it can be shown that Walrasian allocations characterize stable allocations of a matching market between borrowers and lenders under suitable assumptions on the distribution of lenders.[11]

Since market demand changes with θ, the equilibrium profit rate π^* is a function of θ and is denoted by $\pi(\theta)$ where required.

2.5. *Effects of Increasing θ With No GE Effects*

First, consider the case where the loan supply function is perfectly elastic. Then the equilibrium profit rate is fixed at α and the equilibrium credit allocation is given by borrower demands evaluated at the profit rate α.

In this case, the effect of raising θ is straightforward, as can be seen in Figure 1. When θ increases, incentive constraints are relaxed, which permits an expansion of credit ceilings for every borrower. The proportion of firms excluded from the market must fall, since the minimum project size does not change with θ. Borrowers who were previously credit-constrained obtain larger loans and thus attain higher payoffs. Those who were not constrained are unaffected. Lenders are unaffected as well. The result is a Pareto improvement. The distributional impact is favorable, since poorer borrowers gain access to credit.

[11]Specifically, a sufficient condition is the *competitive supply assumption*, which states that for any lender with cost c and lending capacity $L(c)$, there exist other borrowers with cost at or below c with aggregate lending capacity of at least $L(c)$. For example, suppose there exist at least two lenders of any given "type." Then Bertrand-like competition among lenders causes the gross rate of return π on lending to be equal across all active lenders.

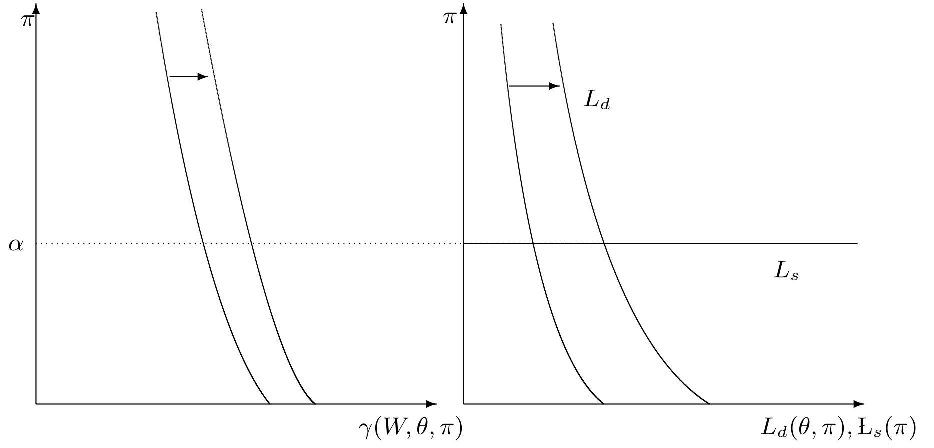


FIGURE 1.—Impact of strengthening enforcement when there are no GE effects.

Borrowers are better off because every contract implementable under weak enforcement is also implementable under strong enforcement.[12] This justifies the conventional intuition that stronger enforcement institutions are uniformly beneficial for borrowers.

2.6. *Effects of Increasing θ With GE Effects*

Now consider the case where the supply of funds is inelastic to some degree. An increase in θ shifts the aggregate credit demand function outward and thus raises the equilibrium profit rate. This GE effect will choke off some demand so as to clear the credit market. The total effect on credit allocation is now composed of a PE effect as well as a GE effect. The PE effect relaxes credit ceilings at any given profit rate, but the GE effect raises the profit rate, which shrinks credit ceilings, raises the minimum viable project scale, and lowers the first-best project scale.

2.6.1. *Nearly Perfect Elasticity of Credit Supply*

As a first step, we consider the case where the supply of funds is nearly perfectly elastic, so the GE effect is sufficiently weak.

[12]This is a fairly general result. In particular, this result holds with costly state verification, where T_k cannot be conditioned on k because the state k is costly to verify. This result has the logic of a mechanism design problem, where a higher θ relaxes incentive constraints. However, the result does not hold if contracts are incomplete and payments cannot vary for exogenous reasons.

PROPOSITION 2.2: *Consider an increase in θ from $\underline{\theta}$ to $\overline{\theta} > \underline{\theta}$. Suppose the elasticity of the credit supply function at any $\pi > \alpha$ is finite but bounded below by some $\underline{\varepsilon}$. If $\underline{\varepsilon}$ is sufficiently large, there are three scenarios*:

(i) *The proportion of firms excluded from the market falls* (*i.e.*, *the minimum asset threshold W_L falls*).

(ii) *The first-best project scale* (*and hence credit allocated to sufficiently wealthy borrowers*) *falls*.

(iii) *For borrowers with intermediate asset sizes*, *the credit allocated rises*.

To see the argument behind this result, note that the increase in the equilibrium profit rate can be made arbitrarily small if $\underline{\varepsilon}$ is sufficiently large.[13] A sufficiently small rise in the profit rate implies that the project ceiling γ^H will rise (by at least a certain amount) for all borrowers due to the rise in θ, while the rise in the minimum viable project scale γ^L will be small. Hence the expansion of the credit ceiling (for borrowers near the minimum asset threshold W_L) outweighs the increase in the minimum viable project scale, thus reducing exclusion and increasing the credit ceiling for all active borrowers. However, the first-best project size declines due to the rise in the profit rate.

Although the effects of changing θ with nearly perfectly elastic supply of loans is similar to the case where GE effects are totally absent, there are some important differences. There is a distributional shift of credit in favor of poorer borrowers and away from wealthy borrowers. The effect is not a Pareto improvement: the wealthiest borrowers are worse off due to the rise in the profit rate. On the other hand, the borrowers at the bottom end of the asset distribution who gain access to the market are made better off. For intermediate-sized borrowers, the effects are ambiguous. On the one hand, their credit limits are relaxed and so they can expand the scale of their projects, but on the other hand, they pay higher interest rates.

2.6.2. *Perfectly Inelastic Credit Supply*

Now turn to the other extreme where the supply of funds is perfectly inelastic. To see the results most clearly, focus on the case where $\nu = 0$, where only the borrowers' initial assets serve as collateral. More generally with $\nu > 0$ but small, it is easy to verify that the cross-partial of γ^H with respect to θ and W is positive. This single-crossing property drives our main result, as it implies that the PE effect of increasing θ is increasing in W. Note also that the cross-partial of γ^H with respect to ν and W is positive if ν is small. Hence if DRTs raise ν apart from θ, we obtain the same conclusion. On the other hand, the cross-partial of γ^H with respect to d and W is negative, so the theory does not accommodate the possibility that DRTs raise reputational fixed cost d. However, if we model reputation costs as increasing linearly in W at the rate δ,

[13]Specifically, $\pi(\overline{\theta}) > \pi(\underline{\theta}) > \alpha$. As $\underline{\varepsilon}$ tends to ∞, $\pi(\overline{\theta})$ tends to α and so $\pi(\overline{\theta}) - \pi(\underline{\theta})$ tends to 0.

then the cross-partial of γ^H with respect to δ and W is positive. So our results extend as long as DRTs raise reputation costs for large firms relative to small firms.

Also we assume that the upper bound of the wealth distribution is low enough that no borrower attains the first-best project scale. Then the project ceiling for a borrower with wealth W is

$$\gamma^H(W, \pi; \theta) = \frac{\theta W + d}{I(1+\pi)}. \tag{2.11}$$

Suppose θ rises to θ' and suppose the corresponding equilibrium profit rate rises from π to π'. Then note that if the project ceiling does not fall for some borrower with wealth W,

$$\Delta(W) \equiv \gamma^H(W; \pi'; \theta') - \gamma^H(W; \pi; \theta) \geq 0, \tag{2.12}$$

then it must rise (and is larger) for all higher wealth borrowers with higher wealth $W' > W$ (i.e., $\Delta(W') > \Delta(W) \geq 0$).[14]

Next, the proportion of borrowers that are excluded must rise. To see this, suppose not. In other words, suppose W_L remains constant or falls. Since we know that the minimum viable scale γ^L has risen, the borrower at the previous minimum threshold W_L must have experienced a rise in the project ceiling. This implies that all borrowers must experience a rise in their ceilings. Since (by assumption) there is no borrower wealthy enough to achieve the first-best scale, the credit allocated to every active borrower must have risen. This is not possible in equilibrium since the total supply of funds available is fixed.

Hence there must be a rise in the incidence of exclusion at the bottom end of the asset distribution and those borrowers must be worse off. Since the aggregate supply of funds is fixed, there must exist wealthier borrowers who receive a larger supply of funds. Indeed, the argument above shows that there must exist a cutoff wealth level $\widehat{W}$ such that the credit level of borrowers with that wealth level is unaffected. Credit expands for borrowers with wealth above $\widehat{W}$ and contracts for all others. Thus, there must be a regressive redistribution of credit across borrowers.

Now turn to the interest rate. It is natural to define the interest rate to be the rate that the firm is obliged to pay as per the contract, which it does pay in the successful state. But if the state is not successful, the actual amount paid is adjusted downward to reflect the borrower's diminished capacity to repay. Using the facts that incentive constraints are binding in both states and that

[14]This follows since $\Delta(W) = W[\frac{\theta'}{I(1+\pi')} - \frac{\theta}{I(1+\pi)}] + d[\frac{1}{I(1+\pi')} - \frac{1}{I(1+\pi)}]$. Since $\pi' > \pi$, we have $d[\frac{1}{I(1+\pi')} - \frac{1}{I(1+\pi)}] < 0$. So $\Delta(W) \geq 0$ implies $\frac{\theta'}{I(1+\pi')} - \frac{\theta}{I(1+\pi)} > 0$, and then the result follows.

lenders have to be paid π in expectation, it is easily checked that the interest rate so defined can be expressed as

$$r = \pi + \theta \frac{\nu}{I}(y_s - \bar{y}) \frac{f(\gamma)}{\gamma}. \tag{2.13}$$

Note that with $\nu = 0$, the interest rate does not vary across firms. Neither do interest payments vary with the state of the world.[15] More realistically, with $\nu > 0$, firms with a higher average rate of return to capital assets $\frac{f(\gamma)}{\gamma}$ are charged a higher interest rate. In that case, interest rates vary across borrowers and interest payments vary across states of nature for any given borrower.[16] However, if ν is small, as assumed by the model, the interest rate does not vary much across firms of varying size.

With a strong enough GE effect, the establishment of DRTs raises π, lowers the scale γ for small firms, and raises it for large firms. If $\gamma > b'$, then the production function is concave over the relevant range, and (2.13) implies that interest rates rise for small firms, but the effect for large firms is ambiguous. The same result follows if DRTs raise ν by making it more difficult for entrepreneurs to strip assets when they default on loans.

We summarize the preceding discussion as follows.

PROPOSITION 2.3: *Suppose the upper bound of the wealth distribution $\overline{\Omega}$ is less than $W(\pi(1))$, so all firms are credit-constrained. In addition suppose that $\nu = 0$ and supply is perfectly inelastic. If θ increases, the profit rate, the interest rate, and the proportion of borrowers excluded rises. Moreover, there exists threshold asset size $\widehat{W}$ such that the following scenarios exist*:

(a) *If $W < \widehat{W}$, credit falls and the borrower is worse off.*

(b) *If $W > \widehat{W}$, credit rises.*

Results (a) *and* (b) *also obtain when ν is positive but small enough, the supply curve is upward sloping, and the production function is concave* (*i.e.*, $b' = 0$) *or almost concave in the sense that b' is small enough that no firms are excluded from the market. In this case, the interest rate rises for borrowers with $W < \widehat{W}$.*

[15] The same is true if we extend the theory to suppose that lenders can recover part of the fixed assets financed. Let us suppose that with $\nu = 0$, the lender can expect to extract $\theta \cdot [W + (1 - \delta)\gamma]$, where δ is a rate of depreciation (or stripping) of capital assets. Then the repayment amount does not vary with the state $T_s = T_f = T$, and the interest factor is equal to $1 + r = \frac{T}{\gamma \cdot I} = 1 + \pi$ for all firms.

[16] Intuitively, the borrower is supposed to pay back an average interest rate of π to lenders. In the successful state, the borrower is able to pay back more than π. The excess paid back above π has to cover the shortfall below π expected in the unsuccessful state. In other words, the interest rate includes an allowance for "default" risk, which is proportional to the average return to capital. If the production function is concave over the relevant range, larger firms earn a lower average rate of return. We therefore expect to see them obtain loans with a lower interest rate.

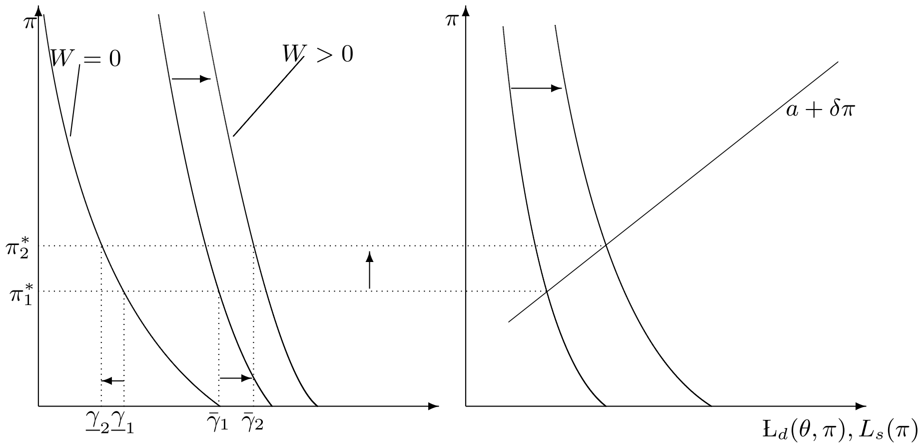
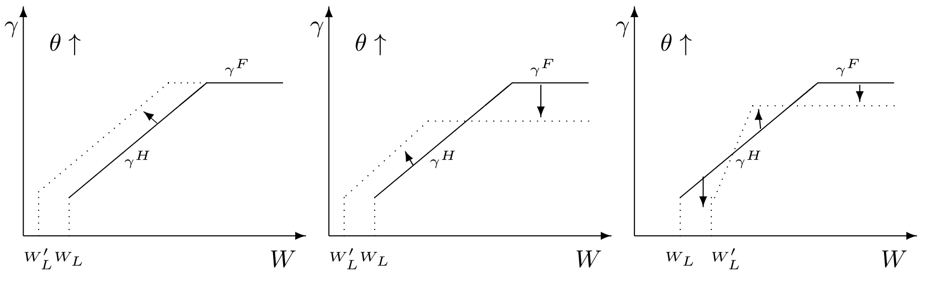


FIGURE 2.—Impact of strengthening enforcement with GE effects.

In the cases covered by this proposition, we find a regressive redistribution of credit among the set of credit-constrained borrowers. The intuition underlying this result is depicted in Figure 2. The left panel shows individual firm demand functions for two different kinds of firms: one with the $W = 0$ and one with $W > 0$. For a firm with zero assets, an increase in θ does not change in the demand for credit. For a firm with $W > 0$, individual demand shifts outward.

In the right panel we see that the increase in θ causes the aggregate demand for credit to shift outward. Since the supply of credit is inelastic, this increases the equilibrium profit rate π faced by all firms. For the firm with $W = 0$, there has been no shift in the credit demand curve, but there will be an upward movement along the demand curve because it faces the higher π that its lenders must earn. As a result, its demand for credit decreases from $\underline{\gamma}_1$ to $\underline{\gamma}_2$. The firm with $W > 0$ also faces a higher interest rate which potentially reduces the quantity of credit demanded, but this movement will be along the new demand curve. The outward shift of the demand curve dominates the effect of the increased profit rate, so demand increases from $\overline{\gamma}_1$ to $\overline{\gamma}_2$. In summary, small firms receive less credit and large firms receive more credit due to the change in θ.

More generally, the distributive effects can take different forms depending on the size of the GE effects and the firm size distribution, as shown in Figure 3. Each panel represents a GE effect of different size: panel A corresponds to elastic credit supply that generates no GE effects, panel B corresponds to a relatively inelastic supply that creates a weak GE effect, and panel C corresponds to a highly inelastic supply that creates a strong GE effect. The horizontal axis of each graph represents firm wealth; we allow here for a wide enough support of the wealth distribution that the largest firms are not credit-constrained. The vertical axis shows the corresponding γ, which captures the scale of the project or the amount of borrowing.



A: No GE-effect B: Weak GE effect C: Strong GE effect

FIGURE 3.—Impact of strengthening enforcement with GE effects.

In the absence of any GE effects (panel A), the stronger credit enforcement shifts the π-incentive compatible demand out for all borrowers who operate at their credit ceiling. The largest firms are unaffected because they were not credit-constrained to begin with. Some excluded borrowers can now also participate and exclusion is reduced. Hence, we have a Pareto improvement. The profit rate is unaffected, and if $\nu = 0$, then the interest rate remains constant as well. If $\nu > 0$, then firms operating on the concave portion of their production function will be charged a lower interest rate.

With sufficiently small GE effects (panel B), the credit ceiling is shifted out, and exclusion is reduced. As a result, all credit-constrained borrowers receive more credit. The effect on small firms is qualitatively similar to panel A. While the interest rate they pay rises, this is outweighed by the gain in their credit access, so they borrow more and are better off. However, large firms that work near or at the first-best project scale are only barely credit-rationed to start with. As a result, the effect of a higher interest rate outweighs the relaxation of their incentive constraints, and their borrowing falls. Here the reform redistributes credit from large to small firms.

With sufficiently strong GE effects (panel C), the distributive effects are reversed. The rise in interest rate is now large enough to increase exclusion at the bottom of the size distribution. Small firms that are not excluded get less credit, while larger firms get more credit. Firms large enough to not be credit-constrained at all experience lower borrowing and profits, while those that are credit-constrained can end up borrowing and earning more, because their credit constraints are relaxed. The effect is inverse U-shaped in general.

It is important to note that which exact outcome occurs depends on two elements: the strength of the GE effects and the firm-size distribution. If the upper bound to firm size is large enough, there will be firms at the very top that experience a contraction in borrowing. If this is the case, and most firms are in the intermediate-size category in panel C, the enforcement reform will result in a progressive redistribution of credit from the largest firms to mid-sized firms. On the other hand, if the upper bound to firm size happens to be in the intermediate range while the smallest firms are in the lowest range, we obtain the opposite result: small firms experience a contraction, while large firms expand. Hence the distributional impact is ultimately an empirical matter. For this reason we turn to the empirical evidence from the DRT reform in India.

3. THE EMPIRICAL CONTEXT: INDIAN DEBT RECOVERY TRIBUNALS

We test our model's predictions by examining the effects of an Indian judicial reform that strengthened the enforcement of credit contracts. In the wake of the financial sector reforms of the early 1990s, the Indian government introduced a few measures aimed at improving the financial health of commercial banks. The central bank established new rules requiring banks to reduce their non-performing loans. To aid the banks in this process, in 1993 the government of India passed a national law establishing new specialized courts to process debt recovery cases.

This law allowed the national government to establish new debt recovery tribunals (DRTs) across the country, where banks and financial institutions could file suits for claims larger than rupees 1 million (currently \$1 is worth approximately Rs 45; in the early 1990s, it was on the order of Rs 25). Before this law, all debt recovery suits were tried in civil courts, according to the Code for Civil Procedure. It was common for cases to continue for extremely long periods; for example, nearly 40 percent of the pending debt recovery cases in civil courts in 1985–1986 had been pending for longer than 8 years (Government of India (1988)). In contrast, DRTs streamlined procedures that allow cases to move through the process more quickly. Defendants are required to respond to summonses sooner, provide written defenses, and make all counterclaims against the bank at the first hearing. DRTs can also issue interim orders to prevent defendants from disposing off their assets before the case is closed and, in some circumstances, may also issue a warrant for the defendant's arrest. Substantive laws governing the cases remain the same; lawyers use the same arguments and precedents to plead and defend their cases, in both civil courts and debt recovery tribunals.

There is evidence to suggest that DRTs have been effective at lowering case processing times. In a small random sample of law suits of a single bank, Visaria (2009) found that cases that were processed in DRTs took significantly less time to pass through the various stages of the process and were just as likely to be resolved in favor of the bank as civil court cases. This suggests that DRTs increased the (present discounted) value of the amount recovered by banks from defaulting loans. Our analysis also shows that there were no significant differences in this improvement for small versus large claims.[17]

[17]This evidence is presented in Table XIV, and is discussed in greater detail in Section 7.

Therefore, we interpret the introduction of a DRT in a state as a uniform increase in the parameter θ for all borrowers in that state.

The DRT law allowed the national government to establish tribunals across the entire country and to determine their territorial jurisdiction; state governments were not given any formal authority to influence this process. In fact, DRTs began to be set up soon after the law was passed; five states received tribunals in 1994. However, as reported in Visaria (2009), this process was halted by a legal challenge to the law. In 1994, in response to a case filed by the Delhi Bar Association, the Delhi High Court ruled that the DRT law was not valid. It was only in 1996, after the country's Supreme Court issued an interim order in favor of the law, that DRT establishment was resumed. New DRTs were set up in quick succession starting in 1996. By 1999, most Indian states had received a DRT. Table I lists the dates on which DRTs were established in different states.

The events described above suggest that the timing of DRT establishment was driven by reasons plausibly exogenous to firms' borrowing behavior across different states. However, it is possible that state-level factors also influenced this timing. To investigate this possibility, we ran Cox hazard rate regressions of the time to DRT adoption, on state-level economic, judicial, and political variables. The results are presented in Table II. Time-invariant regressors include total bank credit at the state-level during 1990–1992, and the 1990–1992 average of the assets and profits of firms located in the state. Time-varying explanatory variables include level and growth rate of state-level GDP, per capita credit disbursed by commercial banks, and the share of small-scale industries

TABLE I

DATES OF DRT ESTABLISHMENT

City of DRT (1)	Date Estab. (2)	Jurisdiction (3)
Early States		
Kolkata	Apr 27 1994	West Bengal, Andaman & Nicobar Islands
Delhi	Jul 5 1994	Delhi
Jaipur	Aug 30 1994	Rajasthan, Himachal Pradesh, Haryana, Punjab, Chandigarh
Bangalore	Nov 30 1994	Karnataka, Andhra Pradesh
Ahmedabad	Dec 21 1994	Gujarat, Dadra & Nagar Haveli, Daman & Diu
Late States		
Chennai	Nov 4 1996	Tamil Nadu, Kerala, Pondicherry[a]
Guwahati	Jan 7 1997	Assam, Meghalaya, Manipur, Mizoram, Tripura, Arunachal Pradesh, Nagaland[b]
Patna	Jan 24 1997	Bihar, Orissa
Jabalpur	Apr 7 1998	Madhya Pradesh, Uttar Pradesh
Mumbai	Jul 16 1999	Maharashtra, Goa

[a]The Chennai DRT's jurisdiction was expanded to include Lakshadweep on Dec 5 1997.
[b]The Guwahati DRT's jurisdiction was expanded to include Sikkim on Dec 5 1997.

TABLE II

SURVIVAL ANALYSIS OF DRT ADOPTION[a]

	(1)	(2)	(3)	(4)	(5)	(6)
Not Time Varying						
Bank credit (1990–1992 avg.)	−0.000			−0.000		
	(−0.604)			(−0.939)		
Firm assets (1990–1992 avg.)		−0.384			0.848	
		(−0.392)			(0.649)	
Firm profits (1990–1992 avg.)			−0.261			−0.403
			(−0.942)			(−0.612)
Time Varying						
Growth rate of state GDP				−0.009	−0.027	−0.022
				(−0.167)	(−0.512)	(−0.441)
Per capita credit				0.009	0.002	0.002
				(0.864)	(0.109)	(0.138)
SSI share in total bank credit				2.331	8.824	3.942
				(0.485)	(0.647)	(0.300)
Growth rate of SSI share of bank credit				−0.094	−6.407	−5.273
				(−0.103)	(−0.928)	(−0.851)
Pending high court cases per capita				−0.009	−0.054	−0.072
				(−0.077)	(−0.400)	(−0.544)
Sitting high court judges per capita				−7.621	2000.640	1539.482
				(−0.087)	(1.418)	(1.264)
Congress party & allies				0.048	−0.219	0.305
				(0.049)	(−0.212)	(0.231)
Janata party & allies				0.806	0.334	−0.079
				(0.650)	(0.274)	(−0.053)
Communist party & allies				0.860	1.251	1.153
				(0.701)	(1.042)	(0.971)
Regional parties				0.942	1.146	0.976
				(0.805)	(1.037)	(0.909)
Centre's ally				0.424	−0.530	−0.795
				(0.502)	(−0.479)	(−0.579)
Observations	80	56	56	76	56	56

[a] t statistics are given in parentheses. A Cox proportional hazards model is fitted to the time taken to establish a DRT in a state. As indicated, explanatory variables include the 1990–1992 averages of total bank credit, firm assets, and firm profits in this state, state GDP, its growth rate, per capita total bank credit, the share of small scale industries (SSI) in total bank credit, the growth rate of this share, per capita pending high court cases, number of high court judges per capita, dummies for political party in the state government, and a dummy for whether the political party in state government was allied with the party in the national government. In results not shown, each of these variables is also entered separately, without any significant effects. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

in commercial bank credit in the state as well as its growth rate, cases pending, and number of judges per capita in the state high court, the nature of the dominant political party in the state government, and whether it was an ally of the party in power at the national level. The data cover the period 1993–2000 for 23 states. The results show that none of these state-level observables correlates with the timing of DRT adoption.

However, we may still worry that state-level unobservable factors affecting firm outcomes were correlated with DRT adoption. In particular, there may be different secular trends for small and large firms in states that adopted DRTs, and those may be driving the observed results. To alleviate this concern, we control for state-specific time trends, firm-size-specific time trends, and state–year targets set by the Reserve Bank of India for bank lending to small firms. Our results are robust to these controls. We also allow time trends to vary by industry and show that our results are not explained simply by the secular growth or contraction of particular industries.

4. DATA

The empirical analysis uses a few different data sets.[18] For the main regressions, we use a firm-level panel data set drawn from the Prowess data base constructed by the Centre for Monitoring the Indian Economy (CMIE). This contains firm-level information for all firms listed on India's major stock exchanges, as well as other smaller firms, and is considered to have a high coverage of the organized manufacturing sector in India. We use data for 1683 firms spanning all major industry groups, with the largest concentration in manufacturing. The data base contains detailed information from balance sheets and income statements, total outstanding credit from all sources, and total outstanding bank borrowing from all banks. In addition, it contains detailed information about the firms' production, sales, and input use. When we examine the effects of the DRT reform from the borrowers' side of the market, we exclude companies from the financial sector since they are more likely to be net lenders rather than borrowers. When we examine the effects of the DRT reform from the lenders' side of the market, we use the Prowess data on the banking firms. We also exclude state-owned enterprises that are not subject to commercial norms or incentives.

This data set derived from Prowess differs from the data set that Visaria (2009) used to analyze the effects of the DRT reform. Her data set consists of detailed loan records obtained from a large private bank in India (hereafter referred to as the private bank data set). While the Prowess data base lacks this loan-level detail on repayments and interest rates, it has certain advantages over the private bank data set: The sample of firms is not selected on their

[18]Further detail about the variables can be found in the Data Appendix in the Supplemental Material (Lilienfeld-Toal, Mookherjee, and Visaria (2012)).

borrowing from a particular bank, the data on borrowing cover loans from all sources, not just from a single bank, and there is much more detail on firm performance, allowing us to examine the effects of the reform on several real variables. In addition, we avoid some attrition problems that exist in the private bank data set, where information is unavailable for loan accounts closed before the year 2000. However, the Prowess data set does not have information on loan repayment, so to examine the effect of DRTs on loan repayment, we rely on the private bank data set alone. Finally, some variables of interest are not directly observed in the Prowess data set and must be imputed on the basis of other information in the data set. In particular, the Prowess data base only allows us to compute the interest rate averaged across all loans outstanding in any given year, not the interest rate on new loans alone, which is the variable we seek to focus on, based on our model. The private bank data set reports the exact variable we seek to focus on: interest rates on new individual loans. For this reason, we present results for interest rates using both the Prowess data set and the private bank data set.

Since we are interested in the differential impact of the reform on firms of different sizes, we use the firm's reported tangible assets as a proxy of size.[19] To avoid any simultaneity problems caused by endogenous changes in the firm's assets due to the DRT reform, we classify firm size on the basis of the firm's tangible assets in 1990, four years before the first DRT was established. This allows us to study the impact of DRTs on firms according to their size before the reform was announced. This also restricts us to look only at firms that existed in 1990, so we cannot study the effect of the reform on firm entry. Given that the Prowess data base excludes all firms in the informal sector as well as the very small formal-sector firms, this constraint is perhaps not that restrictive.

Our sample consists of approximately 16,600 observations spanning the period 1992–2003 for 1683 firms. Descriptive statistics are reported in Tables III and IV. All variables are adjusted for inflation, using the all-India, wholesale price index for 2002. Table III provides means, standard deviations and ranges of the main variables used in the analysis for the entire sample, while Table IV breaks this down for different quartiles. The mean 1990 asset size was Rs 260 million (in 2002 prices) with a standard deviation of Rs 737 million. The total stock of outstanding long-term borrowing per year averaged Rs 622 million for the sample period 1993–2002. Fresh long-term borrowing averaged Rs 200 million; this is our main variable of interest. Table III shows substantial growth between 1990 and the sample period in borrowing, plants and machinery, profits, and wage bill, reflecting the increased rate of the Indian economy starting in the mid-1990s.

Table IV shows the breakdown of average values of various variables by quartiles (with firms ordered by the size of their tangible assets in 1990). The 25%,

[19]We use tangible assets since lenders are likely to use these as collateral.

TABLE III

SUMMARY STATISTICS: ALL FIRMS[a]

	All Years		Years With DRT Changes		1990	
	Mean (SD)	Min/Max	Mean (SD)	Min/Max	Mean (SD)	Min/Max
Tang.Ass.	26.0 (73.7)	0.0044/1342.1	26.0 (73.7)	0.0044/1342.1	26.0 (73.7)	0.0044/1342.1
Borr.	20.0 (212.4)	0/15,717.9	14.5 (77.4)	0/2482.0	1.25 (4.38)	0/45.9
TotalLtermb	62.2 (345.8)	−0.0000010/19,420.8	54.6 (246.9)	−0.00000080/9863.7	8.26 (27.2)	0/503.0
PlaMa	109.4 (678.5)	0/45,769	85.3 (365.7)	0/14126	12.0 (40.4)	0/783
Profits	18.6 (132.7)	−1189.6/8243.8	15.9 (73.2)	−224.3/2258.5	2.42 (8.48)	−14.4/162.8
Intrate	10.4 (18.5)	−11.2/476.1	10.8 (17.2)	−11.2/476.1	6.03 (16.9)	−9.30/457.4
Wage bill	13.6 (42.9)	0/1526.2	11.4 (32.8)	0/928.0	2.38 (6.37)	0/156.5
t	1997.2 (3.46)	1992/2003	1996.4 (1.72)	1994/1999	1990 (0)	1990/1990
Observations	16,602		8409		1741	

[a]Standard deviation (SD) is given in parentheses. The variable Tang.Ass. denotes tangible assets measured in 1990. Borr. denotes borrowing and is new long-term borrowing over the last fiscal year. TotalLtermb is the stock of long-term borrowing. Profits are profits before depreciation and tax provisioning, interest rate is defined as interest expenses over total borrowings (minus inflation rate), and the wage bill is total compensation to employees and includes wages and salaries, gratuities, contributions to private pension funds, and so on. The variable PlaMa is plants, machinery, computers, and electrical installations. Finally, t denotes the year of the end of the fiscal year. All variables are measured in rupees (tens of millions) and adjusted by the 2002 wholesale price index.

50%, and 75% quartiles of 1990 tangible assets are Rs 38, 83, and 202 million, respectively, with minimum and maximum values of Rs 40,000 and 13.4 billion, respectively. Evidently the distribution is substantially skewed with a long upper tail. Interest rates did not vary much across different sized firms, averaging roughly 10% for all four quartiles.

Note that the DRT law only applies to debt recovery claims greater than Rs 1 million. Total long-term borrowing of firms in the first quartile averaged Rs 51.9 million at 2002 prices, well above this threshold. Hence most firms in the data set fell under the jurisdiction of DRTs in terms of the volume of their aggregate debt. In Section 7.1, we present evidence that DRTs appear to have been just as effective at improving repayment for small firms as for large firms, and so it is appropriate to consider all our firms as eligible for treatment by the DRT reform.

TABLE IV

SUMMARY STATISTICS BY QUARTILE[a]

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
	Mean (SD)	Min/Max	Mean (SD)	Min/Max	Mean (SD)	Min/Max	Mean (SD)	Min/Max
Tang.Ass.	2.19	0.0044/3.8	5.73	3.81/8.3	12.9	8.34/20.2	85.9	20.2/1342.1
	(0.99)		(1.25)		(3.35)		(137.7)	
Borr.	2.33	0/425.7	3.68	0/153.9	6.81	0/235.6	48.0	0/15,717.9
	(15.8)		(9.70)		(18.5)		(354.9)	
TotalLtermb	5.19	−0.00000042/726.0	13.0	−0.0000010/544.0	28.0	−0.00000017/1168.7	178.7	0/19,420.8
	(22.6)		(34.9)		(70.6)		(629.3)	
PlaMa	8.04	0/413	20.2	0/768	45.6	0/2101	320.7	0/45,769
	(20.9)		(41.9)		(101.5)		(1243.9)	
Profits	1.24	−21.2/185.2	1.92	−274.5/126.4	6.75	−148.7/550.0	56.7	−1189.6/8243.8
	(5.95)		(12.0)		(21.5)		(243.8)	
Intrate	10.8	−11.2/427.7	10.4	−11.2/417.3	9.85	−11.0/436.7	10.8	−11.1/476.1
	(18.0)		(16.8)		(17.2)		(21.1)	
Wage bill	1.64	0/34.5	3.63	0/75.9	7.77	0/213.2	36.6	0/1526.2
	(2.40)		(4.72)		(10.9)		(74.9)	
t	1997.2	1992/2003	1997.2	1992/2003	1997.2	1992/2003	1997.3	1992/2003
	(3.49)		(3.45)		(3.46)		(3.45)	
Observations	3597		4038		4253		4714	

[a]Standard deviation is given in parentheses. The variable Tang.Ass. denotes tangible assets measured in 1990. Borr. denotes borrowing and is new long-term borrowing over the last fiscal year. TotalLtermb is the stock of long-term borrowing. Profits are profits before depreciation and tax provisioning, interest rate is defined as interest expenses over total borrowings (minus inflation rate), and the wage bill is total compensation to employees and includes wages and salaries, gratuities, contributions to private pension funds, and so on. The variable PlaMa is plants, machinery, computers, and electrical installations. Finally, t denotes the year of the end of the fiscal year. All variables are measured in rupees (tens of millions) and adjusted by the 2002 wholesale price index.

5. EMPIRICAL SPECIFICATION

According to the debt recovery tribunal law, a case can be assigned to a DRT located in the region where the defendant resides or where the cause of action arises (Government of India (1988)). Accordingly, we assign firms to DRT jurisdictions on the basis of their registered office addresses. The DRT variable is a categorical variable at the state–year level, which takes value 1 in years when the jurisdiction had a DRT in place.

We now describe the regression specification used and discuss how it relates to the theory developed in Section 2. As we see below, focusing on the case with $\nu = 0$ allows us to obtain closed-form linear expressions for borrowing, so we use this to guide our specification of the linear regression for borrowing. However, the assumption that $\nu = 0$ predicts that all firms pay a uniform interest rate. Hence the "true" specification corresponds to $\nu > 0$ but close to zero. This generates a nonlinear borrowing regression that allows the interest rate to vary across firms. Note, however, that our model as derived in Proposition 2.3 requires that ν be at most a small positive number; thus the cross-firm variation in interest rates is likely to be small. We present both linear and nonlinear regressions for all variables of interest.

We presume that the key element of heterogeneity of firms is the collateralizable earnings or wealth (W) of their owners, which is unobserved. The entrepreneurial wealth distribution generates a size distribution of firms, with observed capital stock, wage bill, borrowing, and profits. We assume all firms were credit-constrained, that is, the distribution of W has an upper end point which falls below the level at which the first-best can be attained. Thus, we assume a given set of firms with varying W, all of which are active but credit-constrained.

5.1. *Borrowing and Capital Stock*

In the baseline model developed in Section 2, we assume that capital is the sole factor of production. This implies that output $f(\gamma)$ or capital stock (γ) can be used interchangeably to represent firm size. We use capital stock γ. Also, in a static setting, capital stock is proportional to borrowing, so we can use γ to represent either capital stock or firm borrowing.

First, consider the simple case where $\nu = 0$. Given the restriction on the wealth distribution used in Proposition 2.3 and using equation (2.11), we obtain a simple linear equation for capital stock in terms of entrepreneurial wealth as

$$\gamma = \alpha(\theta) + \beta(\theta)W, \tag{5.1}$$

where $\alpha(\theta) \equiv \frac{d}{I(1+\pi(\theta))}$ and $\beta(\theta) \equiv \frac{\theta}{I(1+\pi(\theta))}$. The variable $\pi(\theta)$ denotes the equilibrium profit rate corresponding to the enforcement parameter θ. If credit supply is infinitely elastic, then π is constant, and if credit supply is inelastic, it is increasing. So we know that $\pi(\theta)$ is nondecreasing. Hence $\alpha(\theta)$ is non-increasing. Moreover, $\beta(\theta)$ must be non-decreasing. If not, then when θ increased, credit demand would go down for all firms, which is inconsistent with an upward-sloping supply of credit.

The problem with estimating equation (5.1) directly is that W is unobserved. We therefore proceed on the following assumptions that allow us to proxy W with the firm's assets measured in 1990.

- Entrepreneurs' wealth has not changed between 1990 and year $t > 1990$ or can be proxied by wealth in 1990.
- All states had the same pre-DRT θ, denoted by $\overline{\theta}$.
- Once a state gets a DRT, its θ changes to $\overline{\theta}+\mu$, where $\mu > 0$.[20]

Using $\overline{\gamma}_j$ to denote firm j's fixed assets in 1990, we have

$$\overline{\gamma}_j = \alpha(\overline{\theta}) + \beta(\overline{\theta})W_j, \tag{5.2}$$

which implies

$$W_j = \frac{\overline{\gamma}_j - \alpha(\overline{\theta})}{\beta(\overline{\theta})}. \tag{5.3}$$

If firm j is in a state that has not yet received a DRT in year t, we have $\gamma_{jt} = \overline{\gamma}_j$. If it is in a state that received a DRT in year t, then

$$\gamma_{jt} = \alpha(\overline{\theta}+\mu) + \beta(\overline{\theta}+\mu)W_j.$$

Using this together with equation (5.3), we have

$$\gamma_{jt} = \overline{\gamma}_j + \phi \cdot \mathrm{DRT}_{jt} + \psi \cdot (\mathrm{DRT}_{jt} \times \overline{\gamma}_j), \tag{5.4}$$

where $\phi \equiv \alpha(\overline{\theta}+\mu) - \alpha(\overline{\theta})[1 + \frac{\beta(\overline{\theta}+\mu)}{\beta(\overline{\theta})}] < 0$ and $\psi \equiv \frac{\beta(\overline{\theta}+\mu)-\beta(\overline{\theta})}{\beta(\overline{\theta})} > 0$.

5.2. *Interest Rates*

Corresponding to equation (2.13), the empirical specification for the interest rate is the following. Let $g(\gamma) \equiv \frac{f(\gamma)}{\gamma}$ denote the average rate of return to the firm's assets and suppose it is a locally linear function of γ: $g(\gamma) = \zeta_0 + \zeta_1 \cdot \gamma$. Then

$$r_{jt} = r_0 + \rho \cdot \mathrm{DRT}_{jt} + [\overline{\theta} + \chi \cdot \mathrm{DRT}_{jt}] \times g(\gamma_{jt}), \tag{5.5}$$

[20]To be rigorous, we can assume that 1990 wealth predicts current wealth with error. Then we have a classical source of measurement error, resulting in attenuation bias: our estimated effects will be smaller than the true effects.

where $\rho, \chi > 0$. Substituting for γ_{jt} from equation (5.4) and using the fact that $\mathrm{DRT}_{jt}^2 = \mathrm{DRT}_{jt}$ since it is a 0–1 variable, we get

$$r_{jt} = \rho_0 + \rho_1 \overline{\gamma}_j + \rho_2 \mathrm{DRT}_{jt} + \rho_3(\mathrm{DRT}_{jt} \times \overline{\gamma}_j), \tag{5.6}$$

where $\rho_0 \equiv \overline{\theta}\zeta_0$, $\rho_1 \equiv \overline{\theta}\zeta_1$, $\rho_2 \equiv \rho + \chi\zeta_0 + \overline{\theta}\zeta_1\phi + \chi\zeta_1\phi$, and $\rho_3 \equiv \chi\zeta_1 + \overline{\theta}\zeta_1\psi + \chi\zeta_1\psi$.

If the firm is operating on the concave portion of the production function (i.e., $\gamma_{jt} > b'$), we have $\zeta_0 > 0$, $\zeta_1 < 0$, which implies $\rho_1 < 0$, $\rho_2 > 0$, $\rho_3 < 0$. While the effect on the interest rate for large firms is ambiguous, the theory places restrictions on the intercept and slope effects of DRT: the intercept effect ρ_2 is positive and ρ_3, the slope with respect to 1990 asset size, is negative.[21] However as mentioned in Section 2.3, our model assumes that ν is at most a small positive number. This implies that the interest rate does not vary much across firms of varying size, and so we expect ρ_3 to be small.

5.3. *Profits and Wage Bill*

In the model, the profit of the firm is stochastic, since it depends on the state of nature: success or failure. With idiosyncratic risk, the average profit of a firm of a given W equals $\Pi(W;\theta) \equiv \bar{y}f(\gamma(W;\theta)) - \gamma(W;\theta)I(1+\pi(\theta))$, where $\gamma(W;\theta) \equiv \gamma^H(W,\pi(\theta);\theta)$ denotes the equilibrium capital stock of the firm with wealth W with enforcement parameter θ. Hence

$$\frac{\partial \Pi}{\partial \theta} = \left[\bar{y}f'(\gamma(\theta;W)) - I(1+\pi(\theta))\right]\frac{\partial\gamma(\theta;W)}{\partial\theta} - \gamma(\theta;W)\frac{\partial\pi(\theta)}{\partial\theta}. \tag{5.7}$$

For credit-constrained firms, $\bar{y}f'(\gamma(\theta;W)) > I(1+\pi(\theta))$. For the small firms whose borrowing decreases as a result of DRTs, credit becomes tighter and its cost increases, and so the effect of DRTs is unambiguously negative. The effect is ambiguous for all other firms.

The specification of the profit regression is analogous to the specifications for borrowing and interest rates, since the effect of the DRT variable varies with the size of the firm. We expect the intercept effect to be negative.[22] The DRT effect on the slope with respect to firm size is theoretically ambiguous. If the positive effect on borrowing dominates, then the slope effect is positive.

[21]It is easy to incorporate the possibility of some firms operating on the nonconcave part of the production function by extending the linear specification of the average rate of return to a quadratic specification, which gives rise to an additional quadratic term in firm size on the right-hand side of equation (5.6).

[22]Technically, the intercept effect corresponds to a firm with zero asset size. In Section 6.3, we move away from the linear specification and identify the effects on firms in different parts of the size distribution.

To examine the effect on the wage bill, the model must be extended to a multifactor context. This extension is developed in Appendix A.1, where the analysis justifies a similar specification as above. Under the assumption that GE effects arise in the credit market but not in the labor market, the wage bill is expected to contract in small firms and expand in large firms.[23]

6. EMPIRICAL RESULTS

6.1. *Linear Specification*

Tables V and VI report firm-level linear regressions for borrowing, plants and machinery, and profits (before depreciation and taxes).[24] The presence of a DRT is captured by a dummy variable that takes value 1 if the state where the firm is located had a DRT in operation that year. All regressions include year dummies that control for national level changes in the economic environment that would have affected the dependent variables. All regressions include borrower fixed effects, so we can be assured that all time-invariant firm characteristics are controlled for. In addition, we cluster all standard errors at the state level. This controls for serial correlation in the error terms over time within states (Bertrand, Duflo, and Mullainathan (2004)).[25]

Columns 1–4 in Table V show our results for the effect of DRTs on firm-level borrowing. In column 1, we estimate the average effect of DRTs across firms of different sizes. This average effect of DRTs on all firms in the sample is positive, consistent with the idea that aggregate credit increased as a result of the improved enforcement. In column 2, we add controls for state-specific trends. This ensures that the estimated DRT effect is not confounded by secular changes in borrowing at the state level that may have coincided with DRT establishment. Including these controls reduces the DRT impact by about half, but it continues to be positive and significantly different from zero. In column 3, we introduce interactions of DRT with tangible assets measured in 1990, as in the linear specification in equation (5.4). We also include size-specific year dummies, which control for any year-to-year changes in the national economic environment that may also have had distributive effects on borrowing. In this way, we control for the economywide deregulation and other market-friendly reforms in the 1990s that might have affected firms of different sizes differently. Next, in column 4, we add controls for state-specific time trends. This addresses the concern that states that were more favorable to big

[23]However, as we discuss in Section 7, in an alternative formulation, the GE effects arise in the labor rather than the credit market. That model is developed in Appendix A.2.

[24]Some regressions (e.g., borrowing) are run with fewer firms and observations because of problems with missing values in the Prowess data set. Years are classified by the year in which the fiscal year ends, that is, 1992 means the fiscal year ending in calendar year 1992.

[25]In Appendix B, we control for serial correlation by clustering the standard errors at the borrower level.

TABLE V

THE EFFECT OF DRT ON BORROWING, ASSETS, AND PROFITS: MAIN SPECIFICATION[a]

	Borr.				PlaMa				Profits			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DRT	11.07***	5.023**	−18.81***	−5.793***	8.269	11.08***	−26.93***	−26.75***	3.773***	4.451***	−7.389***	−6.818***
	(3.75)	(2.66)	(−5.38)	(−6.59)	(1.69)	(2.96)	(−8.71)	(−9.68)	(2.83)	(4.12)	(−4.17)	(−4.81)
DRT*Tang.Ass.			0.711***	0.223***			1.273***	1.125***			0.386***	0.350***
			(6.39)	(7.43)			(9.69)	(9.99)			(5.80)	(6.17)
YearDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YDumm*Size	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Statetrend	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Statetrendsize	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Number of firms	1406	1406	1406	1406	1683	1683	1683	1683	1683	1683	1683	1683
$\tilde{W}$			26.45	25.95			21.15	23.77			19.14	19.50
R^2	0.00564	0.00840	0.236	0.331	0.0224	0.0252	0.442	0.489	0.0118	0.0174	0.274	0.353
N	9762	9762	9762	9762	16,605	16,605	16,605	16,605	16,605	16,605	16,605	16,605

[a] t statistics are given in parentheses. Standard errors are clustered at the state level. All regressions run on data from 1992–2003. All regressions include borrower fixed effects. The dependent variables are new long-term borrowing (Borr.), plants and machinery (PlaMa), and profits, respectively. DRT is an indicator variable that takes value 1 if a DRT was operating in the state of the firms' headquarters at the end of the fiscal year and 0 otherwise. DRT*Tang.Ass. is a multiplicative interaction of DRT with tangible assets defined in 1990. The first row for each variable reports the results of a regression which estimates the average impact of DRT and includes year dummies to control for year-specific nationwide shocks. The second row allows for a linear state-specific time trend by introducing a multiplicative interaction of state dummies with time. In the specification which uses DRT*Tang.Ass. as a variable of interest we add year dummies interacted with the size of 1990 tangible assets (YDumm*Size) to allow for year-specific distributional effects. In the fourth column, the plain Statetrend is added in conjunction with Statetrend interacted with 1990 tangible assets to control for state-specific time-varying distributional effects. The statistic $\tilde{W}$ reports the implied value of 1990 tangible assets for which the level of the dependent variable would be the same with and without DRT. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

TABLE VI

ROBUSTNESS WITH RESPECT TO SIZE-SPECIFIC TIME TRENDS, STATE-LEVEL LENDING, AND INDUSTRY SHOCKS[a]

	Borr.				PlaMa				Profits			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DRT	−17.61***	−17.79***	−15.14***	−17.63***	−26.62***	−27.15***	−8.688***	−25.24***	−7.418***	−7.394***	−1.866	−7.715***
	(−5.01)	(−4.98)	(−3.76)	(−3.90)	(−9.22)	(−9.30)	(−3.23)	(−6.54)	(−4.02)	(−3.98)	(−1.26)	(−4.37)
DRT*Tang.Ass.	0.647***	0.647***	0.582***	0.626***	1.129***	1.129***	0.582***	1.090***	0.367***	0.367***	0.237***	0.362***
	(4.64)	(4.59)	(5.55)	(4.11)	(7.60)	(7.51)	(10.89)	(8.87)	(4.81)	(4.79)	(4.49)	(6.57)
YearDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YDum*Size	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes
YDum*SizeClass	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No
YDum*SizeCl.*Size	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
State-loglend	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Y*NIC2	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Number of firms	1406	1406	1382	1406	1683	1683	1679	1683	1683	1683	1679	1683
$\tilde{W}$	27.19	27.47	26.04	28.18	23.58	24.05	14.93	23.16	20.22	20.13	7.886	21.30
R^2	0.267	0.267	0.229	0.391	0.469	0.470	0.468	0.518	0.288	0.288	0.234	0.399
N	9762	9762	8900	9762	16,605	16,605	15,344	16,605	16,605	16,605	15,344	16,605

[a] t statistics are given in parentheses. Standard errors are clustered at the state level. All regressions use borrower fixed effects. All regressions run on data from 1992–2003. The dependent variables are new long-term borrowing (Borr.), plants and machinery (PlaMa), and profits, respectively. DRT is an indicator function which is 1 if a DRT was operating in the state of the firms' headquarters at the end of the fiscal year and 0 otherwise. Column 2 uses a linear specification with the interaction of DRT with tangible assets (as of 1990). This table reruns our main specification in Table V with the following additional controls: YDum*SizeClass interacts year dummies with size class dummies and we use deciles of 1990 tangible assets as our classes and have 10 size classes; YDum*SizeClass creates a dummy variable which is 1 for a specific year and a specific size class, and 0 otherwise. With YDum*SizeCl.*Size, each size class is allowed to have a linear slope effect and the respective dummy variable is multiplied with 1990 tangible assets. This means that the set of dummy variables YDum*SizeCl. is multiplied with 1990 tangible assets. Regressions that have State-loglend includes 12 variables that vary by state year, and measure the level of credit (in logs) given to agriculture, artisan and village industry, small scale industry, and total credit given by the State Bank of India, nationalized banks, and all scheduled commercial banks. Y*NIC2 stands for an interaction of year dummies with industry dummies and we use two-digit NIC code industries. The statistic $\tilde{W}$ reports the implied value of 1990 tangible assets such that the value of the dependent variable would be the same with and without DRT. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

business may have adopted DRTs sooner and that this could have confounded the DRT effect. As column 4 shows, the results continue to hold even after this effect is controlled for: DRTs had a significant negative intercept effect and a positive slope effect on borrowing, as predicted by our model.

Columns 5–8 in Table V show analogous results for plants and machinery, and columns 9–12 show results for profits. The results are similar: a negative effect on the intercept and a positive effect on the slope with respect to firm size. In the bottom rows of the table, we estimate the threshold level of firm wealth ($\bar{W}$) below which the DRT effect is negative. For all three variables, $\bar{W}$ lies between Rs 191 and 264 million, which is roughly within the third quartile of the size distribution. Hence the linear specification indicates that the expansionary effects of DRTs were limited to the top 25% of firms.

In Table VI, we investigate the robustness of our results to additional controls. Whereas in Table V we only controlled for year effects that were linear in size (Table V, columns 3, 7, and 11), we now also control for time-varying patterns more flexibly by interacting the year dummies with size class dummies (Table VI, columns 1, 5, and 9). Next, we allow the time-varying patterns within each size class to also vary with size (columns 2, 6, and 10). It is evident that these alternative specifications of size-specific trends cause almost no change in the estimated effects of DRT on the intercept and slope of the regression. This reassures us that the estimated distributive effects that we attribute to DRTs are not being driven by other coincidental state-level changes that benefit large firms at the cost of small firms.

In columns 3, 7, and 11 in Table VI, we instead address the concern that our results are driven by changes in national policies for small-scale industries. Suppose that the Indian central bank relaxed its rules about lending to the priority sector (agriculture, artisan and village industries, and small-scale industries) in different states to coincide with DRT adoption. This would lead to a contraction of small firms like the one we estimated in Table V. To control for this, we include in the regressions the volume of credit provided by commercial banks to the priority sector, as determined by targets laid down by the Reserve Bank of India. The controls reduce the size of the estimated intercept and slope effects only slightly: the effects remain large and statistically significant. The firm-size thresholds for zero effects on plants and machinery and profits fall, but the threshold for zero effect on borrowing remains unaltered at Rs 260 million, indicating once again that only the top quartile of firms expanded as a result of DRTs.

Finally, columns 4, 8, and 12 address the issue that different industries may have faced different economic environments over time, and if DRT roll-out coincided with a relative growth period for industries that require a large scale of operation, then it would be incorrect to attribute the observed patterns to DRTs. To avoid this problem, we introduce as controls industry-specific time trends in the regressions with industry defined at the two-digit National Industrial Classification levels. The results are very similar to those in columns 1, 5, and 9, indicating that industry-level time patterns cannot explain our results.

6.2. *Was Small Firms' Credit Shrinking Even Before DRTs?*

We have seen that our main results are robust to various alternative controls, including those for size-specific time-varying patterns and state-level controls for credit policy preferences toward small firms. While this is strong evidence that DRTs are associated with a negative distributive effect on borrowing, plants and machinery, and profits, to establish causation, it is important that this negative distributive effect did not occur before DRTs were established. To check this, we separate the Indian states into those that received a DRT early (before 1995) versus those that received late (after 1995). Using data for the time period 1988–1993, we examine whether the time trends in the key variables were different for these two categories of states. We do this in two different ways. First, we run a regression of the form

$$\begin{aligned} y_{jt} = \overline{y}_j + T_t + \beta_1(\text{early}_j \times t) + \beta_2(W \times T_t) \\ + \beta_3(\text{early}_j \times W \times t) + \varepsilon_{jt}. \end{aligned}$$

Here, early_j is a dummy variable that takes value 1 for states that received a DRT before 1995, T_t denotes a set of time dummies, and t is time. Thus the variable β_1 captures the differential time trend during 1988–1993 in the dependent variable in states that received a DRT early as compared to the time trend in late receivers, and the set of time dummies T_t captures the overall time trends in that time period. We begin in columns 1 and 5 of Table VII, and columns 1 and 5 of Table VIII by estimating the average effect on all firms, regardless of size, that is, $\beta_2 = \beta_3 = 0$ in columns 1 and 5. The results indicate that the time trend is significantly different between the two categories of states only for borrowing, not for plants and machinery, profits, or interest rates. In columns 2 and 6, we include the interaction with tangible assets. Once again, trends are different for early versus late states only for firm borrowing. Importantly, however, there is no evidence that early states were more likely to have a negative distributive effect on borrowing before DRTs were set up. In fact, early states were *less* likely to have a negative trend in borrowing for small firms. In fact, the point estimates in column 2 of Table VII indicate that borrowing by small firms was increasing in the pre-DRT period. They were also less likely to have a positive trend in borrowing between 1988 and 1993.

Columns 3 and 4 refine the analysis by replacing the early adoption dummy with the number of years that the state had a DRT during the period 1993–2003. This gives rise to a regression equation such as

$$\begin{aligned} y_{jt} = \overline{y}_j + T_t + \beta_1(\text{DRT years}_j \times t) + \beta_2(W \times T_t) \\ + \beta_3(\text{DRT years}_j \times W \times t). \end{aligned}$$

Clearly, states that received DRTs early have a high value for DRT years_j. If DRTs were established sooner in states where small firms' credit was con-

TABLE VII

PRETRENDS[a]

	Borr.				PlaMa			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Early*t	−1.534***	1.847***			0.307	0.332		
	(−3.07)	(5.19)			(0.39)	(0.64)		
Early*t*Tang.Ass.		−0.0491***				−0.00835		
		(−5.02)				(−0.24)		
Drtyears*t			−0.244**	0.371***			−0.0811	0.122
			(−2.59)	(5.10)			(−0.57)	(1.26)
Drtyears*t*Tang.Ass.				−0.00937***				−0.00168
				(−4.13)				(−0.25)
YearDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YDum*Size	No	Yes	No	Yes	No	Yes	No	Yes
Number of firms	552	552	552	552	1741	1741	1741	1741
R^2	0.0343	0.726	0.0329	0.726	0.0657	0.728	0.0658	0.728
N	1276	1276	1276	1276	8221	8221	8221	8221

[a] t statistics are given in parentheses. Standard errors are at the state level. All regressions use borrower fixed effects. The unit of observation is a firm year. Dependent variables are new long-term borrowing (Borr.) and plants and machinery (PlaMa). These regressions run on data from 1988–1993. The first row reports the time trend in new long-term borrowing (Borr.) for our sample of firms and the interaction of the time trend with the variable early. Early = 1 if the DRT was introduced in the first wave of DRT (before 1995) and Early = 0 otherwise. The second row estimates the changing distribution of credit over time by estimating a time trend for a linear estimation of a time trend interacted with firm size measured in 1990 tangible assets. Then an interaction of the time trend with the early variable is added. DRT years counts the number of years the firm had a DRT in the sample period 1992–2003. For example, row 3 reports the estimates of a time trend and then interacts the time trend with DRT years. The fourth row reports the differential distributional trend for firms with different numbers of DRT years. ** $p < 0.05$; *** $p < 0.01$.

TABLE VIII

PRETRENDS[a]

	Profits				Intrate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Early*t	−0.125	−0.0799			−0.0243	−0.00860		
	(−0.75)	(−1.39)			(−0.21)	(−0.08)		
Early*t*Tang.Ass.		−0.00249				−0.000483		
		(−1.42)				(−0.46)		
Drtyears*t			−0.0656**	−0.0173			0.00893	0.0122
			(−2.82)	(−1.60)			(0.42)	(0.60)
Drtyears*t*Tang.Ass.				−0.000647*				−0.000144
				(−2.07)				(−0.69)
YearDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YDum*Size	No	Yes	No	Yes	No	Yes	No	Yes
Number of firms	1741	1741	1741	1741	1724	1724	1724	1724
R^2	0.0572	0.484	0.0581	0.484	0.0202	0.0208	0.0202	0.0208
N	8221	8221	8221	8221	8083	8083	8083	8083

[a] t statistics are given in parentheses. Standard errors are at the state level. All regressions use borrower fixed effects. The unit of observation is a firm year. Dependent variables are profits and the interest rate (Intrate). These regressions run on data from 1988–1993. The first row reports the time trend in profits for our sample of firms and the interaction of the time trend with the variable early. Early = 1 if the DRT was introduced in the first wave of DRTs (before 1994) and Early = 0 otherwise. The second row estimates the changing distribution of profits over time by estimating a time trend for a linear estimation of a time trend interacted with firm size measured in 1990 tangible assets. Then an interaction of the time trend with the early variable is added. DRT years counts the number of years the firm had a DRT in the sample period 1992–2003. For example, row 3 reports the estimates of a time trend and then interacts the time trend with DRT years. The fourth row reports the differential distributional trend for firms with different numbers of DRT years. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

tracting and large firms' credit was expanding, then "DRT years" should have a negative intercept and a positive slope. Instead, we find the opposite: in states with higher DRT years, credit was increasing for small firms and was decreasing for large firms during 1988–1993. Columns 5–8 show the same analysis for plants and machinery, and no significant difference is found in pre-93 trends between early and late adopters.

Table VIII shows analogous results for profits and interest rates. Only in the case of profits do we see a systematic difference in trends for large firms between early and late DRT adopters; once again this goes in the direction opposite to what occurred after DRTs were set up, where the profits of large firms increased substantially.[26] We conclude that our results on the effects of DRTs cannot be attributed to preexisting trends for borrowing or profits to contract in small firms and expand in large firms.

6.3. *Average Effects for Different Quartiles*

So far we have been working with a linear specification of the effect of DRTs. Although this is a convenient specification and delivers some very useful insights, it is evident from the theory we developed in Section 2 that the linear expression is correct only under restrictive conditions. For example, for borrowing or capital stock, we need to assume that lenders cannot appropriate any firm output or revenues in the event of loan default ($\nu = 0$). We also need to assume that θ—the parameter that captures the effectiveness of credit enforcement—does not vary across firms of different sizes. For profits, the linear specification requires that the production function have constant returns to capital. Finally, since the negative intercept effect pertains to a purely hypothetical firm of zero size, it is difficult to interpret the implied effects for small firms.

For these reasons, in Table IX we turn to a specification where the DRT effect is estimated nonlinearly. This is done with a regression of the form

$$\gamma_{jt} = \sum_{k=1}^{4} Q_{jk}\{\overline{\gamma}_j + \mathrm{DRT}_{jt} + T_t + t \times S\},$$

where Q_{jk} is a dummy variable that takes value 1 for all firms that fall in quartile k of the size distribution, T_t is a set of time dummies, and $t \times S$ are state dummies interacted with a linear time trend. In this way, the DRT effect is estimated separately for each size class. As controls, we include year dummies interacted with size class and state-specific time trends. In column 1, the DRT effect on borrowing is negative and significant at 10% for the first quartile, negative and significant at 5% for the third quartile, and positive and significant at

[26]See Section 6.3.

TABLE IX

AVERAGE EFFECTS BY QUARTILE[a]

	Borr. (1)	PlaMa (2)	Profits (3)
DRT	−1.533*	−2.247*	−0.224
	(−1.95)	(−1.96)	(−0.77)
DRT*quart = 2	2.932***	1.020	0.354
	(3.72)	(0.76)	(0.90)
DRT*quart = 3	−0.167	1.052	1.287
	(−0.15)	(0.63)	(1.55)
DRT*quart = 4	20.06***	43.78***	14.73***
	(3.98)	(3.62)	(4.33)
YearDummy	Yes	Yes	Yes
YearDum*SizeClass	Yes	Yes	Yes
Statetrend*SizeClass	Yes	Yes	Yes
DRT effect on quart. 1	−1.533*	−2.247*	−0.224
p-value quart. 1 effect	(0.0647)	(0.0624)	(0.448)
DRT effect on quart. 2	1.399***	−1.227**	0.129
p-value quart. 2 effect	(0.00958)	(0.0389)	(0.645)
DRT effect on quart. 3	−1.700**	−1.195	1.062
p-value quart. 3 effect	(0.0437)	(0.218)	(0.161)
DRT effect on quart. 4	18.53***	41.54***	14.50***
p-value quart. 4 effect	(0.00175)	(0.00207)	(0.000315)
Number of firms	1406	1683	1683
R^2	0.0189	0.0633	0.0491
N	9762	16,605	16,605

[a] t statistics are given in parentheses. Standard errors are clustered at the state level. All regressions use borrower fixed effects. All regressions run on data from 1992–2003. The dependent variables are new long-term borrowing (Borr.), plants and machinery (PlaMa), and profits, respectively. DRT is an indicator value that takes value 1 if a DRT was operating in the state of the firms' headquarter at the end of the fiscal year and zero otherwise. DRT*quart $= j$ is the additional effect of DRT over and above the baseline effect for first quartile firms captured by the variable DRT. "DRT effect on quart j" is the overall effect of DRTs for a firm of quartile j. "p-value quart. j effect" is the p-value from an F test with the null hypothesis that this effect is zero. All regressions are run with quartile specific time-varying time trends (an interaction of the quartile dummies and the linear time-varying state trend). * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

the 1% level for the second and fourth quartiles. For plants and machinery in column 2, there is a negative and significant effect for the first and second quartiles, negative and insignificant effect for the third quartile, and a positive and significant effect for the fourth quartile. For profits, the effects are insignificant for the bottom three quartiles, and positive and significant (at 1%) for the top quartile. Thus, we confirm that DRTs caused a contraction of credit and fixed assets for small firms, and an expansion for the largest firms. Profits increased significantly only for the top 25% of firms.

6.4. *Effects on Interest Rates*

We now turn to the important task of examining the channel through which our distributive effects come about. In our model in Section 2, we assume that the supply of credit is inelastic; however, as noted previously, an upward sloping supply curve for any input factor could give us the same result. We start by examining the evidence for an inelastic credit supply. In Table X, we use the Prowess data to estimate DRT effects on the interest rate.

Column 1 in Table X shows that, on average, DRTs increased interest rates significantly by 0.9 percentage points, statistically significant at 5%, after controlling for year dummies and state-specific time trends. Column 2 presents the linear specification from equation (5.6), where the DRT effect is interacted with 1990 firm size, with controls for year dummies, size–class–year dummies and state-specific time trends. The intercept effect continues to be significant and of the same magnitude as before. However, the interaction effect is insignificant, indicating that the interest rate effect does not vary by firm size. This is unsurprising, since as discussed previously in Section 2.3, with the small ν assumed in our model, interest rates do not vary much across firms. Column 3 shows the effects on different quartiles. The effects for the bottom three quartiles are not precisely estimated, although the estimate of the effect on the bottom quartile is 2.3 percentage points, with a p-value of 0.103. The effect on the top quartile is a significant 1.8 percentage point increase. The effects on the middle two quartiles are smaller and less significant. We therefore see increases for the top and bottom quartiles, with a large point estimate that is precise only for the top quartile.

One possible reason why these nonlinear estimates are imprecise is that the Prowess data only provide information on interest rate obligations on total debt. Our theoretical predictions are for interest rates on new loans. As we saw in Table III, new loans comprise only about one-third of all long-term borrowing, so it is likely that our results in Table X are an underestimate of the true effect of DRTs. In Table XI, we examine the DRT effects on the interest rates on new loans, taken from the private bank data set used in Visaria (2009).[27] Each observation represents data aggregate over all new loans issued to a firm by the private bank in a particular quarter of the year.

In all specifications, we obtain a significant rise in the interest rate. Column 1 shows that DRT adoption caused the interest rate on new loans to increase by 1.8 percentage points, after controlling for borrower fixed effects and quarter

[27]The private bank data set does not include any loans that were fully paid off by the year 2000, since the bank "retired" these loans from the data base. To avoid attrition bias, we restrict attention to loans of 8 years or longer duration that originated in 1992 or later. Loans in this category would not have been retired from the data base because according to their repayment schedule, they were expected to remain active through 2000. We do not expect loans to be retired from the data base before their repayment duration ends, because prepayment of loans is an extremely rare event in the private bank data set.

TABLE X

EFFECTS ON THE INTEREST RATE USING PROWESS DATA[a]

	(1)	(2)	(3)
DRT	0.989**	0.990**	2.326
	(2.53)	(2.72)	(1.70)
DRT*Tang.Ass.		−0.00112	
		(−0.39)	
DRT*quart = 2			−2.704
			(−1.43)
DRT*quart = 3			−1.530
			(−1.09)
DRT*quart = 4			−0.544
			(−0.33)
YearDummy	Yes	Yes	Yes
YearDum*SizeClass	No	No	Yes
Statetrend*SizeClass	No	No	Yes
YDumm*Size	No	Yes	No
Statetrend	Yes	Yes	No
Statetrendsize	No	Yes	No
DRT effect on quart. 1			2.326
p-value quart. 1 effect			(0.103)
DRT effect on quart. 2			−0.378
p-value quart. 2 effect			(0.656)
DRT effect on quart. 3			0.796
p-value quart. 3 effect			(0.264)
DRT effect on quart. 4			1.782**
p-value quart. 4 effect			(0.0565)
Number of firms	1671	1671	1671
$\tilde{W}$		882.8	
R^2	0.00982	0.0129	0.0210
N	16,049	16,049	16,049

[a] t statistics are given in parentheses. Standard errors are clustered at the state level. All regressions use borrower fixed effects. All regressions run on data from 1992–2003. The dependent variable intpay is the interest rate in percent terms. Tables V and IX describe the setup in detail. DRT takes value 1 if the firm is subject to DRTs and 0 otherwise. The first two regressions estimate the average impact of DRT on the interest rate and year dummies control for year-specific shocks. Column 2 uses a linear specification and includes controls for nationwide year-specific distributive effects (year dummies and YDumm*Size). The last two columns report the results of a quartile regression. DRT*quart $= j$ is the additional effect of DRT over and above the baseline effect for first quartile firms captured by the variable DRT. "DRT effect on quart. j" is the overall effect of DRTs for a firm of class j. "p-value quart. j effect" is the p-value from an F test with the null hypothesis that this effect is zero. All regressions are run with the appropriate state-specific time trends. The statistic $\tilde{W}$ reports the implied value of 1990 tangible assets such that the value of the dependent variable would be the same with and without DRT. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

dummies. In column 2, the effect increases to 1.9 percentage points, after controlling for loan size and duration. The estimate gets larger (2.3) in columns 3 and 4, where we introduce interactions with firm size (measured by 1990 fixed

TABLE XI

EFFECTS ON THE INTEREST RATE OF NEWLY ISSUED LOANS USING BANK DATA: STARTING IN 1992 USING 8 YEAR LOANS OR LONGER[a]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DRT	1.845**	1.930**	2.330***	2.331***	2.487**	2.387*	3.000*	3.359**
	(2.33)	(2.47)	(3.07)	(2.97)	(2.31)	(2.02)	(1.88)	(2.17)
Loan size		0.00460		0.00348		−0.00103		−0.00214
		(0.71)		(0.50)		(−0.19)		(−0.34)
Loan duration		−0.000829*		−0.000819		−0.000898*		−0.000972
		(−1.75)		(−1.46)		(−1.93)		(−1.73)
DRT*Assets			−0.0000453	0.000831			0.00107	−0.00480
			(−0.01)	(0.18)			(0.10)	(−0.41)
QuartDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
QDum*Size	No	No	Yes	Yes	No	No	Yes	Yes
State Trend	No	No	No	No	Yes	No	Yes	Yes
State Trend*Size	No	No	No	No	No	No	Yes	Yes
Number of firms	832	832	670	670	832	832	670	670
R^2	0.126	0.137	0.190	0.198	0.172	0.183	0.291	0.301
N	1557	1557	1344	1344	1557	1557	1344	1344

[a] t statistics are given in parentheses. Standard errors are clustered at the state level. All regressions use borrower fixed effects. The dependent variable intpay is the interest rate on newly issued project loans given out by the private bank. The variable DRT takes value 1 if the state is subject to DRT in the year and 0 otherwise. We observe loans given out in each quarter and control for time effects using quarter dummies. Further, we interact quarter dummies with fixed assets measured in the last available quarter before 1991 and measured in the earliest available quarter if there is no information prior to 1991. In addition, we allow for time-varying state trends, and time-varying size-specific state trends. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

assets). Once again, as in Table X, the effect does not vary significantly with firm size. Columns 5–8 introduce additional controls for quarter size, state, and state-size specific time trends. The estimated effect gets even larger, growing to 3.35 in column 8, which includes all the controls.

6.5. *Implied Elasticities*

Some rough calculations based on these estimates imply very large elasticities of demand for long-term borrowing and assets with respect to the interest rate. The elasticity of long-term borrowing is approximately −4.1 for firms in the lowest quartile and approximately −5.2 for the lowest decile of the asset class. The elasticity of plants and machinery is smaller; −1.5 for the lowest quartile and −2 for the lowest decile of firms.[28]

It is important to keep in mind that since the firms in our data are credit-constrained, these are not the usual price elasticities of demand. One interpretation of these high estimates is that the small firms are driven out of the market entirely. Given the short-run inflexibility of inputs such as expertise and local knowledge, banks may respond to the relatively higher profitability of lending to large firms by lending more to them and, consequently, lending less to small firms. This may cause some small firms to lose access to bank credit altogether. The fact that the negative impact on their assets, although large, is relatively less severe may be a consequence of their finding other sources of finance to make up for the lack of long-term bank credit.[29]

6.6. *Evidence for Low Elasticity of Credit Supply*

The elasticity of credit supply is a key parameter in this paper. While our empirical analysis so far finds support for the predictions that follow from an assumption of inelastic credit supply, whether credit is truly inelastic is an empirical question. In this section, we seek independent evidence on this issue.

We hypothesize that to lend, banks need information, expertise, experience, and relationships with clients. Although DRTs increase the profitability of lending, it is difficult for banks to expand lending operations quickly because they face substantial adjustment costs of increasing these inputs. The transaction

[28]To calculate these elasticities, we use the average values of the interest rate, new long-term borrowing, and assets for each class of firms between 1994 and 1999. The point estimate of the effect of DRT on credit, and on plants and machinery is taken from running the quartile regression as in Table IX or the same specification but with deciles. For the interest rate effects, we use the implied change in the interest rate from the private bank data base based on the specification in column 6 in Table XI, restricted to the smaller one-half of firms in that data set. The interest rate increase for these firms is approximately 4.3 percentage points.

[29]Some of the additional channels we consider below, such as incomplete contracting channel or general equilibrium effects stemming from other input factors, may also be particularly strong for these firms and may reinforce the negative impact.

costs per unit of loan are likely to be higher for small loans, presumably given primarily to small firms. The DRT reform also alters the relative profitability of lending to different clienteles: it becomes more profitable to lend to large corporate clients who have higher collateral, relative to small firms, consumers, and farmers. To take advantage of these opportunities, banks must reorganize their growth patterns, focusing less on growth on the extensive margin (i.e., expanding the number of bank branches, particularly in rural areas) and more on the intensive margin (i.e., expanding lending per branch, particularly in urban areas). Credit and lending personnel have to be shifted from rural to urban sectors. These reorganizations impose administrative costs and delays.

One implication of this hypothesis is that bank lending responses to DRT would occur gradually. Changes would be small at first, and would become larger as time since the reform increased. Table XII provides evidence on how the response to DRTs varies over time. Columns 1–4 show results for new long-term borrowing from the Prowess data set. Controlling for state trends and for priority sector lending targets set by the central bank, columns 1 and 2 show the average post-DRT effect across all firms, while columns 3 and 4 estimate the average effect of the reform 1 year, 2 years, and 3 or more years after DRTs were set up. The effect of DRTs on the level of borrowing increases progressively from the time when the DRT was set up to 3 years after that. The remaining four columns study this issue from the bank's perspective, that is, using data on total outstanding bank credit at the state level from the Reserve Bank of India. Again, the DRT effect increases over time. However, the effects are imprecisely estimated; there is a significant increase only in column 8, for 3 years and more after the DRTs were set up. This lack of precision using the bank lending data may be partly due to the fact that these data are for total outstanding credit rather than new credit. Also, a bank's outstanding credit may include loans that have not yet been repaid. Since as we saw, DRTs improved repayment, this might have a negative effect on outstanding credit, and thus may offset the positive DRT effect on new credit. Moreover, these regressions exclude credit provided by nonbank financial institutions.

In Table XIII, we check whether there is any evidence that banks changed their operations in response to DRTs. Using data from the central bank on location and number of bank branches for each bank, we find that DRTs caused the numbers of city (urban and metropolitan areas) and rural branches to decrease. The number of city branches fell immediately in the year following DRT adoption. The number of rural branches did not change in the year following DRT reform, but in subsequent years decreased progressively by more. At the same time, city credit expanded, particularly 3 or more years after DRT adoption, implying an increase in lending operations per city branch. The effects on rural lending are insignificant, with a negative effect 2 or more years following the reform. These results are consistent with the prediction that DRTs would cause banks to focus more on increasing lending operations within city branches and less on growth on the extensive margin.

TABLE XII

EFFECT OF DRT ON CREDIT: BANK VERSUS FIRMS[a]

	Prowess Data: New Long Term Borrowing				RBI Bank Data: Bank Lending			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DRT	5.023**	−2.065			3179.5	382.2		
	(2.66)	(−0.81)			(0.85)	(1.09)		
DRT year = 1			2.187	−0.652			1832.3	453.7
			(0.95)	(−0.27)			(0.92)	(1.67)
DRT year = 2			6.687***	0.687			2106.0	230.2
			(4.27)	(0.30)			(0.73)	(0.64)
DRT year ≥ 3			16.38***	8.235***			4124.1	1902.0*
			(4.89)	(3.12)			(0.68)	(1.85)
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Statetrend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Priority sector lending	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.00840	0.00837	0.00875	0.00845	0.911	0.974	0.911	0.974
N	9762	8900	9762	8900	1536	1344	1536	1344

[a] Standard errors are clustered at the state level. t statistics are given in parentheses. All regressions run on data from 1992–2003. Columns 1–4 report regressions on Prowess firm-level data and columns 5–8 report regressions on RBI data. Columns 1–4 include firm fixed effects and year fixed effects; columns 5–8 include state fixed effects and quarter fixed effects. The dependent variables are new long-term borrowing (Borr.) for the Prowess data base and statewise total outstanding bank credit for the RBI data. For the Prowess data, DRT is an indicator variable that takes value 1 if a DRT was operating in the state of the firms' headquarters at the end of the fiscal year and 0 otherwise. For the RBI data, DRT take value 1 if the state had a DRT in the previous quarter and 0 otherwise. "DRT year = t" is a dummy that equals 1 if DRTs were introduced t years ago. The dummy DRT year ≥ 3 equals 1 if DRTs were introduced 3 or more years ago. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

TABLE XIII

EFFECT OF DRT ON BRANCHES AND LENDING[a]

	Citybranch		Countrybranch		Citycredit		Countrycredit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DRT	−7.586		−9.536		490.7		13.30	
	(−1.02)		(−1.70)		(1.11)		(0.19)	
DRT year = 1		−9.222***		−0.884		608.5*		20.26
		(−2.84)		(−0.27)		(1.84)		(0.60)
DRT year = 2		−7.230		−12.63*		331.0		−41.50
		(−0.81)		(−1.90)		(0.65)		(−0.59)
DRT year ≥ 3		−8.476		−32.21**		2709.6*		−43.32
		(−0.56)		(−2.22)		(1.96)		(−0.37)
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Statetrend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Priority sector lending	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.973	0.973	0.762	0.778	0.971	0.972	0.980	0.980
N	992	992	1344	1344	992	992	1344	1344

[a]Standard errors are clustered at the state level. t statistics are given in parentheses. All regressions run on data from 1992–2003. The dependent variable are the number of city branches (branches located in metropolitan and urban areas) and country branches (branches located in semi-urban and rural areas) as well as total bank credit reported by city and country branches. All regressions include quarter fixed effects and state fixed effects. DRT is an indicator variable that takes value 1 if a state had a DRT in the previous time-period and is 0 otherwise. "DRT year = t" is a dummy that equals 1 if DRTs were introduced t years ago. The dummy "DRT year ≥ 3" equals 1 if DRTs were introduced 3 or more years ago. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

However, expansion of bank credit per se does not shed evidence on the supply elasticity of credit. Our model predicts that bank profits increased as a result of DRTs. As we have already seen, interest rates increased due to DRTs. In Section 7.1, we see evidence of higher repayment rates by firms. Both factors are consistent with higher bank profits. However, we now examine this question directly by asking whether exposure to DRTs caused banks profits to increase. Using Π_i to denote firm profits, we run regressions of the general form

$$\Pi_{it} = \beta_0 + \beta_1 \sum_{j=1}^{J} (\mathrm{DRT}_{jt} \times \text{presence of bank } i \text{ in state } j) + \beta_2 X_{it} + \varepsilon_{it}. \tag{6.1}$$

We calculate bank i's presence in state j using data on the number of city branches of each bank in each state in 1992, obtained from the Reserve Bank of India.[30] Bank dummies ensure that our results are not being driven by scale effects from the number of total bank branches on the aggregate size of the bank's lending operations. In columns 1–3 of Table XIV, as the dependent variable, we use the profits reported in Prowess for firms that are in the banking industry. For columns 4–6, we use bank profits reported by the Reserve Bank of India (RBI).[31] Using either measure, we find a significant positive effect of exposure to DRTs on bank profits. We run the regression for all banks (column 1), for banks with few branches (column 2), and for banks with a large

TABLE XIV
EFFECT OF DRT ON BANK PROFITS[a]

	Net Profit (RBI Data)			Profits PBDPTA (Prowess Data)		
DRT exposure	9371.9***	14,837.5***	27,987.3***	1.832**	2.015***	3.702***
	(3.55)	(6.31)	(5.29)	(2.55)	(4.06)	(4.48)
Banks	All	Small	Large	All	Small	Large
Observations	924	492	492	859	443	443

[a]Standard errors are clustered at the bank level. All regressions include year fixed effects. All regressions run on data from 1992–2003. The dependent variable is bank profits. Columns 1, 2, and 3 report regressions on bank net profit as reported by the Reserve Bank of India. The last three columns report regressions on the profit data from the Prowess data base. Prowess reports profits gross of depreciation, provisions for bad loans, taxes, and amortization. DRT exposure is the sum of all active branches that were opened in or before 1992 and are located in cities (metropolitan or urban areas) that are subject to DRT in that year. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

[30]Using the number of branches in 1992 avoids the endogeneity problem that might arise if bank branch numbers across states changed in response to DRTs. We focus on city branches since that is where most corporate bank credit is transacted.

[31]The RBI definition of profit subtracts interest and operating expenses as well as provisions and contingencies from income earned. The Prowess data measure profit before taxes and depreciation, but after subtracting interest and operating expenses.

number of branches (column 3), and find the effect is similar and significant in all specifications. Thus DRTs had significant positive effects on bank profits for both small and large banks in the data.

6.7. *Dynamics of Interest Rate and Distributive Effects*

Next we examine how the DRT effects on the interest rate evolved over time. Insofar as the increased interest rate is the primary channel of the general equilibrium effect, this is an important question. If the interest rate effects are immediate but temporary, this could have very different qualitative implications for credit access than effects that are longer lasting.

The interest rate is determined by the interaction of the supply and demand of credit. We hypothesize that credit supply responded to DRTs with a lag, because banks needed to reorganize their lending operations toward large urban clients. Expanding credit to new clients required inputs such as local knowledge, skilled staff, and long-term relationships with customers that are likely to be scarce in the short run. At the same time, clients may also have been slow to formulate new project plans and prepare new loan applications. The time pattern of effects on the interest rate depends on the relative supply and demand responses at different points of time following DRT reform and, therefore, is difficult to pin down in theory. However Table XV provides estimates of the interest rate responses corresponding to different durations since DRT reform, using interest on outstanding loans from the Prowess data, as well as the interest rate on new loans from the private bank data set. As in the case of the average post-DRT effect, the Prowess interest rate effects are imprecisely estimated because old and new loans are pooled together. The private bank data set yields larger and precise effects. In both cases, we see a sizeable rise in the interest rate in the year following DRT reform, followed by a much smaller positive effect the second year, and then a larger effect for the third year and later. Hence the GE effects seem quite persistent over this duration.

We note, however, that inputs such as local knowledge, trained staff, and relationships with customers should be supplied more elastically over a longer time horizon. Therefore, it is likely that if we could estimate the DRT effects on interest rates over a longer duration since DRTs were established, the DRT effect might become progressively smaller.

Returning to our current data, if it were indeed true that the GE effects were persistent, we would expect the same of the distributive effects on borrowing across firms of varying size. Table XVI shows lagged effects of DRT on new long-term borrowing of the first and fourth quartiles of the firm distribution, using Prowess data.[32] Consistent with the evidence on the lagged effects on the interest rate, the effects on borrowing levels are more pronounced 3 or more

[32]Note that the number of firms differs across quarters because quarters are defined on 1990 tangible assets and some firms exit the data base over time.

TABLE XV

DYNAMIC INTEREST RATE EFFECTS[a]

	Prowess Data		Private Bank	
	(1)	(2)	(3)	(4)
DRT year = 1	0.760*	1.181	2.574**	3.871***
	(1.88)	(1.39)	(2.45)	(2.95)
DRT year = 2	0.00474	0.0246	1.252	3.503***
	(0.01)	(0.03)	(0.89)	(3.10)
DRT year ≥ 3	1.028	1.273	4.643**	6.063**
	(1.18)	(1.09)	(2.30)	(2.82)
Loan size			−0.000633	0.00291
			(−0.10)	(0.52)
Loan duration			−0.000998**	−0.000851**
			(−2.23)	(−2.14)
TimeDummy	Yes	Yes	Yes	Yes
Statetrend	Yes	Yes	Yes	Yes
Priority sector lending	No	Yes	No	Yes
R^2	0.00982	0.00721	0.188	0.238
N	16,049	14,875	1557	1490

[a] Standard errors are clustered at the state level. t statistics are given in parentheses. All regressions are run on data from 1992–2003. In columns 1 and 2, the dependent variable is the interest rates reported by firms in the Prowess data set and the data are at the firm-year level, and in columns 3 and 4, it is the interest rates on loans given by the private bank and the data are at the firm-quarter level. All regressions include firm fixed effects; also included are year fixed effects in columns 1 and 2, and quarter fixed effects in columns 3 and 4. DRT is an indicator variable that takes value 1 if a state had a DRT in the previous time period, and is 0 otherwise. "DRT year = t" is a dummy that equals 1 if DRTs were introduced t years ago. The dummy "DRT year ≥ 3" equals 1 if DRTs were introduced 3 or more years ago. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

years following DRT. In addition, there could be substitution effects operating on the supply side: since trained staff and local expertise are in short supply, and lending to large firms presumably require less staff input and expertise, banks might find lending to large firms relatively more profitable. They may thus expand lending to large firms at the expense of contracting credit to small firms.

7. ALTERNATIVE EXPLANATIONS

Our results have been consistent with the particular mechanism that we outlined in our model in Section 2. This mechanism involves general equilibrium effects operating through the credit market, that increased interest rates and had an adverse distributive impact on borrowing and fixed assets of firms. In this section we consider three alternative explanations for the observed patterns of effects and consider the evidence for and against them.

TABLE XVI

DIFFERENTIAL EFFECT OF DRT ON FIRMS OF DIFFERENT ASSET QUARTILE[a]

	Quartile 1		Quartile 4	
	(1)	(2)	(3)	(4)
DRT	−1.533*		18.53***	
	(−1.94)		(3.58)	
DRT year = 1		0.0189		9.317
		(0.03)		(1.50)
DRT year = 2		−0.876		22.71***
		(−1.03)		(4.51)
DRT year ≥ 3		−1.125**		47.06***
		(−2.55)		(4.92)
YearDummy	Yes	Yes	Yes	Yes
Statetrend	Yes	Yes	Yes	Yes
Quartile	1	1	4	4
Number of firms	265	265	421	421
R^2	0.336	0.336	0.0186	0.0195
N	1445	1445	3451	3451

[a]Standard errors are clustered at the state level. t statistics are given in parentheses. All regressions are run on data from 1992–2003. The dependent variable is new long-term borrowing (Borr.). Columns 1 and 2 report regressions where the sample is restricted to firms in the first quartile of 1990 tangible assets, and columns 3 and 4 report regressions where the sample is restricted to firms in the fourth quartile. All regressions include firm fixed effects, year fixed effects, and state trends. DRT is an indicator variable that takes value 1 if a DRT was operating in the state of the firms' headquarters at the end of the fiscal year and 0 otherwise. DRT is an indicator variable that takes value 1 if a state had a DRT in the previous time period, and is 0 otherwise. "DRT year = 1" is a dummy which equals 1 if DRT was introduced last year, "DRT year = 2" takes value 1 if DRT was introduced 2 years ago, and "DRT year ≥ 3" takes value 1 if DRT was introduced three or more years ago. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

7.1. *Lower DRT Effectiveness for Small Claims*

It could be argued that our observed effects are not a result of GE effects caused by inelastic credit supply, but are caused by the differential effectiveness of DRTs themselves. If DRTs were more effective at processing lenders' claims against large firms than claims against small firms, then lenders might take large firms to court more often than they take small firms. If small firms knew this, they might not respond to DRTs to the same extent. Alternatively, large firms may be more concerned about their future reputations, making them more susceptible to the threat of being taken to a DRT.

There are two reasons why this alternative explanation does not fit the facts. First, although this hypothesis can explain why access to credit increased disproportionately for large firms, it cannot explain why borrowing would decrease or the cost of borrowing would increase for small firms. Second, there is no evidence that DRTs were less effective at processing claims against small borrowers. In Table XVII, we use the repayment data from the private bank data set first used in Visaria (2009) to examine how borrowers' repayment rates

TABLE XVII

EFFECT ON DRTS ON FIRM REPAYMENT BEHAVIOR[a]

	(1)	(2)	(3)	(4)
Overdues	−0.197	−0.193	−0.342	−0.348
	(−1.192)	(−1.166)	(−1.520)	(−1.613)
DRT	−0.0351	−0.0306	−0.0355	−0.0426
	(−0.893)	(−0.766)	(−0.826)	(−0.903)
DRT × overdues	0.138***	0.136***	0.158**	0.159**
	(3.250)	(3.220)	(2.522)	(2.460)
Overdues × assets			0.00354	0.00330
			(1.401)	(1.397)
DRT × assets			9.25e-05	0.000179
			(0.613)	(1.107)
DRT × overdues × assets			−0.00131	−0.00104
			(−0.777)	(−0.618)
Observations	2090	2090	2090	2090
R^2	0.549	0.550	0.550	0.553

[a]The unit of observation is a firm year. The dependent variable takes value 1 if all invoices sent by the bank to the firm in a given year are repaid on time, and 0 otherwise. All columns include borrower fixed effects, quarter of average sanction and its square, average logged size of loan origination, and year dummies. In addition, columns 2–4 include borrowers' cash flow. Column 4 includes year dummies × size. Standard errors are clustered at the state level. t-statistics are given in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

responded to DRTs.[33] Since DRTs only applied to claims above Rs 1 million, the average DRT effect is the coefficient on the interaction term of DRTs and overdues, where overdues denotes the amount of payment that a firm owed the bank for more than 180 days. Overdues are measured 1 year before the DRT law was passed, to avoid endogenous changes in eligibility for DRTs. The triple interaction with firm size allows us to check whether the DRT effect was different for small and large firms. The table shows that DRTs significantly increased repayment rates for borrowers with large overdues. These effects do not vary with firm size: all the size interactions are insignificant. This remains true after controlling for different time-varying patterns by firm size in column 4.

Finally, Table XVIII shows additional evidence concerning DRT proceedings against overdue claims of varying size. We use the same random sample used by Visaria (2009) of 49 debt recovery suits filed in the DRTs of the state of Maharashtra by a private bank. Twenty-five of these cases had been filed in civil courts before DRTs were established and the remaining 24 had been filed in debt recovery tribunals. We run simple regressions to see if the time taken to

[33]The dependent variable is 1 if all invoices sent by the bank to the borrower were paid on time and is 0 otherwise. Hence these are linear probability regressions. The regressions control for borrower fixed effects, year dummies, average log of loan size (at the time of sanctioning), and average quarter (and its square) when the loans were sanctioned.

TABLE XVIII

EFFECT OF DRTs ON PROCESSING OF LEGAL SUITS, BY CLAIM SIZE[a]

	Time to					Probability of	
	Summons	First Hearing	First Evidence	File Closure	Interim Relief	Interim Relief	Award to Bank
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
DRT	−403.04*	−1117.02***	−1719.61***	−2828.67**	−425.08	−0.23	0.51**
	(213.00)	(347.54)	(429.57)	(1098.57)	(259.13)	(0.18)	(0.22)
Claim size	−0.18	−1.73**	−1.48	−2.28	−0.51	0.00	0.00
	(0.32)	(0.73)	(0.94)	(2.35)	(0.39)	(0.00)	(0.00)
DRT × claim size	0.07	1.60**	1.31	3.07	0.48	0.00	0.00
	(0.33)	(0.74)	(0.99)	(2.36)	(0.39)	(0.00)	(0.00)
N	49	47	16	20	26	49	17
R^2	0.11	0.31	0.51	0.68	0.13	0.06	0.22

[a]The unit of observation is a legal suit filed by the bank. DRT is an indicator variable that takes value 1 if the case was filed in a DRT and 0 if it was filed in a civil court. In columns 1–5 the dependent variable is the time taken to a particular event. In columns 6 and 7 the dependent variable is the probability of an event. Time to summons = date of summons − date of case filing; time to first hearing = date of first effective hearing − date of case filing; time to first evidence = date when applicant files evidence − summons date; time to interim relief = date when interim relief granted − date of case filing. Robust standard errors are given in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

process these cases varied by the venue where the case was filed and whether the size of the claim had a further differential effect. The table shows results for the time taken (in days) from the date of case filing to various milestones in the life of a court case, as well as the probability that the court makes certain rulings. Although the sample is small, the results show a large and highly significant decrease in the processing time in DRTs compared to civil courts. However, the size of the claim had almost no effect on the time taken, except in column 2, where first hearings appeared to take longer if the claim was larger (not smaller). In addition, we see no effect of DRTs, or claim size, on the probability that the court ruled to prevent the borrower from disposing of assets while the case is sub judice. DRTs were more likely to decide in favor of the bank than civil courts, but this effect did not vary with the size of the claim. Overall, it appears that within the type of cases filed in DRTs, their effectiveness did not vary significantly by claim size. Since all firms in our sample had assets large enough to potentially meet the claim threshold for processing a case in a DRT, we conclude that there is no evidence for the concern that DRTs were only effective at recovering overdues from the larger firms in our sample.

7.2. *GE Effects Through the Labor Market*

7.2.1. *The Biais–Mariotti Effect*

Another alternative explanation is that the GE effects operate not through the credit market as we theorize, but through the labor market, as argued by Biais and Mariotti (2006). In their model, large firms owned by wealthier entrepreneurs are less credit-constrained. As a result, stronger enforcement expands credit access disproportionately more for small firms. Firms owned by poor entrepreneurs who were previously excluded from the credit market can now start a business. This raises the demand for labor in the industry. With an upward-sloping supply of labor, the wage rate rises. At the same time, the supply of credit is assumed to be infinitely elastic, so there is no effect on the cost of capital. Credit access changes by very little for the large firms, but the wage rate rises and so the profits of incumbent large firms decrease. This can explain why contract enforcement institutions tend to be weak: large firms have an incentive to lobby against policies that strengthen them.

We argue that the Biais–Marriotti theory cannot explain our results on two grounds. One, their theory predicts that large firms shrink in size and profits, while small firms expand as a result of stronger enforcement of credit contracts. This is the opposite of our estimated distributive effects of DRTs.

Second, we can test their theory by looking at the effect on the wage bill, namely the product of the average wage rate in a firm and total employment. For any given firm, inferring movements in wage rates from movements in wage bills requires assumptions on the elasticity of labor demand. If labor demand is inelastic, wage bills and wage rates will move in the same direction. Given that medium and large firms operate primarily in the formal labor market and there are strong labor market regulations in India restricting the right of firms to dismiss workers, it is plausible that labor demand is inelastic (Aghion, Burgess, Redding, and Zilibotti (2008), Besley and Burgess (2004)). If the wage rate rose, firms' wage bills would rise and we would expect a positive average impact of DRTs on the wage bill.

Table XIX examines effects of DRTs on the wage bill, using the Prowess data. Column 1 shows a negative average effect which is statistically insignificant. Column 2 presents the linear specification with varying trend controls. Here again, the estimates of both intercept and slope effects are negative and statistically insignificant. Column 3 presents the corresponding effects on different quartiles. The point estimates are negative and insignificant, with the exception of the second quartile for which the effect is negative and significant at 5%. We therefore find no evidence of an expansion of the wage bill as a result of DRTs and find a significant contraction for the second quartile. If anything, the wage rate fell as a result of DRTs, opposite to the prediction in the Biais and Mariotti (2006).

7.2.2. *Labor Intensive Small Firms*

Although we rule out the Biais and Mariotti (2006) model as an explanation for our empirical results, we could also consider an alternative version of our model, where GE effects operate through the labor market. This model is developed in Appendix A.2. As we have seen, DRTs create a PE effect that expands credit access for large firms more than for small firms. Therefore, larger firms are more likely to expand employment. If the supply of labor is fixed, or nearly inelastic, wage rates will rise, which will tend to choke off some of this increase in employment for firms of all sizes. Hence, after incorporating the GE effect through the labor market, employment expands in large firms and contracts in small ones. For similar reasons, output expands in large firms and contracts in small ones.[34]

A similar property can be derived for the wage bill. This model therefore predicts an adverse (favorable) effect on output, employment, and wage bill for small (large) firms. In these respects, the model is consistent with the facts.

Yet there are two key facts that are not consistent with this model. First, it predicts that the wage rate should rise as a result of a DRT, whereas the evidence on the wage bill in Table XIX (just discussed) suggests otherwise. Second, in this explanation, capital assets should expand for all firms. This is because the size of capital assets is tied down entirely by the incentive constraints which are unaffected by the wage rate, and so there is no feedback

[34]Of course, the exact threshold between expansion and contraction of output is larger than for employment (since capital assets rise for all firms). The intuitive explanation is that small firms are more labor intensive and so are harder hit by a rise in the wage rate.

TABLE XIX

EFFECTS ON WAGEBILLS[a]

	(1)	(2)	(3)
DRT	−0.496	−0.263	−0.166
	(−1.37)	(−0.73)	(−1.67)
DRT*Tang.Ass.		−0.00698	
		(−1.32)	
DRT*quart = 2			−0.105
			(−0.74)
DRT*quart = 3			0.147
			(0.72)
DRT*quart = 4			−0.863
			(−0.86)
YearDummy	Yes	Yes	Yes
YearDum*SizeClass	No	No	Yes
Statetrend*SizeClass	No	No	Yes
YDumm*Size	No	Yes	No
Statetrend	Yes	Yes	No
Statetrendsize	No	Yes	No
DRT effect on quart. 1			−0.166
p-value quart. 1 effect			(0.109)
DRT effect on quart. 2			−0.271**
p-value quart. 2 effect			(0.0119)
DRT effect on quart. 3			−0.0192
p-value quart. 3 effect			(0.887)
DRT effect on quart. 4			−1.029
p-value quart. 4 effect			(0.313)
Number of firms	1683	1683	1683
$\tilde{W}$		−37.71	
R^2	0.109	0.701	0.240
N	16,605	16,605	16,605

[a] t statistics are given in parentheses. Standard errors are clustered at the state level. All regressions use borrower fixed effects. All regressions run on data from 1992–2003. The dependent variable is the wage bill. Tables V and IX provide a more detailed description of the setup. DRT is a variable that takes value 1 if the firm is subject to DRT in this year, and 0 otherwise. The first two regressions estimate the average impact of DRT on the wage bill. Year dummies control for year-specific shocks. Column 2 uses a linear specification and includes controls for nationwide year-specific distributive effects (year dummies and YDumm*Size). The last two columns report the results of a quartile regression. DRT*quart = j is the additional effect of DRT over and above the baseline effect for first quartile firms captured by the variable DRT. "DRT effect on quart. j" is the overall effect of DRTs for a firm of quartile j. "p-value quart. j effect" is the p-value from an F test with the null hypothesis that this effect is zero. All regressions are run with the appropriate state-specific time trends. The statistic $\tilde{W}$ reports the implied value of 1990 tangible assets such that the value of the dependent variable would be the same with and without DRT. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

effect from wage rates to capital assets. As shown in Appendix A.2, this model predicts that in the absence of GE effects within the credit market, capital assets expand for all firms and expand by more for larger firms. This runs against what we observed earlier: capital assets contracted for small firms, and interest rates rose.[35]

For the same reason, GE effects operating through any other factor market will not be able to explain the contracting of borrowing or fixed assets of small firms, or the rise in interest rates.

7.3. *Insurance and Incomplete Contracts*

An alternative explanation based on incomplete contracting may also explain adverse distributional effects on borrowing and capital assets. The incomplete contract channel is used in Gropp, Scholz, and White (1997) and Bolton and Rosenthal (2002), who stressed the insurance value of weak enforcement.[36]

The main idea is the following. If the debt contract is not state-contingent, interest obligations cannot be mutually adjusted in times of distress when the borrower cannot pay the nominal interest obligation. Subsequent transfers are dictated by the courts. If the court protects borrowers in these states, they obtain a measure of insurance against bad times. When credit contracts are enforced more strongly, this increases the burden of default costs for borrowers. Since the likelihood of such distress is greater for smaller firms, they face a relatively large increase in default costs, so they borrow less.

Large firms, on the other hand, may decide to borrow more for a number of reasons: they are less risk-averse than small firms, they are less likely to default, and they are able to obtain credit on cheaper terms, since lender risk is reduced.

This theory can, therefore, explain redistribution of credit when enforcement becomes stronger. However, contrary to our results, it predicts a decrease in interest rates. This is because, first, borrowers optimally choose to default less often as liquidation is more costly with stricter bankruptcy laws. This reduces the risk for lenders. Second, lenders can expropriate a larger fraction of firms' assets in case of default, and this also makes lending more profitable. This increases competition among lenders. In the absence of GE effects, there

[35]It could be counterargued that the preceding argument is based on the assumption that firms do not have to borrow to pay workers. A rise in the wage rate raises working capital needs, which may crowd out borrowing for fixed capital. This again requires wage bills to rise. But we have seen that the wage bill, borrowing, and fixed capital moved in the same downward direction for small firms, so the shrinkage in their capital assets cannot be explained by an expansion in their wage bill.

[36]Several other contributions emphasize this aspect of bankruptcy law (e.g., Livshits, MacGee, and Tertilt (2007), Chatterjee, Corbae, Nakajima, and Ríos-Rull (2007), and Vig (2007)). See also Perri (2008) for a discussion of limited enforcement constraints and the interaction with contingent versus noncontingent claims. Noncontingent claims are essentially the same as incomplete contracts, and the following discussion also applies to models with limited enforcement and noncontingent claims.

are no changes in the required rate of return, so the interest rate ought to go down. Therefore, this argument cannot explain why the interest rate went up, as we estimated in Tables X and XI.

8. CONCLUSION

In contrast to Visaria (2009), who only examined average effects on repayment rates and interest rates on loans affected by DRTs, we have examined the differential impact of DRTs on borrowers of different asset size. We have provided evidence that small firms in India experienced a contraction in credit and fixed assets, following a reform that strengthened banks' ability to enforce credit contracts. We explained this through GE effects in the credit markets caused by inelastic supply of loans. Interest rates appear to have increased significantly by 1.7–3.3 percentage points for all categories of firms, depending on the data and specification used. In addition, the reform increased borrowing, fixed assets, and profits of firms in the top quartile of the size distribution. The higher collateralizable assets owned by these large firms enabled them to expand their borrowing significantly, to an extent that outweighed the reduction caused by the rise in the cost of borrowing. On the other hand, small firms had insufficient collateral to permit a large expansion in credit access, so the higher interest rates resulted in less borrowing.

Our analysis considered both firm and bank behavior. We find that bank profits increased, which is evidence in favor of an upward-sloping supply of credit. Bank lending also increased, but only with a time lag after the reform. This is consistent with our hypothesized explanation for why credit supply is inelastic: in the short run, banks have a short supply of critical inputs—such as local knowledge, expertise, and long-term relationships with customers—that are needed to expand lending. This is perhaps also consistent with our finding that banks restructured their activities more toward metropolitan and urban areas. This provides additional evidence to support our hypothesis that general equilibrium effects working through the credit market are driving our findings. However, it is also possible that banks successfully restructured operations with the passage of time, the supply of credit gradually became more elastic, and the distributive effects were weakened.

Finally, we argued that our empirical findings cannot be explained by alternative channels alone, such as GE effects operating through labor or other factor markets, or the reduced insurance value of loan defaults that would arise in an incomplete contracting setup. While there may or may not have been GE effects on other factor or product markets, GE effects operating through the credit market are needed to explain the contraction or rising cost of borrowing resulting from DRTs.

Our empirical and theoretical results cast doubt on the general presumption that stronger lender collection rights or expanded scope for collateral will relax credit market imperfections for most borrowers, or that aggregate efficiency and output will necessarily increase. Lenders were generally made better off by the stronger credit enforcement, but a large fraction of borrowers were adversely impacted. If small firms have higher marginal returns to capital, this redistribution of credit may have an adverse macroeconomic impact.

In addition to the regressive distributive effect on credit, our theoretical analysis suggests an adverse impact on workers. However, a detailed empirical analysis of this phenomenon must await further research using richer data on wage rates and employment. Our data also do not allow us to examine effects on the entry of new firms. In India, the informal sector in manufacturing is very large, and our data do not include this sector. The "small" firms in the Prowess data are probably mid-sized in the entire distribution of firms across both formal and informal sectors. This makes it difficult for us to assess the effects of DRTs on the informal sector.

APPENDIX A: EXTENSION OF THE MODEL TO A MULTIFACTOR CONTEXT

A.1. *GE Effects Operating Through Credit Market Only*

The simplest setting involves a Leontief fixed coefficients technology with two factors: capital and labor. If labor employment can be varied flexibly, it will be adjusted to the amount of capital in the firm which will be set by the incentive constraints. Hence employment and the wage bill will move in the same direction as the level of borrowing or capital stock, with the latter being determined as specified above. So the wage bill will contract in small firms and expand in large firms, analogous to capital stock.

The same result obtains with a Cobb–Douglas specification of the production function,

$$\gamma = AK^{1-a}L^{a} = AK\kappa^{-a}, \tag{A.1}$$

where $a \in (0, 1)$. In this setting, firms select the level of capital intensity κ. We can phrase the firm's problem as choosing scale γ of operation and capital intensity κ, with factor inputs determined as $K = \gamma\frac{\kappa^{a}}{A}, L = \gamma\frac{\kappa^{a-1}}{A}$. Hence the incentive-constrained Walrasian demand maximizes

$$e[y_s f(\gamma) - T_s] + (1-e)[y_f(\gamma) - T_f] - w \cdot \gamma\frac{\kappa^{a-1}}{A} \tag{A.2}$$

subject to

$$T_k \leq \theta\left[W + \nu \cdot y_k f(\gamma) + (1-\delta)\gamma\frac{\kappa^{a}}{A}\right] + d \tag{A.3}$$

and the lenders participation constraint

$$eT_s + (1-e)T_f \geq (1+\pi)\gamma\frac{\kappa^{a}}{A}. \tag{A.4}$$

In equation (A.3) we now allow lenders to recover part of the fixed assets of the firm, in addition to a part of output and the entrepreneurs wealth. This can motivate firms to "overcapitalize" so as to relax the credit constraint.

To obtain closed-form solutions, we assume $\nu = 0$ and also that $f(\gamma) = \gamma^{\varepsilon}$ for $\varepsilon \in (0, 1)$. The key point to note is that the incentive constraint in equation (A.3) involves only the amount of fixed capital assets and is independent of the wage rate. In particular, if the firm is credit-constrained, this ties down the extent of capital assets as before:

$$K \equiv \gamma \frac{\kappa^a}{A} = \frac{\theta W + d}{1 + \pi - \theta(1-\delta)}. \tag{A.5}$$

This equation determines capital intensity as a function of the choice of scale as

$$\kappa = \left[\frac{A}{\gamma}\right]^{1/a} \left[\frac{\theta W + d}{1 + \pi - \theta(1-\delta)}\right]^{1/a} \tag{A.6}$$

and the scale γ is then chosen to maximize

$$\bar{y} f(\gamma) - \gamma \frac{\kappa^a}{A}\left[(1+\pi) - \frac{w}{\kappa}\right]. \tag{A.7}$$

Using equations (A.5) and (A.6), this reduces to

$$\bar{y} f(\gamma) - \frac{\theta W + d}{1 + \pi - \theta(1-\delta)}(1+\pi) - \left[\frac{\theta W + d}{1 + \pi - \theta(1-\delta)}\right]^{1-1/a} \left[\frac{\gamma}{A}\right]^{1/a} w. \tag{A.8}$$

Since a lies between 0 and 1, this is a concave problem, and the optimal scale is determined by the corresponding first-order condition. Using the constant-elasticity form of f, we obtain the expressions for scale and capital intensity:

$$\gamma = \left[\bar{y}\left\{\frac{\theta W + d}{1 + \pi - \theta(1-\delta)}\right\}^{1/a-1} \frac{aA^{1/a}}{w}\right]^{a/(1-a\varepsilon)}, \tag{A.9}$$

$$\kappa = \left[\frac{\theta W + d}{1 + \pi - \theta(1-\delta)}\right]^{(1-\varepsilon)/(1-a\varepsilon)} w^{1/(1-a\varepsilon)} (a\bar{y})^{-1/(1-a\varepsilon)} A^{-\varepsilon/(1-a\varepsilon)}. \tag{A.10}$$

It follows from these expressions that larger firms are more capital-intensive.[37]

[37]The reason is that larger firms are owned by wealthier entrepreneurs and are less credit-constrained. This result is not altogether trivial, as there is a countervailing force: smaller firms that are more credit-constrained are more undercapitalized, so they have a higher marginal rate of return on capital. One way to explain the result is the following. The binding incentive constraints determine the total capital assets the firm can have. Wealthier entrepreneurs have more capital assets. Given the capital assets available, the entrepreneurs then decide on employment. Firms with smaller capital assets compensate by employing more workers per unit of capital.

The capital asset of the firm is given by

$$K = \alpha(\theta) + \beta(\theta)W, \tag{A.11}$$

where now

$$\alpha(\theta) = \frac{d}{1 + \pi - \theta(1 - \delta)}, \quad \beta(\theta) = \frac{\theta}{1 + \pi - \theta(1 - \delta)}.$$

As DRTs cause θ to increase, the incentive-constrained demand for capital, hence credit, increases. This raises the profit rate π. What happens to $1 + \pi - \theta(1 - \delta)$? If it falls, every borrower demands more loans. If the supply of loans is sufficiently inelastic, this cannot happen. With a strong enough GE effect, then $1 + \pi - \theta(1 - \delta)$ must rise. This implies that α is decreasing in θ, and β must be increasing; otherwise every borrower will demand less credit, which is inconsistent with market clearing. It follows that we get the same equations for capital assets and borrowing as in the case where capital is the sole productive asset. The specification of the asset and borrowing regressions continues to be described by equation (5.4).

In addition, equation (A.10) generates a prediction for capital-intensity choices in each firm and how they are affected by DRT. We proceed on the assumption that the supply of labor is perfectly elastic, that is, there are no GE effects operating through the labor market. So w is fixed. The next section considers the case where the entire source of the GE effect is an upward-sloping supply of labor.

Equation (A.10) shows that an increase in θ causes capital intensity in each firm to move in the same direction as its capital assets. Hence if GE effects are strong enough, capital intensity falls in small firms and rises in large firms as a result of DRT.

We also obtain a similar property for wage bill $WB \equiv w \cdot L$:

$$WB_{jt} = C \cdot [\alpha(\theta_{jt}) + \beta(\theta_{jt})W_j]^{\varepsilon(1-a)/(1-a\varepsilon)} w_t^{-a\varepsilon/(1-a\varepsilon)}. \tag{A.12}$$

This is a nonlinear regression. However, it is clear that the wage bill moves in the same direction as capital assets, so it contracts in small firms and expands in large firms if GE effects are strong enough. A linear regression for wage bill therefore takes the same form as for capital assets.

A.2. *When the GE Effect Operates Only Through the Labor Market*

Now consider the case where the supply of credit is perfectly elastic, but the supply of labor is upward-sloping. We saw above that a firm's demand for labor is proportional to its capital assets, with the factor of proportionality decreasing in the wage rate:

$$L_{jt} = C_1 \left[\frac{\theta W + d}{1 + \pi - \theta(1 - \delta)} \right]^{\varepsilon(1-a)/(1-a\varepsilon)} w^{-1/(1-a\varepsilon)}. \tag{A.13}$$

In the absence of GE effects in the credit market, a rise in θ raises capital assets of all firms and thus raises labor demand. Then the wage rate rises. The net effect on firms' output, employment, or profits depends on the trade-off between rising capital assets and rising wage rates.

Let $w(\theta)$ denote the equilibrium wage rate, a rising function of θ. Then

$$L(\theta; W) = C_1[\alpha(\theta) + \beta(\theta)W]^{\varepsilon(1-a)} w(\theta)^{-1} \tag{A.14}$$

and

$$\gamma(\theta; W) = C_2\{[\alpha(\theta) + \beta(\theta)W]^{(1-a)/a} w(\theta)^{-1}\} \frac{1 - a\varepsilon}{a}. \tag{A.15}$$

Hence L is rising in θ if and only if

$$\varepsilon(1 - a)\frac{\alpha'(\theta) + \beta'(\theta)W}{\alpha(\theta) + \beta(\theta)W} > \frac{w'(\theta)}{w(\theta)}. \tag{A.16}$$

It is easily checked that the left-hand side of equation (A.16) is rising in W.[38] Therefore, larger firms are more likely to expand employment. If the supply of labor is fixed, or nearly inelastic, it follows that employment will expand in large firms and contract in small ones.

A similar argument establishes that output expands in large firms and contracts in small ones, though the exact threshold between expansion and contraction of output is larger than for employment (since capital assets rise for all firms). The intuitive explanation for this is that small firms are more labor intensive and so are harder hit by a rise in the wage rate.

A similar property can be derived for the wage bill. This model therefore predicts an adverse (favorable) effect on output, employment, and wage bill for small (large) firms.

APPENDIX B: ALTERNATIVE SPECIFICATIONS

B.1. *DRT Effects on Proportionate Changes in Borrowing*

So far, we have been working with the absolute value of firm borrowing instead of its logarithm. Using the logarithm of firm borrowing as the dependent variable in our main regressions changes the interpretation of the regression dramatically. Consider equation (2.11). If we take the logarithm of the credit limit, the GE effect represented by the expression for the profit rate π in the denominator no longer interacts with firm wealth. This leaves us with an interaction effect of the DRT variable θ and firm wealth W that depends only on the PE effect. Intuitively, the GE effect affects the borrowing of all firms by

[38]This requires $[\alpha + \beta W]\beta' - [\alpha' + \beta' W]\beta > 0$ or $\frac{\beta'}{\beta} > \frac{\alpha'}{\alpha}$. This is verified from the expressions for $\alpha(\theta)$ and $\beta(\theta)$ above.

the same proportion. Taking the logarithm of borrowing shifts attention away from absolute changes to borrowing toward proportionate changes. Since the GE effect does not vary with firm size, using logarithms does not allow us to capture the GE effect. For this reason, we use absolute volume of borrowing and capital stock as the dependent variables in all preceding regressions.

Nevertheless it could be argued that the "true" model is nonlinear, where the GE effect does not operate in the same proportion for all firms. To what extent are the empirical results robust when using logs of the concerned dependent variable? Table XX shows the quartile regression results for log values of

TABLE XX

MEASURING THE DEPENDENT VARIABLES IN LOGS[a]

	Logbor (1)	Logplama (2)	Logprof (3)
DRT	−0.410***	−0.0642*	−0.00305
	(−2.92)	(−1.89)	(−0.08)
DRT*quart = 2	0.545***	0.0575	−0.0708
	(3.21)	(1.29)	(−0.98)
DRT*quart = 3	0.311	0.104***	0.0146
	(1.60)	(2.84)	(0.23)
DRT*quart = 4	0.335**	0.0741**	0.154**
	(2.13)	(2.33)	(2.71)
YearDummy	Yes	Yes	Yes
YearDum*SizeClass	Yes	Yes	Yes
Statetrend*SizeClass	Yes	Yes	Yes
DRT effect on quart. 1	−0.410	−0.0642	−0.00305
p-value quart. 1 effect	(0.00827)	(0.0721)	(0.941)
DRT effect on quart. 2	0.135	−0.00672	−0.0739
p-value quart. 2 effect	(0.419)	(0.721)	(0.372)
DRT effect on quart. 3	−0.0991	0.0393	0.0115
p-value quart. 3 effect	(0.516)	(0.163)	(0.754)
DRT effect on quart. 4	−0.0748	0.00990	0.151
p-value quart. 4 effect	(0.468)	(0.578)	(0.000158)
Number of firms	1335	1674	1618
R^2	0.0884	0.563	0.264
N	7547	16,341	13,180

[a] t statistics are given in parentheses. Standard errors are clustered at the state level. All regressions use borrower fixed effects. All regressions run on data from 1992–2003. The dependent variables are the log values of new long-term borrowing (logbor), plants and machinery (logplama), and profits (logprof), respectively. DRT is an indicator variable that takes value 1 if a DRT was operating in the state of the firms' headquarters at the end of the fiscal year and is 0 otherwise. DRT*quart = j is the additional effect of DRT over and above the baseline effect for the first quartile firms, captured by the variable DRT. "DRT effect on quart. j" estimates the overall effect of DRTs for a firm of class j, and "p-value quart. j effect" reports the p-value from an F test with the null hypothesis that this effect is zero. All regressions are run with quartile-specific time-varying time trends (interactions of the quartile dummies and the linear time-varying state trend). * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

TABLE XXI

BORROWER LEVEL CLUSTERING: AVERAGE EFFECTS BY QUARTILE[a]

	Borr.	PlaMa	Profits	Intrate
DRT	−1.533**	−2.247**	−0.224	2.326*
	(−2.53)	(−2.05)	(−0.74)	(1.77)
DRT*quart = 2	2.932***	1.020	0.354	−2.704*
	(3.26)	(0.69)	(0.51)	(−1.69)
DRT*quart = 3	−0.167	1.052	1.287	−1.530
	(−0.13)	(0.44)	(1.14)	(−1.00)
DRT*quart = 4	20.06	43.78	14.73	−0.544
	(1.50)	(1.14)	(1.63)	(−0.30)
YearDummy	Yes	Yes	Yes	Yes
YearDum*SizeClass	Yes	Yes	Yes	Yes
Statetrend*SizeClass	Yes	Yes	Yes	Yes
DRT effect on quart. 1	−1.533	−2.247	−0.224	2.326
p-value quart. 1 effect	(0.0116)	(0.0402)	(0.459)	(0.0775)
DRT effect on quart. 2	1.399	−1.227	0.129	−0.378
p-value quart. 2 effect	(0.0349)	(0.213)	(0.837)	(0.678)
DRT effect on quart. 3	−1.700	−1.195	1.062	0.796
p-value quart. 3 effect	(0.123)	(0.574)	(0.327)	(0.304)
DRT effect on quart. 4	18.53	41.54	14.50	1.782
p-value quart. 4 effect	(0.165)	(0.278)	(0.108)	(0.141)
Number of firms				1671
Number of groups	1406	1683	1683	
R^2	0.0189	0.0633	0.0491	0.0210
N	9762	16,605	16,605	16,049

[a] t statistics are given in parentheses. Standard errors are clustered at the borrower level. All regressions use borrower fixed effects. All regressions run on data from 1992–2003. The dependent variables are new long-term borrowing (Borr.), plants and machinery (PlaMa), profits, and interest rates (intpay), respectively. DRT is an indicator variable that takes value 1 if a DRT was operating in the state of the firms' headquarters at the end of the fiscal year and is 0 otherwise. DRT*quart $= j$ is the additional effect of DRT over and above the baseline effect for the first quartile firms, captured by the variable DRT. "DRT effect on quart. j" estimates the overall effect of DRTs for a firm of class j, and "p-value quart. j effect" reports the p-value from an F test with the null hypothesis that this effect is zero. All regressions are run with quartile-specific time-varying time trends (interactions of the quartile dummies and the linear time-varying state trend). * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

borrowing, plant and machinery, and profits. We continue to find a significant contraction in borrowing and capital stock for the first quartile, while on the other quartiles, the effects are insignificant. In the case of log profits, we find a significant increase for the fourth quartile. Thus nonproportionate GE effects are not driving our results.

B.2. *Controlling for Serial Correlation*

Finally, one could be concerned that our estimates are affected by serial correlation. While serial correlation cannot explain the sign and magnitude of our estimates, it could bias standard errors downward and thus invalidate the inference about statistical significance. In all regressions, we correct for serial correlation by clustering standard errors at the state level. As Bertrand, Duflo, and Mullainathan (2004) reported from Monte Carlo studies, this procedure performs reasonably well with 20 states (see Table VIII in their paper), and we have 23 states in the current context. As an additional check, Table XXI in Appendix 2 shows how the standard errors of the quartile regressions are affected by clustering at the individual borrower level. We find here that the negative effect on the first quartile becomes even more significant, whereas the positive effect on the fourth quartile no longer survives.

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