The Distributive Impact of Reforms in Credit Enforcement: Evidence from Indian Debt Recovery Tribunals*

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Abstract

It is generally presumed that strengthening the legal enforcement of lender rights increases credit access for all borrowers, by expanding the set of incentive compatible loan contracts. This presumption is based on an implicit assumption of infinitely elastic supply of loans. With inelastic supply, strengthening enforcement generates general equilibrium effects which may reduce credit access for small borrowers, while expanding it for wealthy borrowers. In a firm-level panel, we find evidence of such adverse distributional impacts caused by an Indian judicial reform in the 1990s which increased banks' ability to recover non-performing loans.

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1 Introduction

When it comes to bankruptcy law and the enforcement of credit contracts, economists' views often diverge from popular opinion. Laws or institutions that weaken the rights of lenders to appropriate the secured assets of defaulting borrowers are popularly justified on grounds of distributional fairness. Economists, on the other hand, generally believe that by weak lenders rights have *ex ante* incentive effects that hamper the functioning of credit markets. If lenders cannot seize collateral when borrowers default, borrowers cannot credibly commit to repay their loans. As a consequence, lending involves a high level of risk, causing lenders to charge high interest rates and lowering access to credit, particularly for poor borrowers. The economists' argument is that apart from impairing the efficiency of credit markets, weak enforcement ends up hurting the poor by limiting their access to credit and raising its cost. More generally, it is argued that strong enforcement enlarges the effective set of contracts available, allowing a Pareto improvement.

There is some empirical evidence supporting the economists' view. Porta *et al.* (1997; 1998) show that across countries, weak investor protection is correlated with thinner debt markets. Gropp *et al.* (1997) find that in US states with higher bankruptcy exemption limits (i.e., lower borrower liabilities), poor borrowers were more likely to be excluded from credit markets. In a developing country context, Visaria (2009) uses a micro-panel of loans to study the impact of a judicial reform that improved credit contract enforcement across different states of India. Using the state-time variation in the establishment of the new debt recovery tribunals (DRTs), she finds that stricter enforcement of lender's rights in the event of default significantly improved repayment behavior of delinquent borrowers. Further, interest rates increases were smaller for new loans that were more likely to face DRTs.

With the exception of Gropp *et al.* (1997), most exisiting empirical work on the subject examines average effects rather than their distribution across heterogenous borrowers. In contrast, this paper analyzes the distributional impact of strengthening credit enforcement. We argue that the traditional theoretical argument for stronger enforcement overlooks its potential general equilibrium effects. When the supply of credit is inelastic, stronger enforcement of lender rights can raise interest rates and reallocate credit from poor to wealthy borrowers. This provides a rationale for the popular belief that stronger enforcement hurts poor borrowers, while benefiting lenders and wealthy borrowers, even when *ex ante* incentive effects are incorporated. On the other hand, the standard economists' argument is valid when the supply of credit is infinitely elastic. Which view is correct depends on how elastic the supply of credit is, which is essentially an empirical matter. Our model also shows that the macroeconomic and efficiency impacts of strengthening contract enforcement are ambiguous in general, and 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 on banking relationships and lender-borrower proximity emphasizes, to overcome asymmetric information lenders need specialized expertise in screening and monitoring borrowers, local knowledge and long term lender-borrower relationships (Fisman *et al.* 2010, Gordon & Bovenberg 1996, James & Smith 2000, Petersen & Rajan 1995). To expand lending operations, banks must employ more loan officers, develop expertise, relationships and investment in bank infrastructure, all of which takes time and involves significant adjustment costs. Banks need to reorganize their operations as the relative profitability of serving different client bases changes. At least in the short run, therefore, 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, but it may still fail to be perfectly elastic if there is an underlying scarcity of lending personnel with the requisite skills and local information.

The general equilibrium effect that results from such inelastic credit supply (studied previously in Lilienfeld-Toal & Mookherjee (2008) can be explained as follows.[1] By increasing the likelihood that collateral will be seized, the reform that we study improves the credit-worthiness of borrowers, and shifts outwards the incentive-constrained demand function for credit. We call this the partial equilibrium (PE) effect. Also, since the reform operates on the borrower's collateral, the PE effect is (proportionately) greater for wealthier borrowers. However, because the supply of credit is inelastic, not all of this increased demand can be met at the existing rate of interest. The result is an increase in the interest rate and in profits earned by lenders. This general equilibrium (GE) effect reduces credit, and does so uniformly regardless of borrower wealth. 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. For wealthier borrowers the larger PE effects can overwhelm the GE effect, leading to a net increase in their access to credit. For the poorest borrowers, the GE effect can overwhelms any PE effects, and so they may have less credit. As a result, if the supply of credit is sufficiently inelastic, the reform in enforcement may result in a redistribution of credit. In general, the redistribution may be progressive or regressive, depending on parameter values and the size distribution of firms, and is thus ultimately an empirical matter.[2]

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 increased long-term borrowing and fixed

[1] Further detail about the microfoundation of this model is provided in that paper. The Walrasian model we use here is shown there to characterize stable contract allocations in a market where lenders and borrowers are matched. There are, however, two important differences between the underlying models in the two papers. First, the current model is simpler because it abstracts from *ex ante* moral hazard for borrowers. Second, the focus of that paper was on the effects of reforms in personal bankruptcy law, particularly those involving borrower exemption limits (as in US law). Changes in exemption limits are fundamentally different from changes in enforcement: a lowering of exemption limits results in an equal absolute increase in borrower liability, whereas strengthening enforcement results in a higher increase in liability for wealthier borrowers. Therefore, lowering the exemption limit increases relative credit access for poor borrowers, whereas strengthening enforcement *a la* DRTs reduces it.

[2] 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 this case, the largest firms borrow less due to higher interest rates, while the mid-sized firms borrow more due to the relaxation of their credit constraints.

assets for large borrowers, but decreased these for small borrowers, consistent with the general equilibrium effects postulated by our theoretical model. Importantly, we also find a significant rise in interest rates on new loans for all categories of borrowers, which, as we will discuss later, allows us to discriminate between our hypothesis and alternative explanations.

The empirical analysis uses Prowess, a firm-level panel dataset collected by the Centre for Monitoring the Indian Economy (CMIE).[3] The dataset contains detailed information on both financial and real variables for this firm, which allows us to examine the effect of the reform on firms' borrowing, fixed assets, profits and wage bill. Consistent with our hypothesized general equilibrium effects, we find that the reform was associated with reduced borrowing for the bottom quartile of firms (organized by pre-DRT tangible asset size), and increased borrowing for the top quartile. We find parallel effects on plant and machinery assets. Effects on profits are similar but these estimates are not always statistically significant. There was a significant rise in interest rates on fresh borrowing for all categories of borrowers. All of these results are robust to controls for borrower fixed effects, size-specific year dummies, state-specific time trends as well as state-specific credit targets for small firms set by the Reserve Bank of India.

We find corroborating evidence from the credit supply side, using data on lending by Indian banks from the Reserve Bank of India (RBI database on the Indian Economy). Consistent with the idea that the reform increased the aggregate demand for bank credit, bank lending increased in states after the establishment of DRTs. Our theory predicts that DRTs increased the relative profitability of lending to corporate clients (who have more tangible assets), compared to that of lending to small firms, consumers and farmers. In line with this, we find a reallocation of lending away from rural areas, to urban and metropolitan areas (which is where most businesses would borrow). This shift towards lending 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 banks did indeed slow the growth of branches, particularly rural branches, after DRTs were set up. Consistent with the hypothesis of inelasticity of the supply curve, profits rose significantly in response to DRT for banks with greater presence (measured by number of branches) 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 as time since DRT establishment increased. This gradual increase in the supply of loans raises the question whether the GE effects tended to diminish over time, and whether they disappeared eventually. Since the GE effect is dominant for small firms, we examine how effects on the interest rate and

[3]The advantage of using Prowess rather than the same dataset as Visaria (2009) used, is that it includes all publicly listed Indian firms, their borrowing from all sources rather than a single one, as well as various measures of firm performance such as fixed assets, secured borrowing, profits, and salary costs. Note however that the results we find in this paper are similar to those we find in her dataset. We do present some results in this paper based on that data set, for interest rates and repayment rates on individual loans, since information at the level of individual loans is not available in Prowess.

borrowing by small firms varied with time since the introduction of DRTs. While interest rates rose the year immediately following a DRT, this increase was moderated the year after. However it continued to be positive and significant even three years later, showing no tendency to disappear. The same is true for the effects on credit and capital contraction of firms in the first quartile. Thus we find no evidence that the adverse distributive effects were short-lived.

We subsequently examine competing explanations for these distributional effects of DRTs. First, as discussed, we explain the observed regressive effects through inelastic credit supply. However, any factor of production (such as labor) with inelastic supply can also generate general equilibrium effects with distributive consequences. For example, if labor were scarce, then the increased demand for credit and increased production caused by DRTs would raise the wage rate. If small firms were relatively more labor intensive, a rise in the wage rate would cause their output and profits to contract. For large firms, the relaxation of the credit constraint can outweigh the rise in the wage rate, to yield an expansion in output and profit.[4] However, this cannot explain the patterns we observe for the wage bill, borrowing and capital stocks. We find a negative effect on the wage bill for all quartiles, which is statistically significant for the second quartile. This suggests that wage rates fell as a result of DRTs, i.e., their adverse effects extended to workers.[5] Also, we shall argue that this hypothesis also fails to explain the redistribution of capital assets and borrowing levels.

A second explanation for our observed redistribution of borrowing and capital assets is that strengthening lender rights reduces the insurance value of default (Gropp *et al.* 1997, Bolton & Rosenthal 2002, Perri 2007, Vig 2007). In other words, when their projects fail, small risk-averse borrowers might default on their debt to limit their losses. When default becomes more costly they would then lower their *ex ante* demand for credit. In contrast to our model, this approach is based on the assumption that loan contracts are incomplete, with interest payments that are not state-contingent.[6] However, we argue that this cannot be an explanation for our findings. This is because the insurance argument would imply that strong enforcement reduces the risk borne by lenders, which should lead to a fall in interest rates. In contrast, as noted above, interest rates rose after DRTs were established.[7]

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, 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 Indian judicial

[4]This is a modified version of the model in Biais & Mariotti (2006). In their model, an upward sloping supply curve of labor causes the wage rate to increase, but this causes small firms to benefit and expand, while large firms contract.

[5]Labor market regulations in India make it difficult for Indian firms in the formal sector to lay off workers. So a fall in wage bill is unlikely to arise from sharply reduced employment following a rise in the wage rate.

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

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

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 empirical results. In section 7 we use further empirical analysis to differentiate between competing hypotheses using the empirical results. Finally, Section 8 concludes.

2 The Model

Our approach, based on Lilienfeld-Toal & Mookherjee (2008), differs from conventional models of competitive credit markets. We treat the price of credit to be 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, that 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 the 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 c.d.f. G over support $[\underline{\Omega}, \bar{\Omega}]$. Each borrower seeks to invest in a project of size $\gamma \geq 0$. This requires upfront 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 upfront investments. In contrast to Lilienfeld-Toal & 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

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

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.[8] 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 or nominal 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 nominal obligation, and this is followed by

[8]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.

a mutually-agreed-upon adjustment of the borrower's obligation in accordance with the 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.[9] 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 shall treat ν as a parameter and assume that $\nu < I/(\bar{y} \cdot \theta \cdot f'(b'))$. 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 shall 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

$$(1 - \theta) \cdot [W + \nu \cdot y_k \cdot f(\gamma)] + (1 - \nu) \cdot y_k \cdot f(\gamma) - d$$

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: e.g., $d = \underline{d} + \delta \cdot W$, 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[10]

[9] 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 either be 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.

[10] Here we are abstracting from liquidity constraints that may prevent the borrower from repaying. To ensure that the borrower can make payments we would need the condition $T_k \leq W + y_k f(\gamma)$, i.e., the *ex post* return from the project is sufficient to cover the repayment amount. This constraint will not bind once (IC_k) holds, provided $d < (1 - \theta)W + (1 - \theta\nu)y_k f(\gamma)$, i.e., if d is not too large relative to W. For sufficiently small W, however, the liquidity constraint may bind. In that case (IC_k) will not bind and increasing

$$T_k \leq \theta[W + \nu \cdot y_k f(\gamma)] + d. \qquad (IC_k)$$

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 affect the incentive constraints.[11]

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.[12] We assume that for there to be some supply of credit, lenders must be assured a return that is at least as large as a nonnegative lower bound α i.e., $L_s = 0$ if $\pi < \alpha$ and $L_s > 0$ if $\pi > \alpha$. To avoid a vacuous analysis, assume that $\bar{y} \cdot f(b)/b > I(1+\alpha)$, i.e. 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 shall 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

$$\max_{\gamma}[\bar{y} f(\gamma) - \gamma I(1+\pi)], \qquad (FB)$$

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.

[11] 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.

[12] Consider the following micro-foundation 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 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, $L_s(\pi) \equiv \int_0^{\pi} L(c)dH(c)$.

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 (IC). 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*

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

subject to

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

and

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

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 the IC and PC constraints, it becomes clear that a project size γ is implementable if and only if

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

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

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

The assumption that $\nu < I/(\bar{y} \cdot \theta \cdot f'(b'))$ implies that the right-hand-side of (IC") 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 which solves the equality version of (IC"). We shall denote this project size ceiling by $\gamma^H(W,\theta,\pi)$. It is increasing in W, θ, and decreasing in π.

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

Definition 2 *First best asset threshold: $W^F(\pi) \equiv \{\gamma I(1+\pi) - d\}/\theta - \nu\bar{y}f(\gamma^F)$.*

Maximum project size: $\gamma^H(W,\theta,\pi)$ 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 will operate and gain 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 larger than W_L, those with sufficiently high wealth (we call this the first-best asset threshold, W^F) will 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 3 *The incentive-constrained demand function for credit is*

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

2.4 Market Equilibrium

Next, we solve for the market equilibrium in order 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 4 *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 & 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.[13]

Since market demand changes with θ, the equilibrium profit rate π^* will be a function of θ and will be 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 will obtain larger loans, and thus attain higher payoffs. Those who were

[13] 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 will cause the gross rate of return π on lending to be equal across all active lenders.

not constrained will be unaffected. Lenders will be unaffected as well. The result is a Pareto improvement. The distributional impact is favorable, since poorer borrowers gain access to credit. Borrowers are better off because every contract implementable under weak enforcement is also implementable under strong enforcement.[14] This justifies the conventional intuition that stronger enforcement institutions are uniformly beneficial for borrowers.

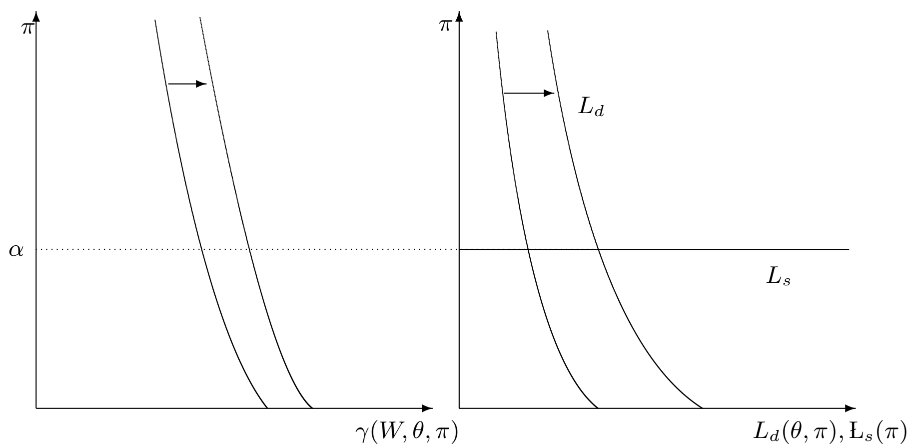

Figure 1: Impact of Strengthening Enforcement When There Are No GE Effects.

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 will shift the aggregate credit demand function outwards, and thus raise the equilibrium profit rate. This GE effect will choke off some demand, in order to clear the credit market. The total effect on credit allocation will now be 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:

Proposition 5 *Consider an increase in θ from $\underline{\theta}$ to $\bar{\theta} > \underline{\theta}$. Suppose the elasticity of the credit supply function at any $\pi > \alpha$ is finite but bounded below by some $\underline{\epsilon}$. If $\underline{\epsilon}$ is sufficiently large:*

[14] This is a fairly general result. In particular, this result will hold 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.

1. *the proportion of firms excluded from the market falls (i.e., the minimum asset threshold W_L falls)*
2. *the first-best project scale (and hence credit allocated to sufficiently wealthy borrowers) falls, and*
3. *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 $\underline{\epsilon}$ is sufficiently large.[15] A sufficiently small rise in the profit rate will imply 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) will outweigh the increase in the minimum viable project scale, thus reducing exclusion, and increasing in the credit ceiling for all active borrowers. However, the first-best project size will decline 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, 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.[16] 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

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

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:

$$\Delta(W) \equiv \gamma^H(W; \pi'; \theta') - \gamma^H(W; \pi; \theta) \geq 0$$

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

[16] 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. On the other hand if we model reputation costs as increasing linearly in W at the rate δ, 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.

then it must rise (and will be bigger) for all higher wealth borrowers with higher wealth $W' > W$, i.e., $\Delta(W') > \Delta(W) \geq 0$.[17]

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. For borrowers with wealth above $\widehat{W}$ credit expands, and for borrowers with wealth below $\widehat{W}$, credit contracts. 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 fact that incentive constraints are binding in both states, and lenders have to be paid π in expectation, it is easily checked that the interest rate so defined can be expressed as

$$r = \pi + \theta \frac{\nu}{I}(y_s - \bar{y})\frac{f(\gamma)}{\gamma}. \tag{2}$$

Note that with $\nu = 0$, the interest rate does not vary across firms. Neither do interest payments vary with the state of the world.[18] More realistically, with $\nu > 0$, firms with a higher average rate of return to capital assets $\frac{f(\gamma)}{\gamma}$ will be charged a higher interest rate. In that case interest rates will vary across borrowers, and interest payments will vary across states of nature for any given borrower.[19]

With a strong enough GE effect, the establishment of DRTs will raise π, lower the scale γ for small firms and raise it for large firms. If the production function is concave over the relevant range (i.e., if $\gamma > b'$), (2) implies interest rates will rise for small firms, but the effect for large firms is ambiguous. The same is true if DRTs raise

[17]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.

[18]The same is true if we extend the theory to suppose that lenders can recover part of the fixed assets financed. Let us suppose 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 will not vary with the state $T_s = T_f = T$, and the interest factor will equal $1 + r = \frac{T}{\gamma \cdot I} = 1 + \pi$ for all firms.

[19]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 will earn a lower average rate of return. We therefore expect to see them obtain loans with a lower interest rate.

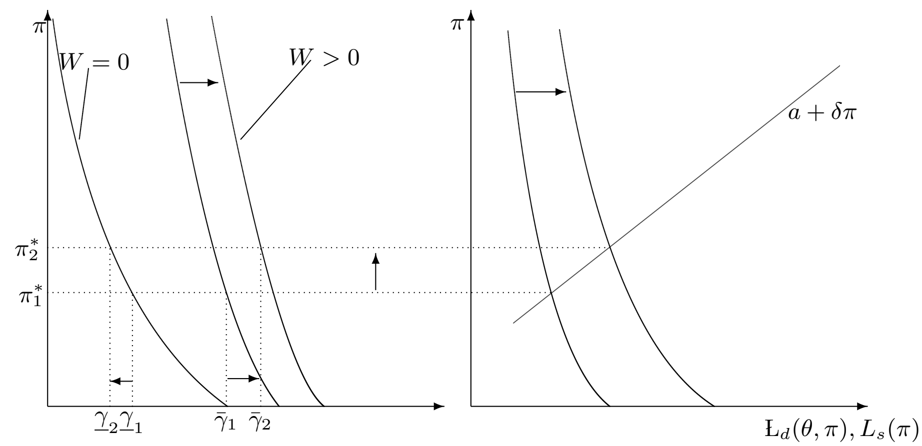

Figure 2: Impact of Strengthening Enforcement with GE Effects.

ν by making it more difficult for entrepreneurs to strip assets when they default on loans.

We summarize the preceding discussion as follows.

Proposition 6 *Suppose $\bar{\Omega}$ the upper bound of the wealth distribution is smaller 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:*

(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 $W < \widehat{W}$.

In the cases covered by this Proposition, we have a regressive redistribution of credit among the set of credit-constrained borrowers. The intuition underlying this result is depicted in Figure 2. On the right side of the figure, we see that aggregate demand increases which implies an increase in the equilibrium profit rate π^*. However, firms with different assets are differently affected, which is mirrored in the left side of Figure 2. The leftmost demand function corresponds to a firm with the smallest possible assets, i.e., $W = 0$. For a firm with zero assets, changing θ does not lead to a change in the demand for credit. Hence for such firms, the same function represents demand for credit, both before and after the reform.

In contrast, individual demand for firms with $W > 0$ shifts outward. The middle and rightward demand functions in the left side of Figure 2 represent incentive compatible demand for the same firm with assets $W > 0$, both before and after the change of θ. Demand before the change corresponds to the demand function in the middle of the left side of Figure 2. This demand function is then shifted outward to the rightmost demand function. This asymmetric effect has the following implication for changes in equilibrium demand: The firm with $W = 0$ cannot benefit from the increase in θ and its demand curve is not shifted outward. At the same time, the firm now faces a higher profit rate that its lenders must earn. As a result, its demand for credit decreases from $\underline{\gamma}_1$ to $\underline{\gamma}_2$.

The firm with $W > 0$ benefits from the increase in θ, and so its demand curve is shifted outward. It also faces a higher interest rate which potentially reduces demand. However, the outward shift of the demand curve dominates the interest rate effect and demand increases from $\bar{\gamma}_1$ to $\bar{\gamma}_2$. Hence, small firms receive less credit and large firms receive more credit due to the change in θ.

More generally, the distributive effects can take many different forms, depending on the size of the GE effects and the firm size distribution. Our results are summarized in Figure 3. Here we allow for the possibility that the support of the wealth distribution of firms is wide enough that the largest firms are not credit constrained.

Panel A depicts the redistributive effect of increasing θ in the absence of any GE effects. Then, π-incentive compatible demand shifts outward for all borrowers who operate at their credit ceiling. Some excluded borrowers can now also participate and exclusion is reduced. Here we have a Pareto improvement. The profit rate is unaffected, and if $\nu = 0$, then so is the interest rate. 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 (depicted in panel B), all credit-constrained borrowers receive more credit: exclusion is reduced and the credit ceiling is shifted outward. The effect on small firms is qualitatively similar to the case in 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. This is in contrast with the effects on large enough firms who work near or at the first best project scale. Since they only barely credit-rationed to start with, 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 (depicted in 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 things: the strength of the GE effects, as well as the firm-size distribution. If the upper bound to firm size is large enough there will be firms at the very top who will 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 distributive impact is ultimately an empirical matter. For this reason we turn to the empirical evidence from a contract enforcement reform in India.

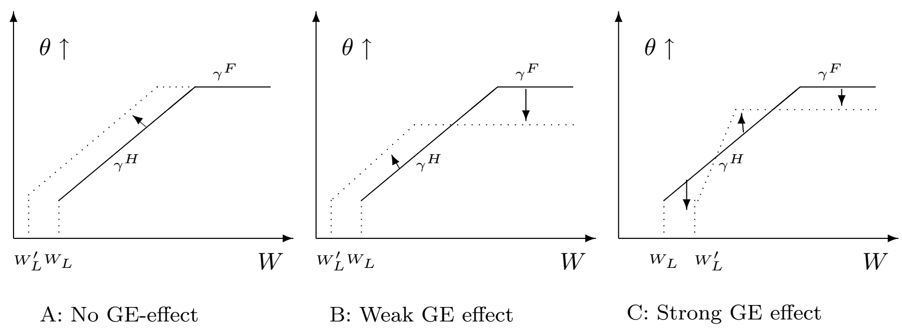

Figure 3: Impact of Strengthening Enforcement with GE Effects.

3 The Empirical Context: Indian Debt Recovery Tribunals

We test our model's predictions by examining the effects of a judicial reform affecting credit contract enforcement, carried out in India in the 1990s. In the wake of the financial sector liberalization of the early 1990s, India's central bank introduced new rules requiring commercial banks to reduce their volume of non-performing loans and improve their financial health. Since it was widely agreed that the inefficient civil judicial system slowed down banks' recovery of bad loans, in 1993 the Government of India passed a national law establishing new specialized tribunals for debt recovery.

The 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 of the order of Rs 25). Before this law, all debt recovery suits were tried in civil courts. In these courts, these cases were processed according to the Code for Civil Procedure, and it was common for cases to continue for extremely long. Nearly 40 percent of the pending debt recovery cases in civil courts in 1985-86 had been pending for longer than 8 years (Government of India 1988). In contrast, DRTs follow a new streamlined procedure. Defendants are given less time to respond to summons, they must provide a written defense, and they can only make counter-claims 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. Thus, the DRT law reflects a change in the procedure for processing debt recovery suits. However, substantive laws governing the cases did not change;

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 in lowering case processing times. Visaria (2009) analyzes data from a small random sample of debt recovery suits to show 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 were no significant differences in the improvement for small versus large claims.[20] Therefore, we interpret the introduction of a DRT in a state as an increase in the parameter θ in our model which affected all borrowers uniformly.

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 0 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, Table 1 presents Cox hazard rate regressions, where time to DRT adoption is regressed on state-level economic, judicial and political variables. Time-invariant regressors include total bank credit during 1990-92 [21], average assets and profits of firms (in the Prowess dataset, described further below) located in the state between 1990-92. Time-varying explanatory variables include level and growth rate of state-level GDP, per capita credit disbursed by commercial banks, the share of small-scale industries 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 help predict the timing of DRT adoption.

However, we may still worry that state-level unobservable factors affecting firm outcomes were confounded 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 will examine the robustness of our results to controls for state and size-specific time trends, and state-year targets for lending to

[20] This evidence is presented in Table 14, and will be discussed in greater detail in section 7.

[21] This variable is the average (over 1990-1992) of the credit extended by commercial banks in the state in question, as reported in the Reserve Bank of India's *Banking Statistics*.

small firms set by the Reserve Bank of India. In addition, we will allow annual trends to vary at the industry level, and show that our results are not explained simply by the secular growth or contraction of particular industries.

4 Data

For our main regressions, we use a firm-level panel data set drawn from the Prowess database 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 Indian organized manufacturing sector. We use data for 1683 firms spanning all major industry groups, with the largest concentration in manufacturing. The database 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. We exclude companies from the financial sector (since they are more likely to be net lenders rather than borrowers) and public (government-owned) companies not subject to commercial norms or incentives.[22]

This dataset differs from that used by Visaria (2009), which consists of detailed loan records obtained from a large private bank in India (hereafter referred to as the *private bank dataset*). The advantages of the Prowess data over the private-bank dataset are the following: (a) Prowess includes all listed firms in India rather than only those that borrow from a particular bank; (b) the data on loans covers loans from all sources, not just from a single bank; (c) we can examine effects on various measures of firm performance apart from borrowing; and (d) we avoid some attrition problems in the private-bank dataset, where information on loan accounts closed before the year 2000 are not available. However, the private-bank dataset has the advantage of including rich information at the loan-level on both repayment and interest rates. The Prowess dataset does not have information on loan repayment, and so to examine the effect of DRTs on loan repayment we rely on the private bank dataset alone. Also, in the Prowess data we can only compute interest rates averaged across all loans outstanding in any given year. Therefore the Prowess data does not allow us to isolate the DRT effect only on new loans (which is what our theory pertains to). The private bank dataset reports the exact variable of interest: interest rates on individual loans. For this reason we shall present results for interest rates using both the Prowess dataset and the private bank dataset.

Since we are interested in the differential impact of the reform on firms of different sizes, we use the firm's reported tangible assets (in the Prowess data) as a proxy of size.[23] To avoid a simultaneity problem 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 that were small or large before the reform was announced. This also restricts us to look at firms

[22]The Prowess data on banking firms will be used in section 6.6 we will examine evidence from the supply side of the credit market.
[23]We use tangible assets since lenders are likely to use these as collateral.

that existed in 1990; post-1990 entry effects are ignored. We thus run regressions on a sample of approximately 16600 observations spanning the period 1992-2003 for 1683 firms.[24]

Descriptive statistics from our sample are reported in Tables 2a and 2b. All variables are adjusted for inflation, using the all-India wholesale price index for 2002. Table 2a provides means, standard deviation and range of the main variables used in the analysis for the entire sample, while Table 2b 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 2a shows substantial growth between 1990 and the sample period in borrowing, plants and machinery, profits and wage bill, reflecting the increased growth rate of the Indian economy starting in the mid-1990s.

Table 2b 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%, 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 Rs 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 16-17% for all four quartiles.

Note that the DRT law only applies to debt recovery claims larger 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 dataset were liable to DRT with regard to the size of their aggregate debt. Later we shall present evidence that DRTs appear to have been just as effective at improving repayment for small firms as for large firms.

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. Accordingly, we assign firms to DRT jurisdictions on the basis of their registered office address. The DRT variable is a categorical variable at the state-year level, which takes value one in years when the jurisdiction had a DRT in place.

We now explain the regression specification used, and how it relates to the theory developed in the previous Section. As we shall 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, it turns out that $\nu = 0$ does not produce interesting predictions for interest rates. Hence the 'true' specification corresponds to $\nu > 0$, which generates to a nonlinear borrowing regression. We will therefore present both linear and non-linear regressions for all variables of interest.

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

We presume that the key element of heterogeneity of firms is the retained 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, i.e., the distribution of W has an upper end-point which falls below the level at which the first-best can be attained. We also ignore issues of entry, since we are working with a database that excludes both very small formal-sector firms and all firms in the informal sector. By restricting attention to firms that existed in 1990 we avoid picking up any DRT effects on firm entry. Thus, we shall assume a given set of firms with varying W, all of whom are active but credit-constrained.

5.1 Borrowing and Capital Stock

In the baseline model developed in the previous section, capital is the sole factor of production. As a result, firm size can be represented interchangeably by output $f(\gamma)$ or capital stock (γ). We focus on the latter. In a static setting capital stock is proportional to borrowing, so we can use γ to represent either capital stock or borrowing of the firm.

Consider the simple case where $\nu = 0$. Given the restriction on the wealth distribution mentioned above, we obtain a simple linear equation for capital stock in terms of entrepreneurial wealth:

$$\gamma = \alpha(\theta) + \beta(\theta)W \tag{3}$$

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. We know that $\pi(\theta)$ is non-decreasing (if supply is infinitely elastic then π is constant, and if supply is inelastic it is increasing). Hence $\alpha(\theta)$ is non-increasing. Moreover $\beta(\theta)$ must be non-decreasing (otherwise 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 (3) directly is that W is unobserved. We therefore proceed on the following assumptions which enable W to be proxied by the firm's historical (year 1990) assets: (i) entrepreneurs' wealth has not changed between 1990 and year $t > 1990$, or can be proxied by wealth in 1990; (ii) all states had the same pre-DRT θ, denoted by $\bar{\theta}$; (iii) once a state gets a DRT, its θ changes to $\bar{\theta} + \mu$ where $\mu > 0$.[25]

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

$$\bar{\gamma}_j = \alpha(\bar{\theta}) + \beta(\bar{\theta})W_j \tag{4}$$

which implies

$$W_j = \frac{\bar{\gamma}_j - \alpha(\bar{\theta})}{\beta(\bar{\theta})} \tag{5}$$

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

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

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

implying (using (5)):

$$\gamma_{jt} = \bar{\gamma}_j + \phi \cdot DRT_{jt} + \psi \cdot (DRT_{jt} \times \bar{\gamma}_j) \tag{6}$$

where

$$\phi \equiv \alpha(\bar{\theta} + \mu) - \alpha(\bar{\theta})[1 + \frac{\beta(\bar{\theta} + \mu)}{\beta(\bar{\theta})}] < 0, \psi \equiv \frac{\beta(\bar{\theta} + \mu) - \beta(\bar{\theta})}{\beta(\bar{\theta})} > 0. \tag{7}$$

5.2 Interest Rates

Corresponding to (2), 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

$$r_{jt} = r_0 + \rho \cdot DRT_{jt} + [\bar{\theta} + \chi \cdot DRT_{jt}] \times g(\gamma_{jt}) \tag{8}$$

where $\rho, \chi > 0$. Substituting for γ_{jt} from (6), and using the fact that $DRT_{jt}^2 = DRT_{jt}$ since it is a 0-1 variable, we get:

$$r_{jt} = \rho_0 + \rho_1\bar{\gamma}_j + \rho_2 DRT_{jt} + \rho_3(DRT_{jt} \times \bar{\gamma}_j) \tag{9}$$

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

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.[26]

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 firms of a given size (i.e., corresponding to a given wealth W of entrepreneurs) 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 firm with wealth W with enforcement parameter θ. Hence

$$\frac{\partial \Pi}{\partial \theta} = [\bar{y}f'(\gamma(\theta;W) - I(1 + \pi(\theta))]\frac{\partial \gamma(\theta;W)}{\partial \theta} - \gamma(\theta;W)I\frac{\partial \pi(\theta)}{\partial \theta} \tag{10}$$

[26] It is easy to incorporate the possibility of some firms operating on the non-concave part of the production function by extending the linear specification of the average rate of return to a quadratic specification, which would give rise to an additional quadratic term in firm size on the right-hand-side of (9).

For credit-constrained firms the term in square brackets on the right-hand-side of (10) is positive. For small firms whose borrowing declines as a result of DRT, the effect on profit is thus unambiguously negative, because credit becomes tighter and its cost increases. For large firms the effect is ambiguous. The specification of the profit regression will be analogous to the specifications for borrowing and interest rates, since the effect of the DRT variable will vary with the size of the firm. We expect the intercept effect to be negative.[27] The effect on the slope with respect to firm size is theoretically ambiguous. But if the positive effect on borrowing dominates then the slope effect would be positive.

To examine the effect on the wage bill, we must extend the model to a multi-factor context. This extension is developed in Appendix 1. The analysis there 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.[28]

6 Empirical Results

6.1 Linear Specification

Tables 3 and 4 report firm-level regressions for borrowing, plants and machinery, and profits (before depreciation and taxes). All specifications include a DRT dummy that captures whether the state in which 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, and any other national level time-variation in the dependent variables. All regressions are run using borrower fixed effects, and 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, which controls for serial correlation in the error terms over time within states (Bertrand *et al.* 2004).

Columns 1-4 in Table 3 show regressions of DRT impact at the state level on firm-level borrowing. In column 1 we run a regression of borrowing on the DRT variable alone. The estimated coefficient tells us that the 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 firm size (measured by 1990 tangible assets), as in the simple linear specification in (6). We also include size-specific year dummies, which control for any year-to-year changes in the national economic environment that may also have

[27] 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 at different parts of the size distribution.

[28] However, as we shall discuss in section 7, in an alternative version of the theory the GE effects arise in the labor rather than the credit market. That model is also developed in Appendix 1, and will be used to evaluate alternative explanations for our observed results.

had distributive effects on borrowing. It is important to note that in this way we control for the economy-wide deregulation and other market-friendly reforms in the 1990s that might have affected firms of different sizes differently. Next, we add controls for state-specific time trends. This addresses the concern that states that were more favorable to big 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 significant negative intercept effect and a positive slope effect on borrowing, as predicted by our model.

Columns 5-8 in Table 3 show analogous results for plants and machinery, and columns 9-12 show results for profits. In all cases, the threshold firm size (denoted $\tilde{W}$ in the table) below which the effect is negative varies between Rs 191 to 264 million, which corresponds roughly to the third quartile of the size distribution (in Table 2b). Hence the linear specification indicates that the bottom three quartiles did not experience any expansion in borrowing, fixed assets or profits.

In Table 4 we investigate the robustness of our results to additional controls. Whereas in Table 3 we had only controlled for year effects that were linear in size (Table 3: columns 3, 7, 11), we now control for time-varying patterns more flexibly by interacting the year dummies with size class dummies (Table 4: 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 4 we instead address the concern that our results are driven by changes in national policies for small-scale industries. For instance, if the Indian central bank relaxed its rules about lending to the priority sector (agriculture, artisan and village industry, and small-scale industries) in different states to coincide with DRT adoption then one could argue that this confounds our results. 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 there was no expansion in borrowing for the bottom three quartiles of firms.

Finally, columns 4, 8 and 12 address the concern that different industries may have faced different economic environments over time, and that DRT roll-out coincided with a growth period for large-scale industries and a contraction for small-scale industries. We introduce controls for year-industry dummies, with industries classified at the two-digit NIC levels. The results turn out to be 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 also state-level controls for credit policy preferences towards small firms. We thus have strong evidence that DRTs caused a negative distributional effect on borrowing, plants & machinery and profits. However, for our claim to be valid, it is important that the "parallel trends" condition is satisfied. In other words, if this negative effect existed even before DRTs were established, then we would worry that credit to small firms was already contracting relative to large firms in the states that adopted DRTs earlier. To examine this issue, we use data for the time-period 1988-1993 and examine if the time trends in the key variables were different for early versus late adopters of DRTs.

Results for borrowing are presented in columns 1-4 of Table 5a. In fact, contrary to our concern, Column 1 in Table 5a shows that the trend in borrowing between 1988 and 1993 was slower in the early adopters of DRT (states that received a DRT in 1994) than in late adopters (those that received DRTs in 1996 or later). Column 2 examines how the pre-trends varied by firm size. We find that in the pre-DRT period the borrowing of small firms was increasing faster in early adopting states than in the late adopting states. Hence early adoption was associated with a higher (resp. lower) trend growth for borrowing of small (resp. large) firms. This is exactly the opposite of what happened after DRTs were set up: they expanded the borrowing of large firms relative to those of small firms. Hence the results on the effects of DRT cannot be attributed to pre-existing trends. Columns 3 and 4 in Table 5a refine the analysis by replacing the early adoption dummy with the number of years since DRTs were set up. In each observation, we now put in a continuous measure for the number of years that a state had a DRT during 1993-2002. States that receive DRTs earlier have higher 'DRT years'. If DRTs were established sooner in states where small firms' credit was contracting and large firms' credit was expanding, then we might expect 'DRT years' to have a negative intercept and a positive slope effect. Instead, we find a higher trend rate for small firms and lower trend rate for large firms before 1993. Columns 5 through 8 carry out the same analysis for plants and machinery, and find no significant difference in pre-93 trends between early and late adopters.

Table 5b 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. However this is the opposite of the post-93 DRT effects, where (as we will see in 6.3) the profits of large firms increased substantially. Hence our results cannot be attributed to pre-existing 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 that the linear functional form is correct only under restrictive conditions: e.g., 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 DRTs were equally effective across firms of differing sizes (θ does not vary across size). Even under these conditions, the linear specification does not apply for profits when the production function displays diminishing 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.

Therefore, in Table 6 we turn to a specification where the DRT effect can be estimated separately for firms in different quartiles of the firm-size distribution. As controls we include year dummies interacted with size class and state-specific time trends. The effect on borrowing is seen to be negative and significant at 10% for the first quartile, negative and significant at 5% for the third quartile, positive and significant at the 1% level for the second and fourth quartiles. For plants and machinery we see a negative and significant effect (at 10% level) for the first quartile, negative and significant effect (at 5% level) for the second quartile, negative and insignificant effect for the third quartile, and positive and significant (at 1% level) for the fourth quartile. In the case of profits, the effects are insignificant for the bottom three quartiles, and positive and significant (at 1%) for the top quartile. Thus, after allowing for non-linearities, 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 largest quartile of firms.

6.4 Effects on Interest Rates

Next we examine possible channels through which these effects may have come about. Our theory is based on an upward sloping supply of credit; however, as we noted previously, upward sloping supply of any input factor could give us the same result. We start by examining the interest rate: in Table 7a we present effects on the interest rate using the Prowess data. As we noted earlier, the Prowess dataset does not allow us to separate interest on new loans and old loans. Therefore our dependent variable is interest rate obligations on total debt. However, our theoretical predictions are for interest rates on new loans, and so in Table 7b we examine the robustness of our results in the private-bank dataset.

Column 1 in Table 7a shows that on average DRTs raised interest rates 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 (9)) 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, but the interaction effect is insignificant, indicating that the interest rate effect does not vary by firm size.

Column 3 shows the effects on different quartiles. Here the effects for the bottom three quartiles are not precisely estimated (though the point estimate for the bottom quartile effect is a 2.3 percentage point rise, which has a p-value of .103). The effect on the top quartile is significant at 5%, with a point estimate of 1.8 percentage points. 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 which is precise only for the top quartile.

One possible reason for imprecision of the non-parametric estimates is that the dependent variable data measures the average interest rate on both old and new loans, while the predicted effect of DRTs is only on new loans. (Recall from Table 2 that new loans comprise one-third of all long-term borrowing). For this reason we turn to the private-bank dataset used by Visaria (2009). This dataset has the feature that all loans fully paid off by the year 2000 were 'retired' from the database by the bank. To avoid attrition bias, we restrict attention to loans of duration 8 years or longer that originated in 1992 or later. Loans in this category would not have been retired from the database because their minimum duration would not have ended by 2000.[29] The data are organized at the borrower-quarter level, i.e., we know the total sum of loan originations and disbursements, average interest rate charged on all new loans, and subsequent repayments by each borrower in each quarter of the year.

Table 7b shows the interest rate results from the private bank dataset. In all specifications we obtain a significant rise in the interest rate. Column 1 shows a rise in the interest rate on new loans (averaging across all loans of 8 years duration or longer) by 1.8 percentage points as a result of DRT adoption in the state in question, after controlling for borrower fixed effects and quarter dummies. Column 2 finds a bigger effect (1.9 % points) after controlling for loan size and duration. The estimate gets bigger (2.3% points) in columns 3 and 4, where we introduce interactions with firm size (measured by 1990 fixed assets). As in Table 7a, the effect does not vary significantly with firm size. Columns 5 through 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.

Some rough calculations based on these estimates imply that the demand for long term borrowing and assets with respect to interest rates is very high. Interest rate elasticity of long term borrowing is approximately -4.1 for the lowest quartile and approximately -5.2 for the lowest 10% 1990 asset class of firms. In contrast, the elasticity of plants and machinery with respect to the interest rate is -1.5 for the lowest quartile and -2 for the lowest 10% of firms. [30]

One interpretation of these elasticities could be that these small firms are driven out of the market entirely. Banks focus their business more on the larger firms and as a result, these firms lose access to long term credit altogether. The fact that the impact on their assets is less drastic (even though it is still very high) may imply that they find other ways of financing to make up for the lack of long term credit.[31]

[29]We do not expect loans to be retired from the database before their stated duration is over, because pre-payment of loans is an extremely rare event in the private bank dataset.

[30]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 effect of DRT on credit and plants and machinery is taken from running the quartile regression as in table 12 or the appropriate version for deciles. For the interest rate effects, we use the implied change in interest rate from the private bank database running specification (6) in table 7b while restricting to the smaller half of firms in that dataset. The interest rate increase for these firms is approximately 4.3%.

[31]It may also be that some of the additional channels we consider below, such as incomplete contracting channel or general equilibrium effects stemming from other input factors, may be active particularly strongly for these firms and may reinforce the

6.5 Alternative Specifications of the Regression Equations

The simple version of the model with $\nu = 0$ generated equation (1) for credit limits which indicates that the correct specification for the borrowing regression involves the absolute volume of borrowing rather than its log. Taking logs of the credit limit in (1) would leave an interaction effect between the DRT variable θ and firm wealth W which represents only the PE effect, while the GE effect represented by the denominator of (1) would no longer interact with firm wealth. Hence the interpretation of the interaction between firm size and DRT changes dramatically if we measure borrowing in logs. Intuitively, the GE effect represented by the expression for the profit rate π in the denominator of (1), operates to change the credit of all firms by the same proportion. Hence in examining the effect on proportional rather than absolute changes of credit, the GE effect no longer varies with firm size. This is the rationale for our use of absolute volume of borrowing and capital stock as the dependent variable in all preceding regressions.

Nevertheless it could be argued that the 'true' model is nonlinear, where the GE effect does not operate equi-proportionately for all firms. To what extent are the empirical results robust when using logs of the concerned dependent variable? Table 16 in Appendix 2 shows the quartile regression results for log values of 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 non-proportionate GE effects are not driving our results.

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 downwards 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 *et al.* (2004) report 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 17 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 effects on the first quartile become even more significant, while those for the fourth quartile no longer survive.

6.6 Evidence for Low Elasticity of Credit Supply

The elasticity of credit supply is an important variable in this paper. As we explained in 2, whether credit is inelastic or not is essentially an empirical question. Inelastic credit supply generates certain predictions for borrowing, interest rates, firm assets and firm profits in our model, and as we have seen, these predictions are supported by the data. However, it is important to also seek corroborating evidence from the credit market on the elasticity of credit supply. We attempt this in this section.

We hypothesize that in order to lend, banks need information, expertise, experience and relationships with

(negative) impact of DRT on these firms.

clients. Although DRTs increase the profitability of lending, it is difficult for banks to expanding lending operations quickly because they face substantial adjustment costs in increasing these inputs. Further, the DRT reform 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 8 provides evidence from both borrowers and lenders sides on how the response to DRTs varies over time. Data on the supply of bank credit is drawn from the Database on the Indian Economy published by the Reserve Bank of India (RBI). A data series called "Quarterly Statistics on Deposits & Credit of Scheduled Commercial Banks" provides information on outstanding credit and the number of branches of all banks at various regional levels (state and city vs. country side). The data are reported on a quarterly basis. The first four columns of table 8 pertain to new long-term borrowing of firms from the Prowess data, while the remaining four columns concern effects on total outstanding bank credit at the state level from the RBI data. For different sets of controls (for state trends and for RBI state-year-specific priority sector lending targets), the table provides both the average (across all firms) of the post-DRT effect, as well as the average effect of the reform one year, two years and three or more years after DRTs were set up, respectively. For the firm-level borrowing data, we see the borrowing levels rise progressively with the passage of time following DRT. In the case of bank credit, the supply effects increase over time but are imprecisely estimated as we see a significant increase in only the last specification after three years and longer. The lack of precision may be partly due to the fact that these data are for total outstanding credit rather than new credit, and include the effects of DRTs on repayment of past loans which would tend to lower the total outstanding credit, offsetting positive effects on new credit) as well. Moreover, these regressions exclude credit provided by non-bank financial institutions.

Table 9 provides evidence of reorganization of banking operations in response to DRT, based on data from the RBI publication. We see a decrease in both the numbers of city (urban and metropolitan areas) and rural branches as a result of DRT. The number of city branches falls immediately in the year following DRT adoption. The number of rural branches shows an insignificant response the year following DRT reform, but in subsequent years experience progressively larger and more significant reductions. At the same time, city credit expands, particularly three 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 two 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.

However, expansion of bank credit *per se* does not shed evidence on the supply elasticity of credit. We would need to show that bank profits also rose as a result of DRT. As we have already seen, interest rates rose due to DRTs. In 7.1 we will see evidence of higher repayment rates by firms. Both factors would be consistent with higher bank profits. However we now examine this question directly by asking if exposure to DRTs caused banks profits to increase. We run regressions of the following general form:

$$y_{it} = \beta_0 + \beta_1 \sum_{j=1}^{J} (\text{DRT}_{jt} \times \text{Presence of bank i in state j}) + X_{it} + \epsilon_{it} \tag{11}$$

To calculate bank i's presence in state j, we use data on the number of branches of each bank in each state, obtained from the Reserve Bank of India's "Directory of Bank Offices". To avoid endogeneity concerns, we only consider branches opened before 1992. Also since banks are likely to lend to corporate clients primarily at city branches, we exclude rural branches when calculating this variable. Bank dummies ensure that our results are not being driven by scale effects from the number of total bank branches of the aggregate size of the bank's lending operations. Bank profit data come from two alternative sources: Prowess data on banks (used in columns 4-6 of Table 10) and the Reserve Bank of India's "Annual Banking Statistics" (used in columns 1-3.).[32] Using either measure, we see a significant positive effect of exposure to DRTs on bank profits. This is true for small banks (number of branches below the median) as well as large banks (number of branches above the median). 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 lag, because banks were slow to reorganize their lending operations towards large urban clients. However, at the same time, clients may also have been slow to formulate new project plans and prepare fresh loan applications. The time pattern of effects on the interest rate would depend 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 11 provides estimates of the interest rate responses corresponding to different durations since DRT reform, using both Prowess data (for interest on outstanding loans, both new and old), as well as the private bank dataset. As in the case of the average post-DRT effect, the Prowess interest rate effects

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

are imprecisely estimated because old and new loans are clubbed together. The private bank dataset 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.

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 12 shows lagged effects of DRT on new long term borrowing of the first and fourth quartiles of the firm distribution, using Prowess data. Consistent with the evidence on the lagged effects on the interest rate, the effects on borrowing levels are more pronounced three or more years following DRT.

7 Alternative Explanations

The evidence in the preceding section was consistent with significant GE effects operating through the credit market, which raised 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.

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 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 small firms. If small firms knew this, they may 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 we do not believe this alternative explanation fits 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 13, we use the repayment data from the private bank dataset first used in Visaria (2009) to examine how borrowers' repayment rates 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. The triple interaction with firm size allows us to check if the DRT effect was different for small and large firms. The table shows that DRTs significantly increased repayment rates for

[33] The dependent variable is 1 if all invoices sent by the bank to the borrower were paid on time, and 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), average quarter (and its square) when the loans were sanctioned.

borrowers with large overdues. These effects do not vary with firm size: all the size interactions are insignificant. This remains true after controlling different time-varying patterns by firm size, in Column 4.

Finally, Table 14 shows additional evidence concerning DRT proceedings against overdue claims of varying size. We use a random sample of 49 debt recovery suits filed in the DRTs of the state of Maharashtra by this 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 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 when summons were issued, the time taken to the first hearing, first filing of evidence, file closure and when interim relief was granted (if granted). In addition it shows the probability that the court passed a verdict in the bank's favor, and the probability that interim relief was granted.

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 granted interim relief to the bank (which is a ruling preventing the borrower from disposing of assets while the matter is sub-judice). We see that DRTs had a higher probability of the court deciding in favor of the bank, but this effect did not vary with the size of the claim. This shows that among the type of cases filed in DRTs, their effectiveness did not vary significantly by claim size. Hence we see no evidence favoring the hypothesis that DRTs were effective in recovering overdues only from large firms.

7.2 GE Effects Through the Labor Market

7.2.1 The Biais & Mariotti (2006) 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 & Mariotti (2006). In their model, large firms owned by wealthier entrepreneurs are less credit-constrained. Stronger enforcement therefore expands credit access disproportionately more for small firms. Firms owned by poor entrepreneurs who were previously excluded from the credit market can then enter. 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. Since credit access changes only slightly for the large firms, but the wage rate rises, the profits of incumbent large firms decrease. This can explain why contract enforcement institutions are not always strong: large firms have an incentive to apply political pressure to prevent the strengthening of contract enforcement institutions.

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 what we observe the effects of DRT to have been.

Second, we can test their theory by looking at the effect on the wage rate. Although the Prowess data does not include wage rates or employment levels, we have firm-year level data on the wage bill. Wage bill is 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 (see Aghion et al (2005) and Besley and Burgess (2004)), it is plausible that labor demand is inelastic. 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 15 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 estimate 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 whom 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 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 & Mariotti (2006).

7.2.2 Labor Intensive Small Firms

Although we rule out the Biais & 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 1. As we have seen, in our model DRTs expand 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 will expand in large firms and contract in small ones. For similar reasons, output will expand in large firms and contract 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

[34] Of course, the exact threshold between expansion and contraction of output will be 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.

should rise as a result of a DRT, whereas the evidence in Table 15 concerning the average impact on firm wage bills suggests otherwise. Second, in this explanation, capital assets should expand for all firms. This is because the capital assets are tied down entirely by the incentive constraints which are unaffected by the wage rate, and so there is no feedback effect from wage rates to capital assets. As shown in the Appendix, this model predicts that in the absence of GE effects within the credit market, capital assets will expand for all firms, and by more for larger firms. This runs against what we have 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 potentially also explain adverse distributional effects on borrowing and capital assets. The incomplete contract channel is used in Gropp et al (1997) and Bolton and Rosenthal (2002), who stress 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 the default costs, and 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 are no changes in the required rate of return, and so the interest rate ought to go down. Therefore this argument cannot explain

[35] It could be counter-argued that the preceding argument is based on the assumption that firms do not have to borrow in order to pay workers. A rise in the wage rate will raise working capital needs, which may have crowded out borrowing for fixed capital. This would again require 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 et al. (2007), Chatterjee et al.(2007) and also Vig (2007). See also Perri (2007) for a discussion of limited enforcement constraints and the interaction with contingent vs. non-contingent claims. Non-contingent claims are essentially the same as incomplete contracts and the following discussion also applies to models with limited enforcement and non-contingent claims.

why the interest rate went up, as we estimated in Tables 7a and 7b.

8 Conclusion

In contrast with 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 category of firms, depending on the data-set 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, and so the higher interest rates resulted in less borrowing.

The analysis looked at both firm behavior and bank behavior. We find that bank lending increased but only after some time has elapsed since the reform. We also find that the reform increased bank profits and caused banks to restructure their activities more towards 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.

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. While lenders are generally better off due to an increase in credit enforcement, 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.

Our results also suggest an adverse impact on workers, but a detailed analysis of this must await further research based on richer data on wage rates and employment. Our data also do not allow us examine effects on the entry of new firms. In India the informal sector in manufacturing is very large, and the Prowess dataset does not include a large fraction of 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. It is difficult for us to assess the effects of

DRTs on the informal sector.

In future research, we plan to examine cross-state spillovers in credit caused by the DRT reform. This would provide insights into how private capital flows across regions and divergence in growth rates can be explained by differences in contract enforcement institutions.

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Table 0: Dates of DRT Establishment

City of DRT (1)	Date of est. (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 1: Survival Analysis of DRT Adoption

	(1)	(2)	(3)	(4)	(5)	(6)
non time varying						
Bank credit (1990-92 avg)	-0.000			-0.000		
	(-0.604)			(-0.939)		
Firm assets (1990-92 avg)		-0.384			0.848	
		(-0.392)			(0.649)	
Firm profits (1990-92 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)
Growthrate 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

t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A Cox proportional hazards model is fitted to the time taken to establish a DRT in a state. As indicated, explanatory variables include the 1990-92 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 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.

Table 2a: Summary Statistics all firms.

	All years		Years with DRT changes		1990	
	mean/sd	min/max	mean/sd	min/max	mean/sd	min/max
Tang.Ass.	26.0	0.0044/1342.1	26.0	0.0044/1342.1	26.0	0.0044/1342.1
	(73.7)		(73.7)		(73.7)	
borr.	20.0	0/15717.9	14.5	0/2482.0	1.25	0/45.9
	(212.4)		(77.4)		(4.38)	
TotalLtermb	62.2	-0.0000010/19420.8	54.6	-0.00000080/9863.7	8.26	0/503.0
	(345.8)		(246.9)		(27.2)	
PlaMa	109.4	0/45769	85.3	0/14126	12.0	0/783
	(678.5)		(365.7)		(40.4)	
profits	18.6	-1189.6/8243.8	15.9	-224.3/2258.5	2.42	-14.4/162.8
	(132.7)		(73.2)		(8.48)	
intrate	16.6	0/480.5	16.9	0/480.5	15.3	0/466.7
	(18.4)		(17.0)		(16.9)	
wagebill	13.6	0/1526.2	11.4	0/928.0	2.38	0/156.5
	(42.9)		(32.8)		(6.37)	
t	1997.2	1992/2003	1996.4	1994/1999	1990	1990/1990
	(3.46)		(1.72)		(0)	
Observations	16602		8409		1741	

Standard deviation in brackets.

The variable Tang.Ass. denotes tangible assets measured in 1990. Furthermore, borr. denotes borrowing and is new long term borrowing over the last fiscal year and TotalLtermborr is the stock of long term borrowing. Profits are profits before depreciation and tax provisioning, interest rate is defined as interest expenses over totalborrowings, and the wage bill is total compensation to employees and includes wages and salaries, gratuities, contributions to private pension funds, etc. 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 RS. crores and adjusted with the 2002 wholesale priceindex.

Table 2b: Summary Statistics by quartile.

	Quart 1		Quart 2		Quart 3		Quart 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	0/15717.9
	(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	0/19420.8
	(22.6)		(34.9)		(70.6)		(629.3)	
PlaMa	8.04	0/413	20.2	0/768	45.6	0/2101	320.7	0/45769
	(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	16.9	0/433.3	16.5	0/425	16.0	0/440	16.9	0/480.5
	(17.9)		(16.7)		(17.2)		(20.9)	
wagebill	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	

Standard deviation in brackets.

The variable Tang.Ass. denotes tangible assets measured in 1990. Furthermore, borr. denotes borrowing and is new long term borrowing over the last fiscal year and TotalLtermborr is the stock of long term borrowing. Profits are profits before depreciation and tax provisioning, interest rate is defined as interest expenses over totalborrowings, and the wage bill is total compensation to employees and includes wages and salaries, gratuities, contributions to private pension funds, etc. 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 RS. crores and adjusted with the 2002 wholesale priceindex.

Table 3: The effect of DRT on borrowing, assets, and profits: Main specification.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	borr.	borr.	borr.	borr.	PlaMa	PlaMa	PlaMa	PlaMa	profits	profits	profits	profits
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
r2	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	16605	16605	16605	16605	16605	16605	16605	16605

t statistics in parentheses

Standard errors are clustered at the state- level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run from 1992-2003. All regressions include borrower fixed effects. The dependent variables are new longterm borrowing (borr.), plants & machinery (PlaMa), and profits, respectively. DRT is an indicator function which is one if a DRT was operating in the state of the firms' headquarter at the end of the fiscal year and zero 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 regressions which estimates the average impact of DRT and includes year dummies to control for year specific nation wide 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.

Table 4: Robustness wrt Size-Specific Time Trends, state level lending, and industry shocks.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	borr.	borr.	borr.	borr.	PlaMa	PlaMa	PlaMa	PlaMa	profits	profits	profits	profits
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
r2	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	16605	16605	15344	16605	16605	16605	15344	16605

t statistics in parentheses

Standard errors are clustered at the - level. All regressions use borrower fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run from 1992-2003. The dependent variables are new longterm borrowing (borr.), plants & machinery (PlaMa), and profits, respectively. DRT is an indicator function which is one if a DRT was operating in the state of the firms' headquarter at the end of the fiscal year and zero otherwise. DRT*Tang.Ass is a multiplicative interaction of DRT with tangible assets defined in 1990. This table reruns our main specification which tests for a distributive impact of DRT by adding the following 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 accordingly. YDum*SizeClass creates a dummy variable which is one for a specific year and a specific size class and zero 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 tangibles assets. Regressions which have State-log lend as controls include control for the level of credit (in logs) of agriculture, artisan & village industry, small scale industry, and total credit given by the State Bank of India, nationalized banks, and all scheduled commercial banks which totals to 12 variables which vary by year and within each year by state. Finally, 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 for which the level of the dependent variable would be the same with and without DRT.

Table 5a: Pre-trends.

	(1) borr.	(2) borr.	(3) borr.	(4) borr.	(5) PlaMa	(6) PlaMa	(7) PlaMa	(8) PlaMa
t	4.109***	-1.649***	4.791***	-2.816***	4.926***	-1.343***	5.509***	-1.873***
	(5.18)	(-5.47)	(6.16)	(-14.14)	(9.90)	(-3.68)	(8.65)	(-4.13)
early*t	-1.534***	1.847***			0.307	0.332		
	(-3.07)	(5.19)			(0.39)	(0.64)		
t*Tang.ass.		0.0541***		0.0828***		0.194***		0.200***
		(50.75)		(10.18)		(31.51)		(8.60)
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
r2	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

t statistics in parentheses

Standard errors are at the state-level. All regressions use borrower fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The unit of observation is a firm year. Dependent variables are new longterm borrowing (borr.) and plants and machinery (PlaMa). These regressions run from 1988-1993. The first row reports the time trend in new longterm 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 DRTS (before 1994) and Early=0 otherwise. The second row estimates the changing the 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. All significant pre time trends are opposite to the effects we are finding in our main regressions.

Table 5b: Pre-trends.

	(1) profits	(2) profits	(3) profits	(4) profits	(5) intrate	(6) intrate	(7) intrate	(8) intrate
t	1.087***	-0.0990***	1.401***	-0.0268	0.602***	0.650***	0.542***	0.580***
	(8.20)	(-2.89)	(12.69)	(-0.48)	(3.71)	(3.50)	(2.96)	(2.93)
early*t	-0.125	-0.0799			-0.0243	-0.00860		
	(-0.75)	(-1.39)			(-0.21)	(-0.08)		
t*Tang.ass.		0.0357***		0.0380***		-0.00141		-0.000888
		(29.57)		(22.79)		(-1.40)		(-0.83)
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
r2	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

t statistics in parentheses

Standard errors are at the state-level. All regressions use borrower fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The unit of observation is a firm year. Dependent variables are profits and the interest rate (intrate). These regressions run 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 the 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. All significant pre time trends are opposite to the effects we are finding in our main regressions.

Table 6: Average effects by quartile.

	(1) borr.	(2) PlaMa	(3) profits
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-valuequart 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
r2	0.0189	0.0633	0.0491
N	9762	16605	16605

t statistics in parentheses

Standard errors are clustered at the state - level. All regressions use borrower fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run from 1992-2003. The dependent variables are new longterm borrowing (borr.), plants & machinery (PlaMa), and profits, respectively. DRT is an indicator function which is one if a DRT was operating in the state of the firms' headquarter at the end of the fiscal year and zero otherwise. The variable DRT*quart=j is the additional effect of DRT over and above the effect captured by the variable DRT which is the baseline impact for the first quartile firms. The overall effect of DRT for a firm of class j is given in the field following "DRT effect on quart j". The field "P-value quart j effect." reports the the p-value for a F-test of the hypothesis DRT+DRT*quart=j =0. All regressions are run with quartile specific time varying time trends, i.e. an interaction of the quartile dummies and the linear time varying state trend.

Table 7a: Effects on the interest rate using Prowess data.

	(1)	(2)	(3)
	intrate	intrate	intrate
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 class 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	
r2	0.00982	0.0129	0.0210
N	16049	16049	16049

t statistics in parentheses

Standard errors are clustered at the state- level. All regressions use borrower fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run from 1992-2003. The dependent variable intpay is the interest rate in percent. Tables 3 and 17 provide a more detailed description of the set-up. DRT is one if the firm is subject to DRT and zero otherwise. The first two regressions estimate the average impact of DRT on the interest rate and year dummies control for year specific shocks. DRT*Tang.Ass is a linear specification and used together with controls for nationwide year specific distributional effects (Year dummies and YDumm*Size). The last two columns report the results of a quartile regression. The variable DRT*quart=j is the additional effect of DRT over and above the effect captured by the variable DRT which is the baseline impact for the first quartile firms. The overall effect of DRT for a firm of class j is given in the field following "DRT effect on quart j". The field "P-value quart j effect." reports the the p-value for a F-test of the hypothesis DRT+DRT*quart=j =0. All regressions are run with the appropriate state specific time trends. 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.

Table 7b: Effects on the interest rate of newly issued loans using bank data: starting in 1992 using 8 year loans or longer.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	intrate	intrate	intrate	intrate	intrate	intrate	intrate	intrate
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
r2	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
F	.	.	.	.	.	.	.	.

t statistics in parentheses

Standard errors are clustered at the state-level. All regressions use borrower fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The dependent variable intpay is the interest rate on newly issued project loan given out by a large indian bank. The variable DRT is one if the state is subject to DRT in a given year and zero otherwise. We observe loans given out in any quarter and we control for time effects using quarter dummies. Furthermore, 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.

Table 8: Effect of DRT on credit: bank vs. firms.

	Prowess data: new lterm borrowing				RBI bank data: bank lending			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	borr.	borr.	borr.	borr.	lending	lending	lending	lending
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
r2	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

Standard errors cluster at state level. t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run from 1992-2003. The left hand side of the table reports coefficient estimates from regressions using Prowess firm level data and the right hand side of the table reports coefficient estimates from regressions using RBI data. All regressions include firm (Prowess data) or state fixed effects (RBI data). All regressions also include time fixed effects (year for Prowess and quarter for RBI data). The dependent variables are new longterm borrowing (borr.) for the Prowess database and state-wise total outstanding bank credit for the RBI data. For the Prowess data, DRT is an indicator function which is one if a DRT was operating in the state of the firms' headquarter at the end of the fiscal year and zero otherwise. Using RBI data, DRT is one if a state had DRT in the last quarter and zero otherwise. 'DRT year =1' is a dummy which equals one if DRT was introduced last year, 'DRT year =2' is a dummy if DRT was introduced 2 years ago and 'DRT year ≥ 3' is a dummy if DRT was introduced three or more years ago.

Table 9: Effect of DRT on branches and lending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	citybranch	citybranch	countrybranch	countrybranch	citycredit	citycredit	countrycredit	countrycredit
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
r2	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

Standard errors cluster at state level. t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run 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 one if a state had DRT in the last quarter (year) and zero otherwise. 'DRT year =1' is a dummy which equals one if DRT was introduced last year, 'DRT year =2' is a dummy if DRT was introduced 2 years ago and 'DRT year $\geq$ 3' is a dummy if DRT was introduced three or more years ago.

Table 10: Effect of DRT on bank profits

	rbi (e) net profit			profits PBDPTA		
DRT exposure	9371.9***	14837.5***	27987.3***	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

t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Standard errors are clustered at the bank level. All regressions include year fixed effects. All regressions run from 1992-2003. The dependent variable is bank profits. The left hand side reports coeffiecient estimates using bank profits data as taken from RBI and substracts 'Interest Expenses', 'Operating Expenses' and 'Provisions and Contingencies' from 'Total Earnings of the Bank'. The right hand side is bank profits from the Prowess database. 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) which are subject to DRT.

Table 11: Dynamic interest rate effects using bank data.

	Prowess data		Private bank	
	intrate	intrate	intrate	intrate
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
r2	0.00982	0.00721	0.188	0.238
N	16049	14875	1557	1490

Standard errors cluster at state level. t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run from 1992-2003. The dependent variable is the interest rates of firms from Prowess (left hand side) and the interest rate of the private bank database (right hand side). All regressions include firm fixed effects; also included are year fixed effects for the Prowess data and quarter fixed effects for the bank data. DRT is one if a state had DRT in the last quarter (year) and zero otherwise. 'DRT year =1' is a dummy which equals one if DRT was introduced last year, 'DRT year =2' is a dummy if DRT was introduced 2 years ago and 'DRT year $\geq$ 3' is a dummy if DRT was introduced three or more years ago.

Table 12: Differential effect of DRT on firms of different asset quartile.

	(1) borrowings	(2) borrowings	(3) borrowings	(4) borrowings
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
r2	0.336	0.336	0.0186	0.0195
N	1445	1445	3451	3451

Standard errors cluster at state level. t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run from 1992-2003. The dependent variable is new long term borrowing (borr.). The left hand side reports coeffiecient estimates for regressions in which the sample is restricted to firms belonging to the first quartile (of 1990 tangible assets) and the right hand side restricts the sample to fourth quartile firms. All regressions include firm fixed effects, year fixed effects and state trends. DRT is one if a DRT was operating in the state of the firms' headquarter at the end of the fiscal year and zero otherwise. 'DRT year =1' is a dummy which equals one if DRT was introduced last year, 'DRT year =2' is a dummy if DRT was introduced 2 years ago and 'DRT year $\geq$ 3' is a dummy if DRT was introduced three or more years ago.

Table 13: Effect on DRTs on firm repayment behavior

	(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 x Overdues	0.138	0.136	0.158	0.159
	(3.250)	(3.220)	(2.522)	(2.460)
	(***)	(***)	(**)	(**)
Overdues x Assets			0.00354	0.00330
			(1.401)	(1.397)
			()	()
DRT x Assets			9.25e-05	0.000179
			(0.613)	(1.107)
			()	()
DRT x Overdues x Assets			-0.00131	-0.00104
			(-0.777)	(-0.618)
			()	()
Observations	2090	2090	2090	2090
R^2	0.549	0.550	0.550	0.553

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 borrower's cash flow. Column 4 includes year dummies $\times$ size. Standard errors are clustered at the state level. t-statistics in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 14: Effect of DRTs on Processing of Legal Suits, by Claim Size

	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

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 the civil court. In columns 1-5 the dependent variable is the time taken to a particular event.
In columns 6 & 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 in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

APPENDIX 1: Extension of Model to Multi-Factor Context

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:

$$\gamma = AK^{1-a}L^a = AK\kappa^{-a} \tag{12}$$

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 follows: $K = \gamma\frac{\kappa^a}{A}, L = \gamma\frac{\kappa^{a-1}}{A}$. Hence the incentive-constrained Walrasian demand maximizes:

$$e[y_s f(\gamma) - T_s] + (1-e)[y_f(\gamma) - T_f] - w.\gamma\frac{\kappa^{a-1}}{A} \tag{13}$$

subject to

$$T_k \leq \theta[W + \nu.y_k f(\gamma) + (1-\delta)\gamma\frac{\kappa^a}{A}] + d \tag{14}$$

and the lenders participation constraint

$$eT_s + (1-e)T_f \geq (1+\pi)\gamma\frac{\kappa^a}{A}. \tag{15}$$

In (14) 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 'over-capitalize', in order to relax the credit constraint.

In order to obtain closed-form solutions, we assume $\nu = 0$, and also that $f(\gamma) = \gamma^\epsilon$ for $\epsilon \in (0,1)$. The key point to note is that the incentive constraint (14) 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:

$$K \equiv \gamma\frac{\kappa^a}{A} = \frac{\theta W + d}{1 + \pi - \theta(1-\delta)}. \tag{16}$$

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

$$\kappa = [\frac{A}{\gamma}]^{\frac{1}{a}}[\frac{\theta W + d}{1 + \pi - \theta(1-\delta)}]^{\frac{1}{a}} \tag{17}$$

and the scale γ is then chosen to maximize

$$\bar{y}f(\gamma) - \gamma\frac{\kappa^a}{A}[(1+\pi) - \frac{w}{\kappa}]\}. \tag{18}$$

Table 15: Effects on Wagebills.

	(1) wagebill	(2) wagebill	(3) wagebill
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 class 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	
r2	0.109	0.701	0.240
N	16605	16605	16605

t statistics in parentheses

Standard errors are clustered at the state- level. All regressions use borrower fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run from 1992-2003. The dependent variable is the wage bill. Tables 3 and 17 provide a more detailed description of the set-up. DRT is one if the firm is subject to DRT and zero otherwise. The first two regressions estimate the average impact of DRT on the wagebills and year dummies control for year specific shocks. DRT*Tang.Ass is a linear specification and used together with controls for nationwide year specific distributional effects (Year dummies and YDumm*Size). The last two columns report the results of a quartile regression. The variable DRT*quart=j is the additional effect of DRT over and above the effect captured by the variable DRT which is the baseline impact for the first quartile firms. The overall effect of DRT for a firm of class j is given in the field following "DRT effect on quart j". The field "P-value quart j effect." reports the the p-value for a F-test of the hypothesis DRT+DRT*quart=j =0. All regressions are run with the appropriate state specific time trends. 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.

Using (16, 17) this reduces to

$$\bar{y}f(\gamma) - \frac{\theta W + d}{1+\pi-\theta(1-\delta)}(1+\pi) - [\frac{\theta W + d}{1+\pi-\theta(1-\delta)}]^{1-\frac{1}{a}}[\frac{\gamma}{A}]^{\frac{1}{a}}w. \tag{19}$$

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 following expressions for scale and capital intensity:

$$\gamma = [\bar{y}\{\frac{\theta W + d}{1+\pi-\theta(1-\delta)}\}^{\frac{1}{a}-1}\frac{aA^{1/a}}{w}]^{\frac{a}{1-a\epsilon}} \tag{20}$$

$$\kappa = [\frac{\theta W + d}{1+\pi-\theta(1-\delta)}]^{\frac{1-\epsilon}{1-a\epsilon}} w^{\frac{1}{1-a\epsilon}}(a\bar{y})^{-\frac{1}{1-a\epsilon}}A^{-\frac{\epsilon}{1-a\epsilon}} \tag{21}$$

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

The capital asset of the firm is given by

$$K = \alpha(\theta) + \beta(\theta)W \tag{22}$$

where now

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

As θ rises owing to a DRT, the incentive-constrained demand for capital, hence credit, rises. 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 continue to be described by (6).

In addition, (21) 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, i.e., there are no GE effects operating through the labor market. So w is fixed. The next section will consider the case where the entire source of the GE effect is an upward-sloping supply of labor.

Equation (21) shows that an increase in θ will cause capital intensity in each firm to move in the same direction as its capital assets. Hence capital intensity falls in small firms, and rises in large firms as a result of DRT, if GE effects are strong enough.

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

$$WB_{jt} = C.[\alpha(\theta_{jt}) + \beta(\theta_{jt})W_j]^{\frac{\epsilon(1-a)}{1-a\epsilon}} w_t^{-\frac{a\epsilon}{1-a\epsilon}} \tag{23}$$

[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 under-capitalized so have a higher marginal rate of return on capital. One way of explaining 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 'make up' by employing more workers per unit of capital.

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

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:

$$L_{jt} = C_1.[\frac{\theta W + d}{1 + \pi - \theta(1-\delta)}]^{\frac{\epsilon(1-a)}{1-a\epsilon}} w^{-\frac{1}{1-a\epsilon}} \tag{24}$$

In the absence of GE effects in the credit market, a rise in θ will raise capital assets of all firms, and thus raise labor demand. Then the wage rate will rise. The net effect on firms output, employment or profits will depend 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

$$L(\theta; W) = C_1[\alpha(\theta) + \beta(\theta)W]^{\epsilon(1-a)} w(\theta)^{-1} \tag{25}$$

and

$$\gamma(\theta; W) = C_2\{[\alpha(\theta) + \beta(\theta)W]^{(1-a)/a} w(\theta)^{-1}\}\frac{1 - a\epsilon}{a} \tag{26}$$

Hence L is rising in θ if and only if

$$\epsilon(1-a)\frac{\alpha'(\theta) + \beta'(\theta)W}{\alpha(\theta) + \beta(\theta)W} > \frac{w'(\theta)}{w(\theta)} \tag{27}$$

It is easily checked that the LHS of (27) 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 will expand in large firms and contract in small ones, though the exact threshold between expansion and contraction of output will be 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.

[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.

APPENDIX 2

Table 16: Measuring the dependent variables in logs.

	logbor	logplama	logprof
DRT	-0.410***	-0.0642*	-0.00305
	(-2.92)	(-1.89)	(-0.08)
DRT*class=2	0.545***	0.0575	-0.0708
	(3.21)	(1.29)	(-0.98)
DRT*class=3	0.311	0.104***	0.0146
	(1.60)	(2.84)	(0.23)
DRT*class=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 class 1.	-0.410	-0.0642	-0.00305
P-value class 1 effect.	(0.00827)	(0.0721)	(0.941)
DRT effect on class 2.	0.135	-0.00672	-0.0739
P-value class 2 effect.	(0.419)	(0.721)	(0.372)
DRT effect on class 3.	-0.0991	0.0393	0.0115
P-value class 3 effect.	(0.516)	(0.163)	(0.754)
DRT effect on class 4.	-0.0748	0.00990	0.151
P-value class 4 effect.	(0.468)	(0.578)	(0.000158)
number of firms	1335	1674	1618
r2	0.0884	0.563	0.264
N	7547	16341	13180
F	.	.	.

t statistics in parentheses

Standard errors are clustered at the state- level. All regressions use borrower fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run from 1992-2003. The dependent variables are the log values of new longterm borrowing (logbor), plants & machinery (logplama), and profits (logprof), respectively. DRT is an indicator function which is one if a DRT was operating in the state of the firms' headquarter at the end of the fiscal year and zero otherwise. The variable DRT*quart=j is the additional effect of DRT over and above the effect captured by the variable DRT which is the baseline impact for the first quartile firms. The overall effect of DRT for a firm of class j is given in the field following "DRT effect on quart j". The field "P-value quart j effect." reports the the p-value for a F-test of the hypothesis DRT+DRT*quart=j =0. All regressions are run with quartile specific time varying time trends, i.e. an interaction of the quartile dummies and the linear time varying state trend.

Table 17: Borrower level clustering: average effects by quartile.

	borrow.	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-valuequart 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
numberofgroups	1406	1683	1683	
r2	0.0189	0.0633	0.0491	0.0210
N	9762	16605	16605	16049

t statistics in parentheses

Standard errors are clustered at the borrower level. All regressions use borrower fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions run from 1992-2003. The dependent variables are new longterm borrowing (borr.), plants & machinery (PlaMa), profits, and interest rates (intpay), respectively. DRT is an indicator function which is one if a DRT was operating in the state of the firms' headquarter at the end of the fiscal year and zero otherwise. The variable DRT*quart=j is the additional effect of DRT over and above the effect captured by the variable DRT which is the baseline impact for the first quartile firms. The overall effect of DRT for a firm of class j is given in the field following "DRT effect on quart j". The field "P-value quart j effect." reports the the p-value for a F-test of the hypothesis DRT+DRT*quart=j =0. All regressions are run with quartile specific time varying time trends, i.e. an interaction of the quartile dummies and the linear time varying state trend.