Do Debt Contract Enforcement Costs Affect Financing and Asset Structure?

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Abstract

Using staggered changes to debt contract enforcement costs in India, we estimate it's causal effect on financing and asset maturity. A reduction in enforcement costs is associated with an increase in long-term debt and a decrease in short-term debt and trade-credit. The increase in debt maturity is confined to firms that borrow from multiple and diverse set of lenders and to smaller firms. Firms reduce the number of banking relationships and increase (decrease) the amount of long-term (short-term) assets on their balance sheet. Our results highlight an important "causal" effect of debt contract enforcement costs on firm financing and investment.

JEL Classification: F34, G32, G33, G38, K42

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Introduction

In recent years, there is increasing interest in academic, industry, and policy circles in understanding how impediments to financial contract enforcement may affect the development of markets. Costs lenders incur to enforce their contracts will reduce the amount they can recover when firms default. This in turn is likely to both increase the cost of external debt finance, and also, in extreme cases, lead to a complete break-down of the market for external finance. This is an important issue because many emerging markets are characterized by weak legal environments, where financial contract enforcement is costly. For example, in India, corporate bankruptcies take on average 6 years to resolve, during which time firms enjoy a complete moratorium on all debt payments (Gopalan et al. (2007)). Estimating the effect of such enforcement costs on firm investment and financing is difficult. Enforcement costs typically vary across countries, and any test that compares firms' investment or debt financing structures across countries will be plagued by the "omitted variables" problem – i.e., the inability to control for all unobserved determinants of investment and financing. Legal enforcement also typically covaries with the underlying law, making it difficult to tease out the effect of weak enforcement as opposed to weak laws. In this paper, we use a quasi-natural experiment from India – one that reduced the cost of legal enforcement of debt contracts in different states without any accompanying change in the underlying law – and estimate its effect on the debt structure of firms from those states. We find that enforcement costs have significant effects on firm financing and investment. Following a reduction in enforcement costs, firms have more long-term debt, less short-term debt, less trade credit and fewer banking relationships. There is also an increase in the maturity of the assets on firms' balance sheets.

The theoretical literature has devoted significant effort to understand how firms and markets may respond to an environment where contract enforcement is costly. The first implication common to all models is that when enforcement costs go down, lenders will be willing to lend more and reduce their demand for collateral. Lilienfeld et al. (2012)

explore this, and present evidence in the context of the law change that we also analyze.[1] Not surprisingly, however, there are other important implications of enforcement costs for credit contract design that have been pointed out by the literature. A prominent example is Douglas Diamond's presidential address to the American Finance Association in 2004 (Diamond (2004)), where he argues that high enforcement costs may force firms to borrow short-term debt from multiple lenders. The threat of withdrawal of finance by a lender – a lender run a la Diamond and Dybvig (1983) – may provide incentives for the other lenders to monitor the firm and sustain external finance. This would imply that a reduction in enforcement costs should be accompanied by an increase in firm's debt maturity and a reduction in the number of lenders a firm borrows from. In addition, the literature on related lending proposes that higher enforcement costs may also increase the importance of informed lending, such as supplier ("trade") credit. Suppliers not only have alternative ways to enforce their contracts, but their expertise in liquidating the collateral (their product) may also prove important (Schwartz (1974), Petersen and Rajan (1997), Fisman and Love (2003), Fabbri and Menichini (2010)).

Finally, one common theme that pervades both these theoretical predictions on financing structure is that poor enforcement tilts debt towards short-term financing.[2] Recent theoretical work by Milbradt and Oehmke (2014) suggests that this might affect asset structure as well. They present a model where a firm's asset and liability sides are jointly determined, giving rise to a feedback from financing frictions on the liability side to asset maturities. Applied to our context, their theory would imply that the shock to financing structure due to DRT adoption may also allow firms to hold more long-term assets on their books. We use our experimental setting to test these predictions.

The experiment we study is the establishment of debt recovery tribunals (DRTs) in India.

[1] Specifically, Lilienfeld et al. (2012) show that if the (short-term) supply of funds does not match the lenders' increased willingness to lend when enforcement costs go down, then we may observe an increase in finance for the larger firms accompanied by rationing for the smaller and more opaque firms.

[2] Short term debt in our data is debt maturing in the next year or earlier, while the typical trade credit contract in India involves an implicit maturity of 3 months (De and Singh (2013)).

Following the initial round of economic reforms in 1991, many Indian banks were saddled with significant non-performing loans. To aid in recovery of money from the bad loans, in 1993, the Government of India (GoI) passed a national law establishing new specialized courts to process debt recovery cases. This law allowed the national government to establish new DRTs across the country, where banks and financial institutions could file suits for claims larger than rupees Rs 1 million (currently $1 is worth approximately Rs 58; in the early 1990s, it was on the order of Rs 25). Each DRT had jurisdiction over firms registered in a set of neighboring states. Importantly, individual states did not have the authority to choose whether or not to establish these tribunals. While DRTs began to be set up soon after the law was passed, with five states receiving tribunals in 1994, this process was halted by a legal challenge to the law (Visaria (2009)). The establishment of new DRTs resumed in 1996 after a favorable ruling from the Supreme Court of India. An important feature of this disruption, for our purpose, was that while no new DRTs were established during this hiatus, existing DRTs continued to function. By 2000, all Indian states had access to a DRT. Appendix A lists the dates when DRTs were established in different states while Figure 1 provides the same information on the map of India. The delay due to legal challenge provides us with a plausibly exogenous variation in the presence of DRTs across Indian states. We exploit this variation and compare the behavior of firms in the early DRT states (that got a DRT in 1994) with those in the latter DRT states (that got a DRT after 1996). Our identification strategy is similar to Lilienfeld et al. (2012) (LMV).

The assumptions we make for identification are two-fold. First, we assume that DRTs significantly reduced contract enforcement costs. There is significant support for this assumption. Visaria (2009) documents that debt recovery cases took much less time to be processed in a DRT as compared to in a civilian court. Our second assumption is that the delay in establishment of DRTs was exogenous to the debt and asset structures of firms. That is, in the absence of a DRT, the debt and asset structures of firms in early and late DRT states would have trended in a similar manner. This is a reasonable assumption because the delay in the

establishment of the DRT was caused by a legal challenge from lawyers from one province who were concerned with the DRTs circumventing the civilian courts. Furthermore, as we show in Section 4, the timing of the adoption of DRTs cannot be explained by observable differences in macroeconomic characteristics or average firm characteristics across states (see also LMV).[3]

Our firm-level data comes from Prowess, a data set maintained by the Centre for Monitoring Indian Economy (CMIE), which has been used by a number of prior studies on Indian firms, including Bertrand et al. (2002), Gopalan et al. (2007), and LMV. Although an important limitation of our data is that we do not observe specific contract terms such as financial covenants, collateral and cost of debt finance, which limits our ability to analyze the effect of enforcement costs on contract terms, the level of detail contained in Prowess (for example, information on which banks and financial institutions lend to individual firms) allows us to examine many other interesting facets of contract theory outlined below. [4]

We begin our empirical analysis by estimating how the average firm's debt structure changed around DRT establishment. Our main dependent variables are *LTD/TA* ($\frac{LongTermDebt}{Assets_{(t-1)}}$), *STD/TA* ($\frac{ShortTermDebt}{Assets_{(t-1)}}$), *TC/TA* ($\frac{TradeCredit}{Assets_{(t-1)}}$) and *Short1* (proportion of short-term debt to the book value of total debt). To provide quick univariate evidence, in Figure 2, we plot the average value of these variables along with the 95% confidence interval (CI) for the states with and without a DRT for the period 1994-2001 (the period from one year before any state received a DRT to one year after all states received a DRT). The red line indicates the average debt level for firms in states with a DRT (early DRT states) while the blue line indicates averages for states that do not have a DRT (late DRT states). As mentioned before, loosely speaking, in our difference-in-difference analysis, our identification comes from comparing firms in early and late DRT states during the time period 1995-2000. From Figure 2, panels

[3]The only variable that systematically predicts DRT timing is the extent of shortfall of judges in the civilian courts, which arise from unfilled judicial vacancies primarily caused by judge transfers and retirements. DRT timing does not correlate with any differences in firm characteristics between the early and late DRT states.

[4]An example of a study using loan level data is Visaria (2009), who analyzes the impact of DRTs on the cost of debt financing using a loan sample from a single lender.

(a)-(c), we find that while firms in early and late DRT states have statistically indistinguishable levels of, *LTD/TA, STD/TA,* and *TC/TA* pre-1995, these levels diverge after the establishment of DRTs in the early states (in 1995), and become statistically distinguishable and then start to converge again after the all the states have a DRT (in 2000). The same trend is also apparent in panel (d), where we examine a composite measure of debt maturity, that measures the proportion of short-term debt to the book value of total debt, *Short1*.[5]

In our multivariate analysis our main independent variables are dummy variables that identify the years relative to the year of establishment of a DRT, with the year before DRT being the base (omitted) category.[6] We also include firm and time fixed effects to control for time-invariant firm specific factors as well as common country-level time-varying factors respectively. Guided by prior research, we also control for lagged values of profitability ($\frac{EBIT}{Sales}$), cash holding ($\frac{Cash}{Total\ assets}$), asset structure (*Tangibility*), and leverage (*Interest coverage*). In order to carefully control for firm size – which might have non-linear effects on maturity structure – we include, in each specification, 100 dummy variables for each percentile of the firm size distribution. All variables that we use in our analysis are defined in Appendix B.

We find that within 3 years of implementation of a DRT, *LTD/TA* goes up by 7.9%, while *STD/TA* and *TC/TA* decline by 10% each (relative to their sample means). To ensure that the effects we document are due to changes in firm's debt structure (the numerator of our dependent variables) and not firm size (the denominator of our dependent variables) we repeat our tests with *Log(1+Long term debt), Log(1+Short term debt),* and *Log(1+Trade credit)* as the dependent variables and obtain consistent results. When we repeat our tests with our composite measure of debt maturity, *Short1*, we find that this measure declines by 10.5% within 3 years after DRT establishment.

We do a number of robustness tests. Our results remain strong after we control for industry trends, exclude all syndicated loans, incorporate different controls for leverage, ex-

[5]Section 3 contains more formal tests for the parallel trend assumption underlying our analysis.

[6]We include individual dummies for each year following DRT, instead of a single post-DRT dummy, to document the time series change in firm financing before and after DRT establishment.

clude all controls, and restrict the sample to only those firms that were in operation before the DRT Act was passed. To ensure that our results are not due to differential state-level trends, we repeat our tests after including a state-specific linear trend, as well as state-level (time-varying) macroeconomic variables and find that our results are unaffected.

Next, we do a number of cross-sectional tests based on the extant theory and its implications for India's institutional environment. According to Diamond (2004), short-term debt is effective in addressing the problem of weak enforcement only if the firm borrows from multiple lenders that cannot coordinate their actions.[7] This implies that the reduction in short-term debt after DRT should be greater among firms that borrow from multiple lenders. Consistent with this, we find that the reduction in *Short1* and *Short2* (which measures the proportion of the sum of short-term debt and trade-credit to total liabilities), after DRT, was confined to the sub-sample of firms that had multiple banking relationships in the pre-DRT period. Firms with multiple banking relationships experience a 18.9% reduction in *Short1* in the 3 years after establishment of a DRT. On the other hand, firms with a single banking relationship in the pre-DRT period did not experience any significant change in *Short1* or *Short2*.

Although the Indian banking sector was dominated by Government owned banks (La Porta et al. (2002)), during our sample period, private banks (and foreign banks) were gaining importance. Coordination is likely to be easier among Government banks that sometimes lend through a consortium. So, potential lender runs – which are a crucial ingredient in sustaining the short-term debt equilibrium, *and result from co-ordination failure* – are less likely to be a credible threat for firms that only borrow from Government banks. Consistent with this idea, we find that the changes in debt maturity after the establishment of a DRT is confined to the sub-sample of firms that borrow from *at least one* private bank in the pre-DRT period. A firm that borrows from at least one private bank experiences a 19.77% (0.087/0.44) reduction

[7] If a single bank lends to the firm and the borrowing firm misbehaves, it may be optimal for the bank to re-negotiate the debt contract, especially in environments where the alternative of taking the borrower to court is costly. With multiple lenders, co-ordination may be difficult and the threat of a single lender holding out may prevent renegotiation and precipitate a lender run.

in *Short1* within 3 years of DRT formation. In comparison, a firm that borrows only from Government banks does not experience any significant change after the first year.

LMV find that only large firms experience an increase in debt following establishment of a DRT. To ensure that our results are not disproportionately affected by the larger firms in our sample, we repeat our tests after dividing our sample into firms with above and below median book value of tangible assets in the year before establishment of a DRT. We find that changes in debt structure occur among both small and large firms in our sample, but small firms are affected more. This is consistent with small firms facing greater information and agency problems and benefiting most from a reduction in enforcement costs. A couple of potential reasons for the difference between our results and those in LMV is that, first, unlike our normalized variables, LMV use the year-on-year change in the book value of total debt as the dependent variable, and second, there has been a significant change in the underlying dataset when CMIE (the data provider) corrected for survivorship bias by adding back data on a number of (inactive) firms that were earlier dropped (Siegel and Choudhury (2012)).

Next, we study the number of banks a firm borrows from. We find that – consistent with Diamond (2004)– firms reduce the number of banks that they borrow from following establishment of a DRT. Again, when we differentiate between large and small firms, we find that the decline in the number of lenders is concentrated among small firms.

Finally, we examine the effect of DRT on firm's asset structure. If firms try to match the maturity of their assets and liabilities (Myers (1977), Stohs and Mauer (1994), Milbradt and Oehmke (2014)), say due to collateral requirements from lenders, the increase in long-term debt and the reduction in short-term debt and trade-credit may be accompanied by an increase in the average maturity of firm's assets. Consistent with this idea, we find that firms increase the average maturity of their assets after establishment of a DRT. Specifically, *Fixed Assets/TA* (fixed assets as a proportion of total assets) goes up by 0.018 for the average firm, while *Current Assets/TA* (analogously defined) declines by 0.019 in the 3 years following the establishment of a DRT. Again, these effects are stronger for small firms. For example, *Fixed*

Assets/TA increases by 5.8% and *Current Assets/TA* declines by 7.7% for small firms, while there is no significant change for large firms. Moreover, when we divide firms into two groups – those that experience an increase in long-term debt following the establishment of DRTs, and those that do not, we find that the change in asset structure is concentrated in the first group.

Overall, we make three contributions in this paper. First, we use a quasi-natural experiment to estimate the *causal effect of enforcement costs* on firm financing structure. A distinguishing feature of our paper in comparison to prior literature is our ability to isolate the effect due to changes in enforcement costs as opposed to changes in the underlying law. Consistent with the hypothesis in Diamond (2004), our results highlight that enforcement costs can have a significant effect on firm's debt maturity. The lengthening of debt maturity that occurs with easier contract enforcement may be important in reducing firms' exposure to rollover risk and consequent financial fragility.

Second, consistent with theories of trade-credit (Schwartz (1974), Fabbri and Menichini (2010)), we show that trade-credit may be an important source of finance in an environment where enforcement costs are high. To the extent trade credit is a significant part of firm balance sheets (19% of book assets in our sample), and to the extent it is more expensive than alternative sources of finance (in India, the implicit annualized interest rate on trade credit is as high as 23% for small firms,De and Singh (2013)), removal of financing frictions that allow firms to reduce their dependence on trade credit could be important in fostering firm growth.

Third, we also document how reduction in debt contract enforcement costs can allow firms to invest in long-term assets, possibly through its effect on firms' debt structure. Note that our evidence is novel because the extant empirical corporate finance literature has mostly considered asset maturity as an exogenous determinant of firms' debt maturity. Our results on the other hand show that financing and asset structures may be jointly determined in equilibrium as highlighted in recent theoretical literature (Milbradt and Oehmke (2014)).

Relaxing constraints that prevent firms from obtaining long-term finance may help them invest in long-term assets. The effect of DRT on firm's asset structure that we document highlights that improvements in contract enforcement may be an important step for emerging markets to attract investments in long-term infrastructure projects, an urgent need in many countries.

Finally, it is perhaps worthwhile to draw the reader's attention to the generalizability of our results to other contexts and countries. Although we conduct our experiment in India, we believe that our results are generalizable because India is representative of many of today's emerging financial markets along various dimensions. Like most emerging markets, India's debt markets are dominated by government owned banks and the ratio of private credit to GDP is 0.3, as compared to a world average of 0.418 (Djankov et al. (2007)). India's creditor rights index value is 2, as compared to a world average of 1.787, and, on average, it takes 425 days for contract enforcement in India as compared to a world average of 391 (Djankov et al. (2007)).

The rest of the paper proceeds as follows. We discuss the related literature in Section 1, outline the relevant mechanism and provide the background of the DRT law in Section 2, describe our data and provide summary statistics in Section 3, describe the empirical results in Section 4, and conclude the paper in Section 5.

1 Related Literature

In this section we discuss two separate strands of literature related to our study, and outline our contribution by pointing out how our paper is different. First, our paper contributes to the literature on debt structure choice. The theoretical literature highlights the role of risk, asymmetric information (Flannery (1986) and Diamond (1991b)) and asset liquidation value (Hart and Moore (1994), Berglöf and Von Thadden (1994) and Benmelech (2005)) in determining the firm's debt maturity structure. In comparison, we highlight the role of

enforcement costs. On the empirical side, the literature finds that smaller firms (Titman and Wessels (1988)), firms with higher asset volatility (Barclay and Smith, Jr. (1995)), firms with higher proportion of short-term assets (Stohs and Mauer (1994)) and firms in the high and low end of the risk spectrum (Guedes and Opler (1996)) have more short-term debt. Berger et al. (2005) also find that asymmetric information is an important determinant of debt maturity choice. Saretto and Tookes (2013) show that that firms with traded CDS contracts on their debt are able to maintain higher leverage ratios and longer debt maturities. We include variables shown by prior literature to affect debt maturity as controls in our tests.

Our paper is also related to recent papers that study debt structure in a cross-country setting. Qian and Strahan (2007) show that firms in countries with strong creditor protection borrow from a more concentrated group of lenders, for longer maturities, and at lower interest rates. Bae and Goyal (2009) also employ cross-country data to show that in countries with weak contract enforceability, firms tend to borrow lower amounts, for shorter maturities and face a higher loan spread.[8] Our paper is different from these empirical papers in four important ways. First, we use a natural experiment to identify the *causal* effect of contract enforcement costs on debt maturity structure, and are able to show that controlling for firm-level unobserved heterogeneity does not alter the basic conclusions in these papers. Second, unlike the above papers, we examine how short-term debt and multiple lenders go hand-in-hand to overcome enforcement costs – an argument that lies at the core of Diamond's (2004) model. Third, we look at the effect of enforcement on the *entire* debt structure of firms, *including supplier credit*, which is not only a substantial part of firm financing in poor enforcement environments (the average firm has trade credit amounting to 19% of its assets in our sample), but is also affected significantly by legal enforcement costs, as we show. Finally, unlike the existing literature, we also examine the effect of enforcement costs on asset structure, which allows us to test predictions of recent models like Milbradt and

[8]On a related note, Giannetti (2003) finds that firms operating in sectors with highly volatile returns are able to borrow long-term debt in countries with strong creditor protection. In another early paper, Demirgüç-Kunt and Maksimovic (1999) examine firm debt maturity in 30 countries, and look at the link between stock market activity and maturity structure.

Oehmke (2014).

This study is also related to interesting work by Vig (2012), who uses the passage of a mandatory secured transactions law in India, the SARFAESI Act (Securitization and Reconstruction of Financial Assets and Enforcement of Security Interests Act 2002), to examine the effect of changing bankruptcy law on corporate debt structure. Vig (2012) finds that the reform was associated with a reduction in secured debt, total debt, debt maturity, asset growth and an increase in liquidity hoarding by firms. Vig argues that this evidence is consistent with firms shunning debt in the face of greater bargaining power for creditors. It is difficult to directly compare our results with those of Vig (2012) because while we study improvements in ability to enforce existing laws, Vig (2012) studies the effect of a new law. The apparent contrast in the results in Vig (2012) and our paper (and Visaria (2009)) may partly have to do with this and the fact that the DRT Act improved enforcement from a very low base. Thus in our view there may be non-monotonocity in the relationship between creditor rights (and enforcement) and debt finance and a need to condition on the initial level of creditor rights in estimating the relationship.

The identification strategy in our paper is similar to that employed by both Visaria (2009) and Lilienfeld et al. (2012), although the question tackled is completely different. While Visaria (2009) studies the effect of DRTs on borrower delinquency and loan interest rates, Lilienfeld et al. (2012) show that in the face of inelastic credit supply, a change in enforcement costs may reduce credit access for small borrowers. Thus they highlight the distributional effects of a change in enforcement costs. Our paper is significantly different on several dimensions. First, we examine the *maturity structure* of debt, and *trade credit* – variables that have generated considerable interest in recent theory as well as policy work – which they do not consider. Second, we find that while loan quantity effects documented by Lilienfeld et al. (2012) largely accrue to bigger firms, small firms experience significant changes in their debt structure. Finally, we examine the effect of a change in enforcement costs on the types of *assets* firms invest in, which is a result new to the enforcement literature,

and one that is, perhaps, important from a policy standpoint in many countries.

Second, our paper contributes to the literature on trade credit. Theoretical literature suggests that credit rationing related to ex-ante asymmetric information could result in more use of trade credit (Schwartz (1974); Stiglitz and Weiss (1981)), especially in countries with weak creditor protection. More recent work by Fabbri and Menichini (2010) suggests that in addition to informational advantage, firms' suppliers are better able to extract value from the liquidation of assets in the case of default. Empirical papers that have looked at the role of creditor protection include Petersen and Rajan (1997), Fisman and Love (2003), and Allen et al. (2012). These studies motivate some of our tests, the main difference being our use of a natural experiment particularly suited to examining the effects of legal enforcement on the use of trade credit.

Finally, there is a recent literature which highlights that reliance on short-term debt may expose a firm to rollover risk and increase its overall credit risk (e.g., He and Xiong (2012a), He and Xiong (2012b) and Gopalan et al. (2011)). Empirical papers studying the recent subprime crisis highlight the effect of rollover risk on firm investments (Almeida et al. (2009) and Duchin et al. (2010)). In comparison, our paper highlights the ex ante benefits of rollover risk as documented in Diamond (2004), and points out a link between rollover risk and asset maturity.

2 Mechanism and background about the DRT law

2.1 Outline of the mechanism

In this section we briefly outline the mechanisms that link contract enforcement costs to a firm's financing and asset structures.

Consider an environment where contract enforcement is costly and firms suffer from information and agency problems, say diversion of cash flows by insiders. On default, a lender

can either take the borrower to court or privately renegotiate with the borrower. If enforcement costs are significant enough, a single lender will always choose to renegotiate with a defaulted borrower. Anticipating this, borrowers may divert firm cash flows with impunity as they know that such diversion will not become public through a court process. This in turn may sometimes preclude firm financing.

In his presidential address, Diamond (2004) proposes one possible solution to the problem. If the firm borrows from multiple (say two) lenders, then on default, it may face a credible threat of a Court case, even if going to Court is costly. This will happen because each lender can impose an externality on the other lender. For example, a lender that provides short-term debt can threaten to take the firm to court unless paid in full. The borrower might indeed be willing to pay up to prevent a Court case and liquidation. Note that this payment will come at the expense of the long-term lender. Anticipating this, as in a prisoner's dilemma, both lenders will want to lend short-term and retain the ability to withdraw financing first. In equilibrium, the firm will end up borrowing short-term debt from multiple lenders. This makes the threat of Court action on default a credible one and may in turn reduce ex ante borrower diversion.

Now, if the costs of going to Court fall, the sustainability of financing depends less critically on having short-term debt from multiple lenders. Given that the short-term-debt-from-multiple-lenders solution also involves costs (for example, having multiple lenders may limit the ability of a firm to renegotiate better terms when its credit quality improves, and short-term debt can be destabilizing), a decrease in enforcement cost should, *ceteris paribus*, reduce the proportion of short-term debt and the number of lenders a firm borrows from.[9]

Note that the above argument includes both an increase in firm's demand for long-term debt and an increase in lender's willingness to supply such debt when enforcement costs go

[9]Apart from Diamond (2004), other papers have also identified the disciplining role of short-term debt, most notably Diamond (1991a). Furthermore, other authors have also long recognized the ex ante benefits of borrowing from multiple lenders, e.g., Bolton and Scharfstein (1996). The key distinguishing feature of Diamond (2004) is that short-term debt and multiple lenders go hand in hand to overcome weak enforcement. Our rich data set allows us to highlight this interaction.

down. Our data will not permit us to distinguish the effects due to changes in demand versus changes in supply. On the other hand, our cross-sectional tests motivated by the supply side theories will help us understand the extent to which shifts in the supply drive our results.

If there is heterogeneity among firms in terms of (unobserved) agency costs, then we expect firms with greater agency costs to borrow short-term debt from multiple lenders. A decrease in enforcement costs should especially affect the debt structure of such firms. Since we do not observe the extent of agency costs, we expect the reduction in short-term debt to be more among firms that borrow from multiple lenders to begin with. If there is heterogeneity among banks, we expect firms that borrow from a diverse set of banks to be more susceptible to lender runs, as co-ordination is especially difficult among such banks. Similarly, we also expect small firms – which suffer from greater information asymmetry, and are likely to face a particularly difficult time securing long-term external financing in an environment with weak institutions – to benefit more when enforcement costs are reduced.

Theory also highlights the unique role of trade credit in helping overcome credit rationing due to information and agency problems (Schwartz (1974); Stiglitz and Weiss (1981)). Trade creditors possess some unique advantages. Not only can they hold up borrowers by withdrawing supply of raw materials but they also have an advantage when it comes to realizing value from the collateral (Fabbri and Menichini (2010)) and may also be better informed about the borrower's prospects. This would predict that firms in environments with high enforcement costs will have a higher proportion of supplier ("trade") credit in their debt structure.

Finally, to the extent that enforcement problems in the pre-DRT period limits the availability of long-term debt, and to the extent that firms match the maturity of assets and liabilities (Myers (1977),Stohs and Mauer (1994)), firms may be constrained in their ability to invest in long-term assets (Milbradt and Oehmke (2014)). If this constraint is relaxed after implementation of DRTs, then we expect firms to invest more in fixed assets which are known to have longer maturity. Summarizing, the predictions we test are:

Prediction 1 *A decrease in enforcement costs should be accompanied by an*

1. *increase in long-term debt and a decrease in short-term debt.*
2. *a decrease in the use of trade credit.*

Prediction 2 *The changes in the proportion of short-term debt should be greater among firms that:*

1. *borrow from multiple lenders.*
2. *borrow from a diverse set of lenders.*
3. *are smaller in size.*

Prediction 3 *A decrease in enforcement costs should be accompanied by a decrease the number of lenders a firm borrows from.*

Prediction 4 *A decrease in enforcement costs should be accompanied by an increase the firms' ability to invest in long-term assets.*

2.2 Background about the DRT law

As part of the economic reforms in early 1990s, the Government of India (GoI) reduced import tariffs across the board and also eased entry norms for many industries. Following these reforms, the Reserve bank of India (RBI) – India's central bank – established new rules strengthening both income recognition and capital adequacy norms for banks. The greater competition unleashed by the economic reforms along with stricter income recognition norms resulted in a significant increase in corporate loan defaults. Banks' ability to recover money from the defaulted loans was severely compromised by the inefficient court system. It was common for cases in the Indian court system to continue for extremely long periods; for example, nearly 40 percent of the pending debt recovery cases in civil courts in 1985-1986 had been pending for longer than 8 years (Government of India (1988)).

To expedite the processing of loan default cases, in 1993, the GoI passed a national law establishing new specialized courts to process debt recovery cases. This law (The Recovery of Debt Due to Banks and AIs (RDDB) Act, 1993) allowed the GoI to establish new debt recovery tribunals (DRTs) across the country, where banks and financial institutions could

file suits for claims larger than Rs. 1 million. In contrast to the civil courts, DRTs streamline procedures to allow cases to move through the process more quickly. Defendants are required to respond to summons sooner, provide written defenses and make all counterclaims against the bank at the first hearing. This significantly limits the ability of defendants to delay the process. DRTs also have the power to issue interim orders so as 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.[10] As documented in Visaria (2009)(Table 3), in a small random sample of law suits of a large Indian bank, the DRTs reduced the average time to complete hearing both the applicants' and the defendant's evidence by more than 2,000 days as compared to the civil courts. This suggests that DRTs increased the (present discounted) value of the amount recovered by banks from defaulting loans. Therefore, we interpret the introduction of a DRT in a state as a decrease in the cost of legal enforcement for firms in that state.

The DRT law allowed the federal government to establish tribunals across the entire country and to determine their territorial jurisdiction. Importantly, (1) Individual states did not have the authority to choose whether or not to establish these tribunals; and, (2) neither did individual litigants have the authority to decide whether to go to a civil court or a DRT – the law required that all eligible open cases to be transferred to the appropriate DRT once it was set up. Therefore, in our analysis, all firms in a region are considered to be exposed to DRTs once a DRT is established in its region.

DRTs began to be set up soon after the law was passed; five states distributed across the four regions of the country (North, South, East and West) received tribunals in 1994. However, as reported in Visaria (2009), 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. This halted the establishment of new DRTs, *but existing DRTs were allowed to function* as before. The federal

[10]Functionally, DRTs were set up to be similar to civil courts. The governing law was the same and lawyers did not require special training or qualifications to appear before the DRT. The DRT judges are usually retired civil court judges, familiar with standard legal procedure. The DRT law also allowed for appeals against a judgment, much like Indian civil courts.

government appealed against this ruling to the country's Supreme Court, which issued an interim ruling in favor of the law in 1996. This enabled the establishment of further DRTs. By 1999, most Indian states had received a DRT. Appendix A lists the dates on which DRTs were established in different states. Note that the years in our empirical analysis refer to financial years which typically end on March 31st in India. Thus 1995 refers to the financial year ending on March 31st 1995.

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. We explore this further in Section 4 and find that none of the state level economic factors, including average firm characteristics, are able to consistently explain the timing of the establishment of DRTs.[11] LMV perform an analysis similar to ours and come to the same conclusion.

3 Data and Summary statistics

3.1 Data

We obtain data for our analysis from Prowess, a data base constructed by the Center for Monitoring Indian Economy (CMIE). Prowess provides annual financial data and other descriptive variables for firms, including their industry classification, year of incorporation, and group affiliation. Prowess is a panel of both listed and unlisted public limited companies with assets plus sales greater than Rs 40 million. It covers between 2,000 to 6,000 listed and unlisted firms each year, and about twenty-five percent of the firms are unlisted firms. Prowess provides detailed information from the firm's balance sheet and income statements including a detailed break-up of the firm's liabilities that helps us identify the amount of short-term debt. We also get the registered office address (including the pin-code) for each

[11] The only variable that is correlated with the timing of DRT establishment is the shortfall in the number of judges serving in the civilian courts in the jurisdiction of the DRT. We discuss this further in Section 4.

firm from this database, which we use to identify the DRT jurisdiction within which the firm falls.[12] Prowess also provides a list of banks that the firm borrows from, enabling us to measure the number of banking relationships in any given year.

Importantly, as discussed in Siegel and Choudhury (2012), the Prowess database earlier had an explicit survivorship bias. CMIE systematically omitted all historical observations of companies that eventually failed, ceased to exist independently, or failed to provide disclosure for three consecutive years. This survivorship-bias problem was eliminated when the CMIE publicly announced reincorporation of the historical graveyard set into its data. More recent vintages of Prowess, such as the one we use here, no longer have a survivorship bias. Due to this reason, and also because over time CMIE expanded its inclusion criteria to encompass an incrementally larger percentage of the economy, the number of firm-year observations in our study is larger than those using earlier vintages of Prowess.

In this dataset, we use public data sources to classify banks as Government owned or privately owned. From the overall Prowess sample for the period 1993-2010, we exclude all financial firms (NIC code: 641–663), firms owned by the state and federal governments, firms with less than three years of data with positive values for total assets and sales, and, firms that report no loan outstanding from a bank or financial institution at the end of the financial year.

We complement Prowess with state-level macroeconomic indicators from a GoI data repository web page `http://mospi.nic.in/mospi_new/site/India_Statistics.aspx`. Finally we obtain data on the Indian judicial system from two sources: we thank Sujata Visaria for providing data on the number of judges serving on the bench in various High Courts in India in the 1990s. We supplement this data with the number of approved judicial positions in each of these High Courts collected from another GoI web page `http://pib.nic.in/archieve/lreleng/lyr2003/roct2003/30102003/r301020036.html`.

[12]The pin-code is, loosely, the Indian equivalent of the US zip-code.

3.2 Summary statistics

Table 1 provides the summary statistics of the key variables we use in our analysis. We have a total of 47,319 observations with positive values for sales and total assets. We use five key variables to measure debt maturity. The first three variables are scaled by total assets: *LTD/TA* is defined as $\frac{LongTermDebt}{Assets_{(t-1)}}$, *STD/TA* is defined as $\frac{ShortTermDebt}{Assets_{(t-1)}}$, and *TC/TA* is defined as $\frac{TradeCredit}{Assets_{(t-1)}}$. Short term debt in Prowess is debt maturing within one year while the typical trade credit contract involves an implicit maturity of 3 months (De and Singh (2013)). Our other two measures of debt maturity scale the amount of short-term debt by total debt: *Short1* is the proportion of short-term debt to the book value of total debt and *Short2* is the proportion of the sum of the book value of short-term debt and accounts payable to the sum of total debt and accounts payable. We describe the construction of each variable in detail in Appendix B. To prevent outliers from biasing our conclusions, we winsorize all variables of interest at the 2.5% and 97.5% level.[13]

The mean value of *LTD/TA*, *STD/TA*, and *TC/TA* are, respectively, 29%, 18%, and 19%. The mean value of *Short1* for our sample firms is 44%, while the median is 43%. To put the mean value of *Short1* in context, Gopalan et al. (2011) find that the average proportion of short-term debt to total debt for a sample of Compustat firms with bond ratings during the time period 1980-2008 to be 19.5%. To the extent that contract enforcement is more costly in India than in the U.S., the higher value of *Short1* among Indian firms offers preliminary evidence consistent with Diamond (2004). When we include accounts payable as part of short-term liabilities we find that total short-term liabilities are on average 61% of total liabilities. Another institutional aspect worth highlighting here is that we do not use any public bond data because public bonds were a negligible part of firm financing during the years of DRT implementation.[14]

[13]Winsorizing at 1%, 99% leaves results qualitatively unchanged. We adopt the 2.5% and 97.5% level since our inspection of the data suggests that outliers, especially those that look very much like data entry errors, remain even if we winsorize at 1% and 99%.

[14]For example, according to the Reserve Bank of India's Handbook of Statistics on the Indian Economy, there were only 12 bond issues by Indian corporates during the financial year 1998-99.

The mean value of *Log(Total assets)* in our sample is 5.9 which translates into a book value of total assets of Rs. 3000 million. Firms in our sample are profitable as seen by the mean value of $\frac{EBIT}{Sales}$ of 5%. Firms in India have small cash balance on their balance sheet as seen by the mean value of $\frac{Cash}{TA}$ of 5%. On average, 36% of the firm's assets is comprised of tangible assets as seen from the mean value of *Tangibility*. The median interest coverage of our sample firms is 1.8, which is low as compared to the median interest coverage among Compustat firms reported in Gopalan et al. (2011) of 4.6. This indicates that the firms in our sample are highly levered. The proportion of fixed assets to total assets is 45%, while that of current assets to total assets is similar at 44%. The median firm in our sample borrows from 2 banks as seen from the median value of *Number of banks*. We also find that the median firm in our sample does not have a relationship with a private bank, as seen from the median value of *Private*.

4 Empirical results

4.1 Investigating the timing of DRT establishment across states

We begin our empirical analysis by studying the timing of establishment of DRTs. As mentioned in the introduction, the reform was introduced through a national law that applies to all states of India, which meant that individual states did not have the authority to choose whether or not to establish these tribunals.[15] The National government began to set up DRTs soon after the DRT law was passed, with five states, distributed across four regions of India (North, East, West, and South), receiving tribunals in quick succession in 1994. This process was halted by a ruling by the Delhi High Court in response to a legal challenge to the DRT law (Visaria (2009)). No new states received DRTs in 1995, although existing DRTs continued to function. The establishment of new DRTs resumed in 1996 after a favorable

[15]The DRT law, like most other laws, did not apply to the state of Jammu and Kashmir, which has special autonomous status.

ruling from the Supreme Court of India. As Visaria (2009) argues, events suggest that in the absence of legal difficulties, DRT establishment might have been quick, providing almost no difference in timing across states. Since the staggered timing resulted from a legal challenge mounted by a body of Delhi lawyers soon after Delhi got a DRT, it is highly likely that the stagger is exogenous to the financing and asset structures of firms in the early (and late) DRT states. Nevertheless, in this section, we employ a Cox survival model to investigate which, if any, state-level variables are related to the timing of DRT establishment.

The state-level variables we look at include macroeconomic and judicial indicators, as well as average characteristics of firms in each state prior to DRT establishment. The main macroeconomic variables we include are lagged values of *State GDP*, *State GDP growth*, *Bank Credit/State GDP*, *Share of bank credit*, and *Per capita credit*. We also include variables that characterize the local civilian courts. These include *Judicial shortfall* and *Judges/State GDP*.[16] All the variables are described in Appendix B. The sample for this regression spans 1993-1999 and includes the states that did not have a DRT in any given year.

The results of the regression are reported in table 2. From the results in panel A, we find that the only variable that predicts the timing of DRT adoption with some consistency is *Judicial shortfall*. This is the ratio of unfilled judicial positions in the state scaled by the total number of approved positions ("full strength of the bench"). The significant and positive co-efficient on *Judicial shortfall* is consistent with a stated objective of the DRT, which is to relieve the burden on civilian courts. From the coefficient in column (6), we find that a one standard deviation increase in *Judicial shortfall* (an increase of 0.1) increases the hazard rate of a state receiving a DRT the next year by 26%.

A legitimate concern from this analysis is whether judicial shortfall in the early DRT states is correlated with differences in the financing patterns of firms from those states, which in turn might affect our identification assumption. This is unlikely to be the case, since judicial shortfall is primarily caused by judges retiring or being transferred to fill vacancies at other

[16]Visaria (2009) and Lilienfeld et al. (2012) look at a different list of state-level variables, and also find that none of their variables predict DRT timing.

state High Courts or the Supreme Court, coupled with a complex and time-consuming process for new judge appointments.[17] Nevertheless, we explore this possibility in panel B.

In panel B we repeat the analysis from panel A after including average firm characteristics as additional covariates. We estimate state-wide averages of these firm characteristics over the period 1990-92, which represents the three years before passage of the DRT law. Specifically we include the average value of *LTD/TA*, *STD/TA*, *TC/TA*, *Short1*, *Short2*, *Number of banks*, *Fixed Assets/TA* and *Current Assets/TA* for all the firms in the state as additional regressors. We find that the coefficient on all the firm characteristics are insignificant in all the specifications, so that their inclusion does not alter our conclusions.[18] Thus, there is no systematic difference in the average characteristics between the firms in early and late DRT states across dimensions of interest to us. This finding is similar to Visaria (2009) and Lilienfeld et al. (2012), who look at a list of other firm-level aggregates, as well as other macroeconomic, judicial, and political variables, and find no evidence of DRT establishment being significantly associated with any of them.

4.2 Effect of DRT on debt structure

We begin our empirical analysis by documenting how firms' debt structures change around DRT establishment. Our main dependent variables are *LTD/TA*, *STD/TA*, *TC/TA*, and *Short1* (proportion of short-term debt to the book value of total debt). Panels (a), (b), (c) and (d) of figure 2 presents the levels of these four variables of interest, along with the 95% confidence intervals, for the period 1994-2001 (one year before any state received a DRT to one year after all states had access to a DRT). The red line indicates the average value for firms in states with a DRT (early DRT states), while the blue line indicates averages for firms

[17] The process of appointing a High Court judge in India is outlined in the following GoI web page: `http://doj.gov.in/sites/default/files/memohc.pdf`.

[18] In addition, none of these firm characteristic-based controls predict DRT timing, either in isolation, or when other controls are included.

in states without a DRT (late DRT states).[19]

The first thing to note from the figure is that all the four variables of interest, *LTD/TA*, *STD/TA*, *TC/TA* and *Short1* are statistically indistinguishable between the early and late DRT states during the pre-1995 period. Second, the levels for each of these variables for firms in the early DRT states starts to diverge from those of firms in late DRT states after the early states get a DRT in 1995. The difference between the levels of all variables between early and late-states is statistically significant at the 5% level by 1998-1999. Finally, after all the late states get a DRT in 2000, the lines start to converge. Note that this is univariate evidence and should be taken to be suggestive. We now discuss these multivariate difference-in-difference estimates.

We first provide a test for *Prediction 1* by estimating the change in various debt variables in the years preceding and following the establishment of a DRT. We do that by estimating the following fully saturated model:

$$\mathrm{y}_{it} = \beta_0 + \sum_{s=-3}^{-2} \Gamma_s \text{Pre-DRT(-s)}_{it} + \sum_{s=0}^{12} \Gamma_s \text{Post-DRT(s)}_{it} + \gamma \times X_{it-1} + \delta_i + \delta_t + \varepsilon_{it} \quad (1)$$

where the dependent variable y_{it} is one of *LTD/TA*, *STD/TA*, *TC/TA, Short1* or *Short2*. *Pre-DRT(-s)* (*DRT(s)*) is a dummy variable that takes a value one if it is 's' years before (after) the establishment of a DRT in the firm's state and zero otherwise. Since we have fewer firm-year observations more than three years before and 12 years after DRT implementation dates, we have one dummy variable each for multiple years at the two end points. That is, Pre-DRT(-3) equals one if it is three or more years before the establishment of a DRT and DRT(12+) equals one if it is twelve or more years after DRT. The model is fully saturated with the year immediately before the establishment of a DRT as the excluded category. Therefore, the coefficients on *Pre-DRT(-s)* (*DRT(s)*) compare the level of the dependent variable 's' years

[19]Note that in our classification, we carefully account for the staggered nature of the DRT implementation. For example, when Tamil Nadu gets a DRT in financial year 1997, we remove it from the 'late states' average, and include it in the 'early states' one. In that sense, the figures represent the average debt levels in states with and without a DRT.

before (after) the establishment of a DRT, to the year immediately before it's establishment. We only present coefficients on DRT(t=0) to DRT(t=+3) because although the difference between early and late DRT states remains for at most 5 years (from 1995-2000), most of our effect is manifest within year t+3.[20]

We also include a set of control variables, X_{it-1}, from prior literature (Barclay and Smith, Jr. (1995), Berger et al. (2005), Guedes and Opler (1996) and Stohs and Mauer (1994)). We control for firm profitability using $\frac{EBIT}{Sales}$, for cash using $\frac{Cash}{TA}$, and asset type using *Tangibility*. We measure *Tangibility* as the proportion of book value of property, plant and equipment to total assets. We also control for leverage using *Interest coverage*. All our control variables are lagged by a year. In addition, we carefully control for firm size – which can potentially have non-linear effects on maturity – using 100 dummy variables, one each for each percentile of the size distribution. All variables that we use in our analysis are defined in Appendix B.

According to the DRT law, a case can be assigned to a DRT located in the region where the defendant resides or where the cause of action arises i.e., the location where the defaulted loan is registered (Government of India (1988)). Since loans are usually registered in the state where the firm is located, we assign firms to DRT jurisdictions on the basis of the location of their registered office. The inclusion of firm fixed effects, δ_i, ensure that each indicator is estimated using only variation in the dependent variable around the DRT year relative to all other firms, and time dummies, δ_t, control for country-level trends. The standard errors are corrected for heteroscedasticity and autocorrelation and are clustered at the individual state-level.

As mentioned, our identification assumption in these tests are two-fold. First, we assume that DRTs significantly reduced contract enforcement costs. There is significant support for this assumption. As documented in Visaria (2009), the introduction of DRTs significantly decreased the processing time of a debt recovery case. Our second identifying assumption is that the delay in establishment of DRTs was exogenous to the financing structure of firms.

[20]Table 4 panel A provides coefficients up to DRT(t=+6).

That is, in the absence of a DRT, the debt maturity structure of firms in early and late DRT states would have trended in a similar manner. This is a reasonable assumption because the delay in the establishment of the DRTs was caused by a legal challenge from lawyers who were concerned with the DRTs circumventing the civilian courts. Furthermore, as seen in table 2, the differences in the timing of adoption of DRTs cannot be explained by observable differences in economic characteristics across states. Finally, as we show in Table 4, Panel A, there are no discernible pre-trends in our data.

In panel A of table 3 we provide the results of estimating (1) in our full sample. In columns (1) – (3), we present results for *LTD/TA*, *STD/TA*, and *TC/TA* respectively. Columns (4) and (5) look at *Short1* and *Short2*. First, we note that coefficient on DRT (t=+1) through DRT(t=+3) are positive and significant in column (1) and negative and significant in all other columns. Looking at the first year in which the DRT started functioning (coefficient on DRT(t=0)), we find that it only has a significant negative effect on *STD/TA*. The other changes appear to take more than one year to set in. These coefficients indicate that there was an increase in long-term debt, and a reduction in short-term debt after DRT establishment. Further, the monotonic pattern of the coefficients shows that debt changes happen slowly.[21] This could happen if banks and firms learn about the actual (rather than promised) efficiency of these tribunals as they see them start functioning.

In terms of economic magnitudes, the magnitudes of the DRT(t=+3) coefficients in columns (1) and (2) indicate that *LTD/TA* increased by 7.9%, while *STD/TA* declined by 10%, as compared to their sample means, in the 3 years after DRT implementation. Trade credit, measured using *TC/TA*, also declined by 10% as compared to its sample mean during the same time period. From column (4) we find that the decline in *Short1*, in the 3 years following DRT, was about 10.5%.

In order to put these numbers in perspective, consider a comparison between these coefficient magnitudes and those of other well-known determinants of debt maturity choice.

[21] Recall that the coefficients on the DRT(s) dummies measure the level of the dependent variable in different years following DRT establishment, *relative to the year before DRT*.

The effect of DRT on long-term debt is about the same as that of a 0.67 standard deviation change in tangibility (0.171*0.2= .0342), while it's effect on short-term debt is about the same as that of a three standard deviation change in tangibility. Similarly, the effect of DRT on *TC/TA* and *Short1* is about the same as a 1.5 standard deviation change in tangibility. Furthermore, the magnitude of the effect from DRT(t=+3) is at least equal to a three standard deviation change in profitability, for each of our dependent variables. So, enforcement changes resulting from DRT establishment appear to be at least as important as large moves in well-known determinants of debt structure, like tangibility and profitability. Of course, we recognize that our comparison of magnitudes of these traditional determinants with DRTs is after we control for firm and size-group fixed effects, which may remove a significant amount of variation in the traditional firm characteristics.

In panel B of table 3 we ensure that our results are due to change in the level of debt (the numerator of the dependent variables employed in prior specifications), and not due to changes in firm size (the denominator of the dependent variables in prior specifications). To do so, we repeat our tests with *Log(1+Long-Term Debt)*, *Log(1+Short-Term Debt)*, and *Log(1+Trade Credit)* as the dependent variables and present the results in panel B. Our results show a very similar pattern to panel A.

Overall, this table shows that establishment of DRT is accompanied by large changes in firm's debt structure. There is an increase in long-term debt, and a decrease in both short-term debt and trade credit. This evidence is consistent with various implications of the theoretical models discussed in Section 2.

4.3 Pre-trends and Robustness

In this section we conduct a series of robustness tests. In all subsequent analysis we only present results using our composite maturity variables *Short1* and *Short2* for brevity. Other measures of credit structure yield consistent results and are available upon request.

First, we examine whether there are any pre-trends in our maturity variables. The absence of any such pre-trends is a necessary condition for the validity of our difference-in-difference analysis. In table 4, panel A, we present coefficients for our dummy variables before DRTs were established, and find evidence consistent with our no pre-trends assumption. When we examine the effect of DRTs beyond 3 years after the establishment, we find that the effect continues to remain strong. In other words, the effect we capture appears to be long lived.

In panel B of table 4, we examine the robustness of our results along a number of dimensions. We present results only for *Short1* for brevity, those for *Short2* are very similar. First, a few firms in our sample borrow from the syndicated loan market. Such firms may face a lower risk of lender run because of greater coordination among the banks in a syndicate. We find that our results continue to hold even if we exclude all firms with a syndicated loan – 97 firm-year observations out of 47,319 – from our sample (column (1)).

Next, we find that our results are robust to using $(\frac{Debt}{TotalAssets})$ instead of *Interest coverage* to control for leverage (column (2)). We repeat our tests after excluding the current portion of long-term debt from *Short1* to address concerns that changes in these are involuntary and do not constitute a prediction from Diamond (2004), and find our results remain unchanged (column (3)). In column (4) we control for linear state-year trends and find that our results are robust. In the next robustness check, we include state level macroeconomic indicators (which we use in table 2) as additional regressors in (1). We find that our conclusions are robust to their inclusion (column (5))

Again, one might be concerned about how much of our results are due to firm entry and exit following DRT establishment. First, we try to isolate the intensive margin of our results i.e., the change in debt structure only for firms that exist prior to DRT implementation by repeating our tests in a sample that only includes firms that were active in 1993. Results in this sample are very similar (column (6)). Second, one might be concerned that the (possible) redistributive effects of DRTs as documented in LMV may bias our results. If small firms are unable to borrow following DRT, they may exit the sample at a faster rate. If small

firms typically have more short-term debt and trade credit, then their exit could bias our conclusions. To control for this in column (7) we estimate our model on a balanced panel i.e., we do not allow for any entry or exit.[22] Here again we find that despite a significant reduction in sample size, the establishment of a DRT is accompanied by a significant reduction in Short1 for two years (the year of DRT establishment and the following year).

Next, another possible concern a reader might have is that some coincident economic reform is driving our results. This concern is significantly allayed by the staggered nature of DRT implementation and to the best of our knowledge, no other reform introduced by the Government of India at that period had a geographic stagger that even roughly coincides with that of the DRTs. To allay the concern that industries with high values of short-term debt may disproportionately influence our results, we employ non-parametric controls for time-varying, industry-specific shocks. In particular, we repeat our tests after including industry-year fixed effects (with industry defined at the 2-digit NIC code level), following Gormley and Matsa (2014), and find our results robust (column (8)). Note that this specification is also likely to control for concindent economc reforms such as allowance of foreign investment that affect certain industries and not others.

Finally, one other concern with our regression specification could be that some of the control variables we employ, such as interest coverage, tangibility, or cash, are themselves choice variables for the firm and therefore can bias our estimates. To mitigate such concerns, we use the lagged value of all our control variables in all tests. However, to allay further concerns, we repeat our tests after dropping all control variables, and find very similar results. These results are available on request.

[22]Here, we limit our analysis to 2001 because as we are looking at a balanced panel, the further in time we extend our analysis, the more our sample size shrinks. Our results are not sensitive to the choice of the year 2001 in particular.

4.4 Cross-sectional tests

In table 5 we test to see if the change in debt structure depends on the number of banking relationships a firm has. We do this by estimating (1) after replacing each *DRT(s)* (and *Pre-DRT(-s)*) dummy with interaction terms *DRT(s)* × *Single* and *DRT(s)* × [1- *Single*], where *Single* is a dummy variable that identifies firms with one banking relationship in the year before establishment of a DRT. In this specification, we also include a full set of interaction terms between *Single* and the control variables and time fixed effects. This allows the control variables to have different effects for the firms with single and multiple banking relationships and for both set of firms to have a differential time trend. Note that in this test our sample is confined to (a) firms for which we have information on banking relationships; and (b) firms that were operational in the year before DRT implementation in their state. This causes our sample size to be smaller than in the previous table.

The result in columns (1) – (3) indicate that the fall in *Short1* after DRT is confined to the sub-sample of firms with multiple banking relationships. From column (3) (titled *Diff*) we find that the coefficient on the two interaction terms are significantly different from one another in every year following DRT establishment. Our estimates are economically significant. We find that the amount of short-term debt reduces by -0.083 for firms with multiple banking relationship after establishment of a DRT. Thus the proportion of short-term debt decreases by 18.9% (0.083/0.44) for firms with multiple banking relationships. In columns (4) – (6), we repeat the tests using *Short2* as the dependent variable (recall that the main difference between *Short1* and *Short2* is that the latter includes trade credit in the numerator and in the denominator) and obtain similar results.

In table 6 we test to see if the reduction in *Short1* depends on whether the firm borrows from a private bank. As mentioned before, Prowess provides the names of the banks and financial institutions that a firm borrows from. We obtain the ownership structure of a bank from public sources and classify banks as private if the federal or the state government does not own any shares in them. Of the 330 individual banks that firms borrow from in

our sample, we classify 149 banks as private. To the extent that Government owned banks lend through a consortium, coordination may be easier in case of borrower default.[23] Thus firms that borrow only from Government owned banks are less likely to face a lender run as compared to firms that also borrow from private banks.

To test this prediction, we estimate (1) after replacing each *DRT(s)* (and *Pre-DRT(-s)*) dummy with interaction terms *DRT(s)* × *Private* and *DRT(s)* × [1- *Private*]. *Private* is a dummy variable that identifies firms that borrow from at least one private bank in the year before establishment of DRT in their state. Columns (1) – (3) look at *Short1*, while columns (4) – (6) examine *Short2*. The results indicate that the fall in the proportion of short-term debt after DRT is confined to the sub-sample of firms that borrow from at least one private bank. From the columns titled *Diff* we find that the coefficient on the two interaction terms are significantly different from one another in some cases, but not all. Here again, our estimates are economically significant. We find that, after the establishment of a DRT, the amount of short-term debt, measured by *Short1,* reduces by 19.8% (0.087/0.44, as compared to the sample mean) for firms that borrow from at least one private bank in the pre-DRT period. When we repeat the tests with *Short-2* as the dependent variable, we get very similar results.

In table 7 we test if the reduction in *Short1* and *Short2* varies with firm size. Smaller firms are likely to have greater information and agency problems and face greater difficulty in raising external finance, especially when contract enforcement is costly. Thus these firms may benefit more from a reduction in enforcement costs. On the other hand, Lilienfeld et al. (2012) find that following implementation of DRT, large firms experience a greater increase in loan amount as compared to smaller firms. This would predict that the changes in debt

[23]We expect co-ordination to be easier among Government owned banks because they always lend through a consortium (read syndicate) which is managed by a lead lender. Furthermore, given the domination of Government banks in the Indian banking sector, they repeatedly interact with one another in consortia and reputation concerns may prevent them from taking actions – such as early withdrawal – that may hurt their fellow lenders. Furthermore, Government owned banks in India can sometimes have different incentives other than profit maximization (for example, they are required to lend to certain "priority sectors" in the economy, see Banerjee Abhijit et al. (2004)). To the extent they do, they may not force a borrower towards default by not refinancing the loan.

structure should be greater among larger firms. To test these contrasting predictions, we estimate (1) after replacing each *DRT(s)* (and *Pre-DRT(-s)*) dummy with interaction terms *DRT(s)* × *Small Firms* and *DRT(s)* × [1- *Small Firms*]. We define smaller firms as those that have below-median tangible assets in the year prior to the establishment of a DRT in their state. Our results indicate that the fall in short-term debt after DRT is present both among small and large firms, but the effect is stronger for small firms. From the columns titled *Diff* we find that the coefficients on the two interaction terms are significantly different from one another. Our estimates are economically large for small firms. We find that short-term debt (*Short1*) decreases by -0.097, which is 22% of the sample mean, for small firms. We also find that the difference between large and small firms is statistically significant everywhere.[24]

Finally, we repeat our tests with state-year fixed effects and obtain results similar to the ones reported. In each of these cross-sectional regressions, and find that the effect on financing structure is still significantly more pronounced among small firms, with multiple lenders, one of at least one of which is private. These results are available on request. This helps rule out any concerns regarding unobservable, time-varying, state-level omitted variables, correlated with DRT passage that may confound our cross-sectional results.

4.5 Effect of DRT on the number of banks

In the next set of tests we estimate the effect of DRT on the number of banks a firm borrows from. From *Prediction 3* we expect the firm to reduce the number of banks that it borrows from following the establishment of a DRT in its state.

[24] As mentioned before, there could be two reasons for the difference between our results and that of LMV: the different dependent variables we employ, and the different samples we employ mainly due to improvements in Prowess data coverage. When we repeat LMVs main tests using their dependent variable, change in long-term borrowings, but scale the measure by lagged assets, we find that the increase in long-term borrowings is significant only among small firms. Further, when we repeat the tests in table 7 with dependent variables similar to the ones employed by LMV, *Log(1+Long Term Debt)* and *Log(1+Short Term Debt),* we find that as compared to large firms, small firms obtain more long term debt and similar amounts of short term debt after DRTs are established. This shows that scaling the dependent variable appears to produce a significant effect on the results.

In table 8 we estimate a specification similar to (1). Since the number of banks a firm borrows from is measured using an ordinal scale, we use an ordered-logit specification instead of an OLS specification in these tests. Furthermore, since the ordered logit is a non-linear specification, we do not include firm fixed effects so as to avoid the incidental parameters problem (Neyman and Scott (1948)). Instead, we include state fixed effects along with time fixed effects. The state fixed effects control for unobservable state-level factors, such as the extent of competition in the local banking market, while the time fixed effects account for any change in the number of banks for the country as a whole. The negative coefficient on the *DRT(s)* dummies in column (1) indicate that firms reduce the number of banks that they borrow from after the establishment of a DRT. In terms of economic magnitudes, the coefficient on DRT(t=+3) indicates that, on average, the odds ratio of an average firm borrowing from an additional bank is 0.143 lower three years *after* DRT as compared to the year *before* DRT.[25] Considering the unconditional odds ratio in the overall sample of a firm borrowing from multiple banks (as compared to a single bank) is 2.125[26], this represents a 6.7% decline in the odds ratio.

To explore the cross-sectional patterns, in column (2) we differentiate between large and small firms. As before, we estimate the model in column (1) after replacing each *DRT(s)* (and *Pre-DRT(-s)*) dummy with interaction terms *DRT(s)* × *Small Firms* and *DRT(s)* × [1-*Small Firms*], where *Small Firms* are defined as before. The evidence shows that small firms are more likely to reduce the number of banks that they borrow from after implementation of the DRT, while large firms are more likely to increase the number of banks that they borrow from. This evidence is consistent with the view that the banking sector overall was not shrinking – but small firms, who were forced into multiple banking relationships when contract enforcement was poor, reduce the number of banking relationships after DRT

[25]The coefficient in the table is -0.154. Recall that in the logistic specification, this represents change in the log-odds ratio. To convert it into the odds ratio, we simply take $e^{-0.154} = 0.857$, which means that compared to the base year (year before DRT), the odds ratio is -0.143 lower.

[26]From table 1 row 19 we know that 32% of firms borrow from a single bank. This implies an odds ratio for a firm to borrow from multiple banks to be =(1-0.32)/0.32 = 2.125.

implementation. Overall, our evidence indicates that firms reduce the number of banking relationships after establishment of a DRT. In terms of economic significance, the coefficient on DRT(t=+3) indicates that, on average, the odds ratio that a small firm borrows from multiple banks vs. one bank, is 0.348 lower three years *after* DRT, compared to what it was the year *before* DRT was implemented. This is a 16.4% reduction in odds of having one more bank, given the unconditional odds ratio in the overall sample of 2.125.

In unreported tests, we find the results reported in table 7 to be robust along a number of dimensions. Our conclusions hold if we use a negative binomial model, or if we include industry fixed effects in addition to the state fixed effects. When we estimate a logistic regression framework with a dependent variable that identifies firms with multiple banking relationships, we find that firms are significantly less likely to have multiple banking relationships after implementation of a DRT.

4.6 Effect of DRT on asset structure

In this section we investigate if the establishment of DRTs had any effect on the asset side of a firm's balance sheet. Recent theoretical work by Milbradt and Oehmke (2014) suggests that asset and liability sides are jointly determined, giving rise to a feedback from financing frictions on the liability side to asset maturities. This contrasts with the empirical corporate finance literature which has mostly focused on asset maturity as an exogenous determinant of firms' financing choices. Here, we attempt to see whether the shock to financing frictions in form of DRT establishment had any effect on asset structure, as their theory would suggest.

We begin in Table 9 by documenting how the book value of fixed (long-term) and current (short-term) assets change around DRT establishment. *Fixed Assets* is the book value of (gross) land, buildings, plant and machinery, adjusted for inflation using the Wholesale Price Index (in units of Rs. 10 million, at year 2000 prices). *Current Assets* is the sum of book value of cash and bank balance, marketable securities, sundry debtors outstanding, bills receivable,

accrued income, and inventories, adjusted for inflation. We estimate a specification similar to (1) with *Fixed Assets/TA* and *Current Assets/TA* as the dependent variables.

In column (1), we present results for *Fixed Assets/TA*, while in column (5), we show results for *Current Assets/TA*. First, we note that coefficients on the DRT dummies are monotonic throughout the table. This is consistent with firms taking time to adjust their asset structure, very similar to the gradual adjustment we saw in the debt structure regressions (for example, in table 3, panel A). The coefficients indicate that there is an increase in *Fixed Assets/TA*, and a decrease in *Current Assets/TA* after DRT establishment. [27] This along with our earlier result showing an increase in long-term debt and a decrease in short-term debt is consistent with firms changing their asset mix coincident with a change in the liability mix. In columns (2) – (4), and in (6) – (8), we look at how the changes in asset structure depend on the size of the firms in question. As before, we estimate (1) after replacing each *DRT(s)* (and *Pre-DRT(-s)*) dummy with interaction terms *DRT(s)* × *Small Firm* and *DRT(s)* × [1- *Small Firm*]. Again, our results indicate that the effect is much stronger for small firms. In terms of economic magnitudes, *Fixed Assets/TA* increases by 5.8%, and *Current Assets/TA* declines by 7.7% for small firms, while there is no significant change for large firms.

Finally, in table 10 we divide firms into two groups – those that experience an increase in long-term debt *(LTD)* one year after the establishment of DRT, and those that do not. When we repeat our tests within these two groups, our evidence shows that the increase (decrease) in fixed assets (current assets) as a proportion of total assets is confined to the sub sample of firms that experience an increase in *LTD*. Again, in terms of economic magnitudes, for firms that experience an increase in long-term debt after DRT establishment experience a 8.2% increase in *Fixed Assets/TA*, and a 9.6% decline in *Current Assets/TA* within three years. We find no significant change in asset structure for the other sample of firms, those that do not change their debt structures following DRT. We obtain consistent results if we partition the sample based on short-term debt(STD) or *Short1* instead of LTD (not reported). These

[27] The decrease in current assets is inconsistent with the view that firms perceived the DRTs as increasing the threat of inefficient liquidation and hoarded liquidity in response.

results highlight a strong link between the incerease in debt maturity and asset maturity among the firms in our sample.

Overall, our evidence shows that a legal reform that tilts firms' financing structures towards more long-term financing also affects the composition of their assets. Firms that increase their long-term debt following the reform also increase their investment in long term assets. These results highlight two things – first, our empirical evidence is consistent with recent theories that suggest that firms financing terms and investment decisions could be jointly determined, leading to short-term-ism in financing translating into short-term-ism in investment. Second, and perhaps more importantly, our results also suggest that reducing enforcement costs may be an important step for emerging markets to attract investment in long-term projects, an urgent need in many countries.

5 Conclusion

How can we sustain external finance in an environment where contract enforcement is costly? This is an important question given that many emerging markets are characterized by weak legal environments where contract enforcement is costly and time consuming. The existing theory literature has proposed various solutions, two prominent ones among them being (i) the use of short-term debt with multiple lenders, and, (ii) the use of trade credit. Testing these theories is difficult because legal enforcement typically varies across countries, and any test that compares firms' debt structure across countries will be plagued by the omitted variables problem – i.e., the inability to control for all unobserved factors that are correlated with debt and asset structures.

In this paper we use the staggered establishment of debt recovery tribunals (DRTs) in India as a shock to enforcement costs, and study its effect on firm's debt and asset structures. Apart from controlling for firm-level unobservables and time-varying country effects, our experimental setting allows us to examine differential time trends (which we do not find).

Our detailed firm level data also helps us understand how the number of lenders changes with contract enforcement costs.

Consistent with Diamond (2004), we find that firms reduce the proportion of short-term debt after implementation of a DRT in their state. This reduction is especially among firms that borrow from multiple lenders in the pre-DRT period, among firms that borrow from a diverse set of lenders that are likely to face greater co-ordination costs, and among small firms. We also find that firms reduce the number of banks that they borrow from after the establishment of a DRT.

Consistent with theories of trade credit (Schwartz (1974), Fabbri and Menichini (2010)), we also find large reductions in the usage of trade credit following improvements in enforcement. This is consistent with the view that improvements in contract enforcement can enable firms to graduate from relationship-based borrowing to arms-length, financial market-mediated borrowing.

Finally, we show that the availability of long term debt also allows firms to tilt their asset mix towards those with longer maturities, consistent with Milbradt and Oehmke (2014). Ours is the first paper to document a link between a country's institutions and the *maturity* of the financing firms obtain and the *maturity* of real assets held on their balance sheet. Overall, our evidence highlights that reducing enforcement costs may be an important step for emerging markets to attract investment in long-term projects, an urgent need in many countries.

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Appendix A: DRT Establishment dates

The table provides the detailed information on the date, location and jurisdiction of DRTs established in different states of India under The Recovery of Debt Due to Banks and FIs (RDDB) Act, 1993

City of DRT	Date of establishment	Jurisdiction	Financial Year	Number of affected firms
Kolkata	Apr 27 1994	West Bengal, Andaman and Nicobar Islands	1995	277
Delhi	Jul 5 1994	Delhi	1995	243
Jaipur	Aug 30 1994	Rajasthan, Himachal Pradesh Haryana, Punjab, Chandigarh	1995	307
Bangalore	Nov 30 1994	Karnataka, Andhra Pradesh	1995	399
Ahmedabad	Dec 21 1994	Gujarat, Dadra and Nagar Haveli, Daman and Diu	1995	442
Chennai	Nov 4 1996	Tamil Nadu, Kerala, Pondicherry	1997	487
Guwahati	Jan 7 1997	Assam, Meghalaya, Manipur, Mizoram, Tripura, Arunachal Pradesh and Nagaland	1997	13
Patna	Jan 24 1997	Bihar, Orissa	1997	49
Jabalpur	Apr 7 1998	Madhya Pradesh, Uttar Pradesh	1999	209
Mumbai	Jul 16 1999	Maharashtra, Goa	2000	1264
		Total Pre-DRT Firms		3690

Appendix B: Description of Variables

Variable Name	Description
Total Debt	Total borrowing (*Prowess* variable) *excluding* borrowings from central and state governments, foreign currency borrowings, loans from promoters, directors, subsidiaries, group associates and deposits from customers, adjusted for inflation using WPI (in units of Rs. 10 million at year 2000 prices).
Short-Term Debt	Secured and unsecured short-term borrowings from banks and financial institutions, commercial papers and current portion of long term debt, adjusted for inflation using WPI (in units of Rs. 10 million at year 2000 prices).
Long-Term Debt	Total Debt *minus* Short-Term Debt (in units of Rs. 10 million at year 2000 prices).
Trade Credit	Accounts payable includes accounts payable (excluding accounts payable to group companies and subsidiaries), acceptances and advances from customers, adjusted for inflation using WPI (in units of Rs. 10 million at year 2000 prices).
Short 1	Ratio of short-term debt to total debt.
Short 2	Ratio of the sum of short-term debt and accounts payable to the sum of total debt and trade credit.
DRT	Dummy variable that takes a value one for firms in the jurisdiction of a functioning DRT and zero otherwise.
$Size_{t-1}$	Natural logarithm of book value of total assets (in units of Rs. 10 million), lagged by one year.
$EBIT/Sales_{t-1}$	Ratio of earnings before interest and taxes to sales, lagged by one year.
$Cash/TA_{t-1}$	Ratio of cash and marketable securities to book value of total assets, lagged by one year.
$Tangibility_{t-1}$	Ratio of the book value of land, buildings, plant and machinery to total assets, lagged by one year.
$Interest\ Coverage_{t-1}$	Ratio of earnings before interest and taxes over interest paid, lagged by one year.
Fixed Assets	Calculated as *Gross* Land, buildings, plant and machinery,adjusted for inflation using WPI (in units of Rs. 10 million at year 2000 prices).
Number of banks	Number of banks, financial institutions (including private, public and foreign banks) and cooperatives from which the firm has borrowed in a given year.
Single	Dummy variable that identifies firms with a single banking relationship in the year before establishment of a DRT in their state.
Private	Dummy variable that identifies firms that borrow from at least one private bank in the year before establishment of DRT in their state. Here, we combine all private banks, cooperatives and foreign banks into private category.
Large	Dummy variable that identifies large firms in the year before establishment of a DRT in their state, based on median tangible assets.

Appendix B: Description of Variables (contd.)

Variable Name	Description
State GDP	Gross domestic product of the state.
State GDP growth	Growth rate of the state's gross domestic product.
Bank credit/State GDP	Ratio of bank credit in the state to state GDP.
Share of bank credit	Fraction of credit extended by scheduled commercial banks in the state.
Judicial shortfall	Ratio of the difference between the number of approved judges and the number of appointed judges in the state high court to the number of approved judges in the state high court.
Per capita credit	Ratio of bank credit in the state to the population.
Judges/State GDP	Ratio of the number of judges serving in the state High Court, scaled by the state's GDP.

Table 1: Summary Statistics

This table reports descriptive statistics for our sample firms. All variables are defined in Appendix B. The sample includes all non-Government, non-foreign, non-financial, and non-utility firms from *Prowess*.

	N	Median	Mean	SD
Long Term Debt/Assets(t-1)	47319	0.20	0.29	0.34
Short Term Debt/Assets(t-1)	47319	0.15	0.18	0.16
Trade Credit/Assets(t-1)	47319	0.15	0.19	0.17
Short 1	47319	0.43	0.44	0.30
Short 2	47319	0.63	0.61	0.26
Long Term Debt(in Rs. Mn.)	47319	71.10	339.04	665.89
Short Term Debt(Rs. Mn.)	47319	52.10	187.55	324.25
Total Debt(Rs. Mn.)	47319	145.7	526.6	909.8
Trade Credit (Rs. Mn.)	47319	57.10	197.48	340.25
DRT Dummy	47319	1.00	0.88	0.32
Size(t-1)	47319	5.90	6.04	1.51
EBIT/Sales(t-1)	47319	0.07	0.05	0.20
Cash/TA(t-1)	47319	0.03	0.05	0.06
Tangibility Ratio(t-1)	47319	0.34	0.36	0.20
Interest Coverage(t-1)	47319	1.80	3.53	7.12
Fixed Assets/TA	44701	0.45	0.45	0.22
Current Assets/TA	44701	0.44	0.44	0.20
No. of Banks and FIs	29430	2.00	3.05	2.41
Single	18715	0.00	0.32	0.47
Private	18715	0.00	0.47	0.50
Large	28535	0.00	.50	.50

Table 2: Timing of Establishment of Debt Recovery Tribunals in Different Indian States: Cox Survival Regressions

This table reports results from Cox survival regressions that investigate the timing of establishment of DRTs in different Indian states. Panel A uses lagged values of state-level macroeconomic indicators, while Panel B looks at state-level aggregate firm characteristics in addition to the macro variables. All variables are described in Appendix B. Firm characteristics are constructed as state-level averages over the three years before the passage of DRT law, i.e., from 1990-1992. Standard errors reported in parentheses are cluster adjusted at the state level. * , **, and *** indicate statistical significance at 10%, 5%, and 1% level respectively.

Panel A: DRT and state level macroeconomic and judicial characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
State GDP(SGDP)	-.0001	-.0009	-.0007	-.0009	-.001	-.0008	-.0007
	(.00004)***	(.0006)	(.0007)	(.0007)	(.0007)	(.0007)	(.0001)
SGDP growth	-2.368	-2.160	-1.779	-2.344	-2.240	-2.360	-2.247
	(2.141)	(2.091)	(2.279)	(2.261)	(2.311)	(2.060)	(2.198)
Bank Credit/SGDP	.502	5.051	1.626	1.185		5.277	5.230
	(.405)	(5.946)	(.929)*	(.620)*		(6.363)	(6.360)
Share of bank credit		-.055	-.075	-.051	-.035	-.072	-.083
		(.046)	(.073)	(.051)	(.050)	(.049)	(.078)
Per Capita bank credit		-.0002			.00006	-.0002	-.0002
		(.0003)			(.00004)	(.0004)	(.0004)
Judges/SGDP			-37.726				-10.822
			(29.208)				(44.862)
Judicial Shortfall				2.388	2.346	2.303	2.252
				(1.328)*	(1.372)*	(1.329)*	(1.378)
Obs.	80	80	80	80	80	80	80
Log Pseudo likelihood	-42.887	-42.682	-42.738	-42.399	-42.482	-42.265	-42.259
χ^2 statistic	13.941	14.912	13.648	13.208	12.806	15.42	15.712

Panel B: DRT and state level aggregate firm characteristics (averages estimated over 1990-1992)

	(1)	(2)	(3)	(4)	(5)	(6)
Long Debt/TA(State Avg.)	1.089					
	(1.531)					
Short Debt/TA(State Avg.)		-1.019				
		(3.483)				
Trade Credit/TA(State Avg.)			7.857			
			(6.474)			
Fixed Assets/TA(State Avg.)				-2.070		
				(2.486)		
Current Assets/TA(State Avg.)					1.495	
					(3.844)	
No. of Banks(State Avg.)						0.240
						(.212)
State GDP	-.0009	-.0008	-.0006	-.0009	-.0007	-.001
	(.0007)	(.0007)	(.0007)	(.0007)	(.0007)	(.0007)
Share of bank credit	-.056	-.071	-.121	-.065	-.083	-.082
	(.057)	(.050)	(.060)**	(.049)	(.063)	(.047)*
State GDP growth	-2.633	-2.203	-4.990	-2.667	-2.780	-1.528
	(1.928)	(2.103)	(4.382)	(2.132)	(2.669)	(2.031)
Bank Credit/State GDP	4.754	5.244	2.530	4.039	4.762	5.359
	(5.925)	(6.204)	(6.684)	(6.389)	(6.381)	(5.110)
Per Capita bank credit	-.0002	-.0002	-.00005	-.0002	-.0002	-.0002
	(.0003)	(.0004)	(.0004)	(.0004)	(.0004)	(.0003)
Judicial Shortfall	2.158	2.332	2.551	2.286	2.329	2.827
	(1.336)	(1.340)*	(1.238)**	(1.287)*	(1.337)*	(1.547)*
Obs.	80	80	80	80	80	80
Log Pseudo likelihood	-42.177	-42.253	-41.992	-42.177	-42.242	-42.073
χ^2 statistic	15.902	15.827	15.419	15.354	15.386	15.706

Table 3: Debt Maturity and Contract Enforcement Reforms: Evidence from Debt Recovery Tribunals in India

This table provides results from regressions estimating the effect of Debt Recovery Tribunals in a given state on firm's debt structures. Panel A reports results for (1) Long-Term Debt/Total Assets (LTD/TA), (2) Short-Term Debt/Total Assets(STD/TA), (3) Trade Credit/Total Assets(TC/TA), (4) Short-1 and (5) Short-2. Panel B reports results for unscaled measures of debt i.e., (1) Log(1+Long-Term Debt), (2) Log(1+Short-Term Debt) and (3) Log(1+Trade Credit). All these variables are described in Appendix B. We estimate the following regression equation for each different dependent variable (y_{it}):

$$y_{it} = \beta_0 + \sum_{s=-3}^{-2} \Gamma_s \text{Pre-DRT(-s)}_{it} + \sum_{s=0}^{12} \Gamma_s \text{Post-DRT(s)}_{it} + \gamma \times X_{it-1} + \delta_i + \delta_t + \varepsilon_{it} \quad (1)$$

Pre-DRT(-s) (*Post-DRT(s)*) is a dummy variable that takes a value one if it is 's' years before (after) the establishment of a functioning DRT in the firm's state of jurisdiction, and zero otherwise. At the end points, Pre-DRT(-3) equals one for all years that are three or more years before the establishment of a DRT, while Post-DRT(12) equals one for all years that are twelve or more years after DRT. X_{it-1} is a set of borrower-specific time varying control variables that include $(\frac{EBIT}{Sales})_{t-1}$, $(\frac{Cash}{TA})_{t-1}$, Tangibility_{t-1}, $\text{Interest Coverage}_{t-1}$ and a set of 100 dummy variables that identify firm size percentiles based on $Size_{t-1}$. The coefficients on *Post-DRT(s)* for s=0,1,2 and 3, and controls are reported. The coefficients on other variables are not reported for brevity. The model is fully saturated with the year immediately before the establishment of a DRT as the base category. Thus the reported coefficients estimate the amount of short-term debt/ long-term debt/ debt maturity 's' years after the establishment of DRT as compared to the year immediately preceding the establishment of a DRT in the firm's state. The specification includes firm and time fixed effects, and standard errors are corrected for heteroscedasticity and autocorrelation, and clustered at the state-level, as suggested by Bertrand, Duflo and Mullianathan (2004). * , **, and *** indicate statistical significance at 10%, 5%, and 1% level respectively. The base sample includes all non-missing observations for non-Government, non-foreign, non-financial, and non-utility firms from *Prowess*.

Panel A: Scaled Variables

	LTD/TA	**STD/TA**	**TC/TA**	**Short1**	**Short2**
	(1)	(2)	(3)	(4)	(5)
DRT(t=0)	.005	-.008	-.005	-.027	-.022
	(.006)	(.002)***	(.005)	(.003)***	(.004)***
DRT(t=+1)	.014	-.011	-.013	-.028	-.026
	(.006)**	(.003)***	(.007)**	(.008)***	(.005)***
DRT(t=+2)	.016	-.013	-.013	-.040	-.035
	(.007)**	(.004)***	(.006)**	(.010)***	(.005)***
DRT(t=+3)	.023	-.018	-.019	-.046	-.041
	(.011)**	(.005)***	(.009)**	(.013)***	(.008)***
Tangibility Ratio(t-1)	.171	-.031	-.067	-.162	-.177
	(.016)***	(.012)***	(.008)***	(.020)***	(.013)***
EBIT/Sales(t-1)	-.177	-.051	-.028	.013	.010
	(.016)***	(.005)***	(.008)***	(.012)	(.008)
Cash/TA(t-1)	-.039	-.113	-.072	-.094	-.016
	(.019)**	(.015)***	(.019)***	(.023)***	(.016)
Interest Coverage(t-1)	-.001	-.001	.0004	-.001	.002
	(.0003)***	(.0002)***	(.0001)***	(.0006)*	(.0002)***
Size Dummies	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Obs.	47319	47319	47319	47319	47319
No. of Firms	6178	6178	6178	6178	6178

Panel B: Unscaled Variables

	Log(1+Long-Term Debt)	**Log(1+Short-Term Debt)**	**Log(1+Trade Credit)**
	(1)	(2)	(3)
DRT(t=0)	.068	-.064	-.056
	(.019)***	(.022)***	(.027)**
DRT(t=+1)	.060	-.093	-.095
	(.043)	(.029)***	(.041)**
DRT(t=+2)	.107	-.131	-.114
	(.059)*	(.037)***	(.045)**
DRT(t=+3)	.086	-.193	-.171
	(.088)	(.054)***	(.072)**
Tangibility Ratio(t-1)	.684	-.203	-.383
	(.041)***	(.105)*	(.036)***
EBIT/Sales(t-1)	-.154	-.029	-.091
	(.055)***	(.044)	(.047)*
Cash/TA(t-1)	-.864	-1.266	-.602
	(.126)***	(.160)***	(.151)***
Interest Coverage(t-1)	-.016	-.021	.002
	(.002)***	(.002)***	(.0007)***
Size Dummies	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Obs.	47319	47319	47319
No. of Firms	6178	6178	6178

Table 4: Pre-Trends and Robustness Checks

Panel A of this table provides results from regressions 1 for the dependent variables (1) Short-1 and (2) Short-2. Here, we report coefficients on *Pre-DRT(s)* for s=-3,-2 and *Post-DRT(s)* for s=0 to 6. Panel B reports results for various robustness checks for results presented in Table 3 using Short-1 as the dependent variable. Col(1) of panel B reports results after excluding 83 firm-year observations with non-zero syndicated loans. Col(2) reports results explicitly controlling for leverage using debt to assets, instead of interest coverage. In col(3), we report the results for Short-1 measure after excluding current portion of long-term debt from the numerator. Col(4) reports results after including state-year trends in our base specification. We include state-level macroeconomic and judicial variables to our base specification and report results in col(5). In col(6), we limit our sample to firms that existed at the beginning of the sample period. In col(7), we examine a fully balanced panel (no entry or exit). In col(8) we include industry-year fixed effects (with industry defined at the 2-digit NIC code level). * , **, and *** indicate statistical significance at 10%, 5%, and 1% level respectively.

Panel A: Pre-trend

	Short1	**Short2**
	(1)	(2)
DRT(t=-3)	-.006	.009
	(.009)	(.009)
DRT(t=-2)	-.001	.005
	(.006)	(.006)
DRT(t=0)	-.027	-.022
	(.003)***	(.004)***
DRT(t=+1)	-.028	-.026
	(.008)***	(.005)***
DRT(t=+2)	-.040	-.035
	(.010)***	(.005)***
DRT(t=+3)	-.046	-.041
	(.013)***	(.008)***
DRT(t=+4)	-.048	-.048
	(.015)***	(.009)***
DRT(t=+5)	-.053	-.049
	(.014)***	(.011)***
DRT(t=+6)	-.052	-.051
	(.018)***	(.013)***
Controls	Yes	Yes
Size Dummies	Yes	Yes
Firm FEs	Yes	Yes
Year FEs	Yes	Yes
Obs.	47319	47319
No. of Firms	6178	6178

Panel B: Robustness Checks

Dependent Variable: Short 1								
	Excl. Syndicated Loans	**Incl. Leverage**	**Excl. current portion of LTD**	**State-year Trend**	**Incl. State variables**	**1993 sample (No Entry)**	**Balanced sample (No Entry or Exit)**	**Industry-year FEs**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DRT(t=0)	-.025	-.024	-.021	-.030	-.024	-.030	-.018	-.024
	(.004)***	(.003)***	(.004)***	(.006)***	(.003)***	(.007)***	(.008)**	(.004)***
DRT(t=+1)	-.027	-.025	-.023	-.036	-.026	-.032	-.031	-.027
	(.008)***	(.007)***	(.007)***	(.011)***	(.007)***	(.012)***	(.014)**	(.007)***
DRT(t=+2)	-.038	-.039	-.035	-.050	-.038	-.045	-.026	-.039
	(.010)***	(.010)***	(.010)***	(.013)***	(.010)***	(.013)***	(.018)	(.010)***
DRT(t=+3)	-.045	-.045	-.039	-.062	-.045	-.053	-.039	-.048
	(.014)***	(.013)***	(.013)***	(.018)***	(.013)***	(.019)***	(.024)	(.012)***
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Obs.	47222	47113	47319	47319	47319	19551	6379	47319
No. of Firms	6179	6178	6179	6179	6179	1834	872	6179

Table 5: Multiple Banking Relationships

The table provides results from regressions that investigate the effect of Debt Recovery Tribunals on firms with single vs. multiple banking relationships in the pre-DRT period. *Single* (*Multiple*) is a dummy variable that identifies firms with a single(multiple) banking relationship in the year before establishment of a DRT in their state. Our sample is confined to firms that were present in the year before DRT implementation in their state. We interact all the RHS variables in 1 with *single* and *multiple* dummies, and report results for dependent variables *Short 1* and *Short 2*. Standard errors reported in parentheses are cluster adjusted at state level, as suggested by Bertrand, Duflo and Mullianathan (2004). * , **, and *** indicate statistical significance at 10%, 5%, and 1% level respectively. The sample includes all non-Government, non-foreign, non-financial, and non-utility firms from *Prowess*.

	Short1			**Short2**		
	Single	**Multiple**	**Diff**	**Single**	**Multiple**	**Diff**
	(1)	(2)	(2)-(1)	(4)	(5)	(5)-(4)
DRT(t=0)	.002	-.040	-.042	-.005	-.037	-.032
	(.014)	(.009)***	(.018)***	(.009)	(.007)***	(.014)***
DRT(t=+1)	.017	-.051	-.068	.004	-.053	-.057
	(.029)	(.010)***	(.031)***	(.018)	(.007)***	(.021)***
DRT(t=+2)	.019	-.065	-.083	.007	-.066	-.073
	(.034)	(.012)***	(.04)***	(.024)	(.010)***	(.03)***
DRT(t=+3)	.013	-.083	-.097	.007	-.084	-.091
	(.048)	(.019)***	(.056)***	(.030)	(.015)***	(.039)***
Firm FEs		Yes			Yes	
Year FEs× Single		Yes			Yes	
Year FEs× Multiple		Yes			Yes	
Controls× Single		Yes			Yes	
Controls× Multiple		Yes			Yes	
Obs.		18494			18494	
No. of Firms		1524			1524	

Table 6: The Role of Private Lenders

This table provides results from regressions that investigate the effect of Debt Recovery Tribunals on firms that borrow from private banks vs. firms that only borrow from Government owned banks. *Private* (*Non-Private*) is a dummy variable that identifies firms that borrow from at least one *private* (no *private*) bank in the year before establishment of DRT in their state. We combine all private banks, cooperatives and foreign banks into private category. Our sample is confined to firms that were present in the year before DRT implementation in their state. We interact all RHS variables in 1 with *Private* and *Non-Private* dummies, and report results for dependent variables *Short 1* and *Short 2*. Standard errors reported in parentheses are cluster adjusted at state level, as suggested by Bertrand, Duflo and Mullianathan (2004). * , **, and *** indicate statistical significance at 10%, 5%, and 1% level respectively. The sample includes all non-Government, non-foreign, non-financial, and non-utility firms from *Prowess*.

	Short1			**Short2**		
	Private	**Non-Private**	**Diff**	**Private**	**Non-Private**	**Diff**
	(1)	(2)	(1)-(2)	(4)	(5)	(4)-(5)
DRT(t=0)	-.028	-.026	-.002	-.033	-.018	-.015
	(.013)**	(.009)***	(.017)	(.009)***	(.005)***	(.011)
DRT(t=+1)	-.049	-.017	-.031	-.055	-.016	-.039
	(.017)***	(.019)	(.029)	(.011)***	(.009)*	(.016)***
DRT(t=+2)	-.075	-.016	-.059	-.077	-.015	-.062
	(.019)***	(.021)	(.033)**	(.013)***	(.010)	(.018)***
DRT(t=+3)	-.087	-.029	-.058	-.088	-.022	-.066
	(.030)***	(.029)	(.047)	(.021)***	(.014)	(.027)***
Firm FEs		Yes			Yes	
Year FEs× Private		Yes			Yes	
Year FEs× Non-Private		Yes			Yes	
Controls× Private		Yes			Yes	
Controls× Non-Private		Yes			Yes	
Obs.		18494			18494	
No. of Firms		1524			1524	

Table 7: Size of the Firm

This table provides results from regressions that investigate the effect of Debt Recovery Tribunals on small vs. large firms. *Large* (*Small*) is equal to 1 if the firm's tangible assets are above (below) median in the year before establishment of DRT in their state, else zero. Our sample is confined to firms that were present in the year before DRT implementation in their state. We interact all RHS variables in 1 with *Large* and *Small* dummies, and report results for dependent variables *Short 1* and *Short 2*. Standard errors reported in parentheses are cluster adjusted at state level, as suggested by Bertrand, Duflo and Mullianathan (2004). * , **, and *** indicate statistical significance at 10%, 5%, and 1% level respectively. The sample includes all non-Government, non-foreign, non-financial, and non-utility firms from *Prowess*.

	Short1			**Short2**		
	Large	Small	Diff	Large	Small	Diff
	(1)	(2)	(2)-(1)	(4)	(5)	(5)-(4)
DRT(t=0)	-.015	-.055	-.040	-.014	-.040	-.026
	(.008)*	(.008)***	(.013)***	(.005)**	(.010)***	(.012)***
DRT(t=+1)	-.021	-.060	-.039	-.018	-.051	-.033
	(.010)**	(.015)***	(.020)***	(.007)***	(.015)***	(.02)*
DRT(t=+2)	-.026	-.078	-.052	-.024	-.061	-.037
	(.011)**	(.017)***	(.023)***	(.013)***	(.010)***	(.018)***
DRT(t=+3)	-.036	-.097	-.062	-.025	-.080	-.055
	(.016)**	(.018)***	(.026)***	(.012)***	(.021)***	(.027)***
Firm FEs		Yes			Yes	
Year FEs× Large Firms		Yes			Yes	
Year FEs× Small Firms		Yes			Yes	
Controls× Large Firms		Yes			Yes	
Controls× Small Firms		Yes			Yes	
Obs.		27958			27958	
No. of Firms		2644			2644	

Table 8: Number of Lenders and Contract Enforcement Reforms: Ordered Logit

This table provides results from regressions investigating the effect of Debt Recovery Tribunals on the number of banks that a firm borrows from. Since the number of banks a firm borrows from is measured using an ordinal scale, we use an ordered-logit specification. We include borrower-specific time varying control variables that include $(\frac{EBIT}{Sales})_{t-1}$, $(\frac{Cash}{TA})_{t-1}$, Tangibility$_{t-1}$, Interest Coverage$_{t-1}$ and a set of 100 dummy variables that identify firm size percentiles based on $Size_{t-1}$. The model also includes state fixed effects and year fixed effects. We only report coefficients on *Post-DRT(s)* for s=0,1,2 and 3 to conserve space. We also re-run same model with all the RHS variables interacted with *Large* and *Small* dummies to estimate the differential effect on large vs. small firms. Standard errors reported in parentheses are cluster adjusted at state level, as suggested by Bertrand, Duflo and Mullianathan (2004). * , **, and *** indicate statistical significance at 10%, 5%, and 1% level respectively. The base sample includes all non-Government, non-foreign, non-financial, and non-utility firms from *Prowess*.

	Number of Banks			
	All Firms	Large Firms	Small Firms	Diff
	(1)	(2)	(3)	(3)-(2)
DRT(t=0)	-.026	.072	-.128	-.2
	(.040)	(.048)	(.100)	(.103)
DRT(t=+1)	-.112	.153	-.253	-.406
	(.040)***	(.088)*	(.114)**	(.143)***
DRT(t=+2)	-.078	.261	-.223	-.484
	(.060)	(.134)*	(.123)*	(.144)***
DRT(t=+3)	-.154	.317	-.428	-.746
	(.073)**	(.140)**	(.223)*	(.198)***
Controls	Yes		No	
State FEs	Yes		No	
Year FEs	Yes		No	
State FEs× Large Firms	No		Yes	
State FEs× Small Firms	No		Yes	
Year FEs× Large Firms	No		Yes	
Year FEs× Small Firms	No		Yes	
Controls× Large Firms	No		Yes	
Controls× Small Firms	No		Yes	
Obs.	29411		21406	
No. of Firms	4163		2252	

Table 9: Asset maturity and Contract Enforcement Reforms

This table provides results from regressions investigating the effect of Debt Recovery Tribunals in a given state on a firm's asset structure. We estimate a model similar to (1) with Fixed Assets (as a proportion of total assets) and Current Assets (as a proportion of total assets) as the dependent variables. Fixed assets include *Gross* Land, buildings, plant and machinery. We include borrower-specific time varying control variables that include $(\frac{EBIT}{Sales})_{t-1}$, Interest Coverage$_{t-1}$ and a set of 100 dummy variables that identify firm size percentiles based on $Size_{t-1}$. To estimate the differential effect for large vs. small firms, we interact all RHS variables with *large* and *small* dummies. Standard errors reported in parentheses are cluster adjusted at state level, as suggested by Bertrand, Duflo and Mullianathan (2004). * , **, and *** indicate statistical significance at 10%, 5%, and 1% level respectively. The sample includes all non-Government, non-foreign, non-financial, and non-utility firms from *Prowess*.

	Fixed Assets/TA				**Current Assets/TA**			
	All	Large	Small	Diff	All	Large	Small	Diff
	(1)	(2)	(3)	(3)-(2)	(5)	(6)	(7)	(7)-(6)
DRT(t=0)	.004	-.002	.010	.012	-.008	-.001	-.018	-.016
	(.005)	(.006)	(.009)	(.007)	(.005)*	(.005)	(.009)**	(.007)**
DRT(t=+1)	.006	-.011	.015	.026	-.011	.004	-.026	-.03
	(.006)	(.010)	(.007)**	(.01)***	(.006)**	(.008)	(.009)***	(.01)***
DRT(t=+2)	.012	-.007	.017	.024	-.015	-.0005	-.027	-.027
	(.007)*	(.012)	(.007)**	(.011)**	(.007)**	(.008)	(.009)***	(.01)***
DRT(t=+3)	.018	-.015	.026	.041	-.019	.004	-.034	-.038
	(.010)*	(.015)	(.009)***	(.013)***	(.011)*	(.014)	(.014)**	(.016)**
Firm FEs	Yes		Yes		Yes		Yes	
Year FEs	Yes		No		Yes		No	
Controls	Yes		No		Yes		No	
Year FEs× Large	No		Yes		No		Yes	
Year FEs× Small	No		Yes		No		Yes	
Controls× Large	No		Yes		No		Yes	
Controls× Small	No		Yes		No		Yes	
Obs.	44701		26841		44701		26841	
No. of Firms	5761		2528		5761		2528	

Table 10: Asset maturity and Debt Maturity

This table provides results from regressions investigating the effect of DRT establishment on the asset structure of firms that changed their debt maturity vs. those that did not. We explicitly divide firms into two groups – those that experience an increase in long-term debt (LTD) one year after the establishment of DRTs, and those that do not. Within each group, we estimate a model similar to (1) with Fixed Assets (as proportion of total assets) and Current Asset (as proportion of total assets) as the dependent variables. Fixed assets include Gross Land, buildings, plant and machinery. We include borrower-specific time varying control variables including $(\frac{EBIT}{Sales})_{t-1}$, Interest Coverage$_{t-1}$ and a set of 100 dummy variables that identify firm size percentiles based on $Size_{t-1}$. Standard errors reported in parentheses are cluster adjusted at state level, as suggested by Bertrand, Duflo and Mullianathan (2004). * , **, and *** indicate statistical significance at 10%, 5%, and 1% level respectively. The sample includes all non-Government, non-foreign, non-financial and non-utility firms from *Prowess*.

	Fixed Assets/TA		**Current Assets/TA**	
	Δ LTD ≤ 0	Δ LTD > 0	Δ LTD$\leq$0	Δ LTD$>$0
	(1)	(2)	(3)	(4)
DRT(t=+1)	-.009	.015	.004	-.024
	(.009)	(.007)**	(.008)	(.008)***
DRT(t=+2)	-.008	.027	-.001	-.031
	(.011)	(.008)***	(.009)	(.010)***
DRT(t=+3)	-.012	.037	.002	-.042
	(.015)	(.011)***	(.013)	(.014)***
Year FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Obs.	11711	16140	11711	16140
No. of Firms	5751	4874	5751	4874

Figure 1: Establishment Pattern of DRTs

The figure below provides detailed information on the date, location and jurisdiction of DRTs established in different states of India under The Recovery of Debt Due to Banks and FIs (RDDB) Act, 1993. The arrows are targeted to states in which DRTs were established, while the similar colored areas provide the jurisdiction of the DRTs

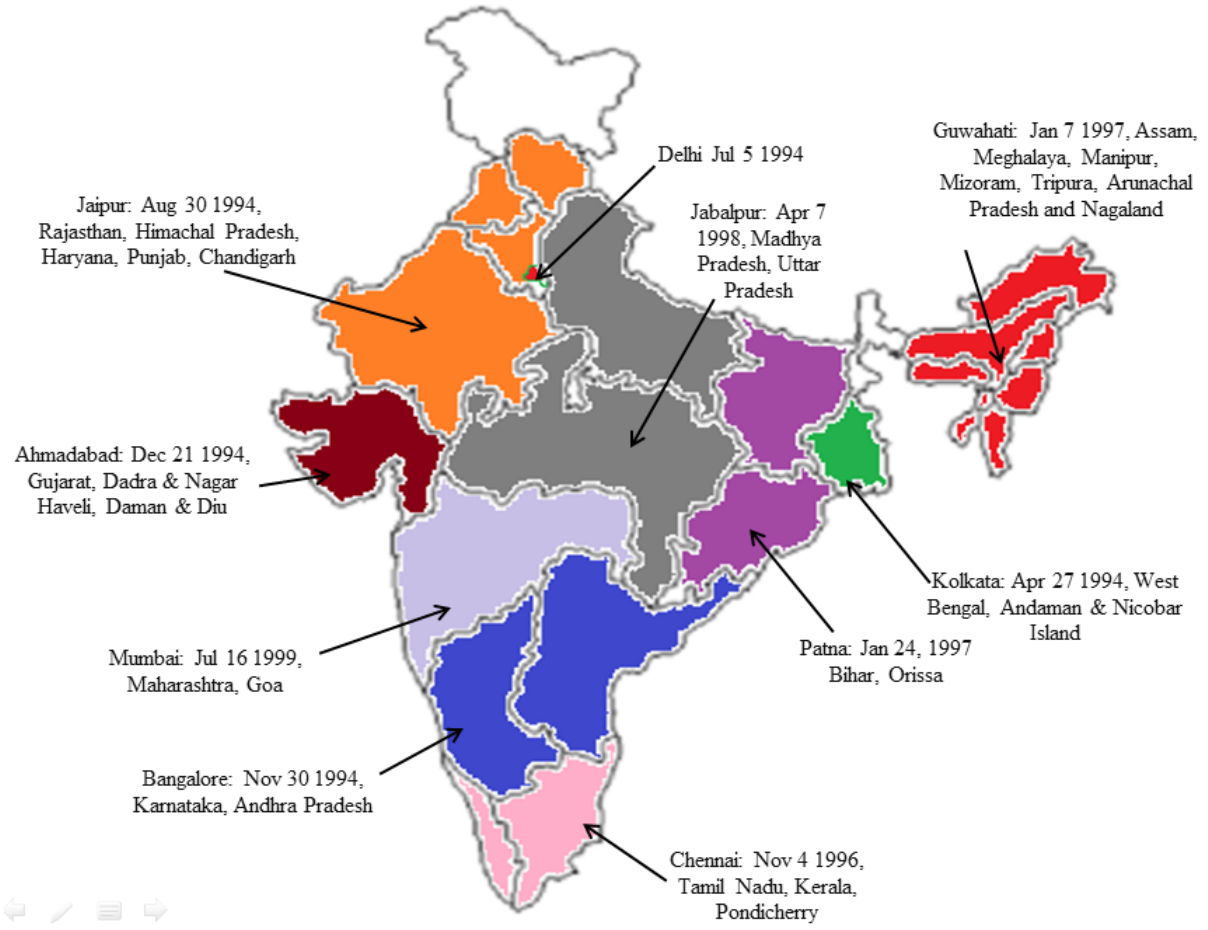
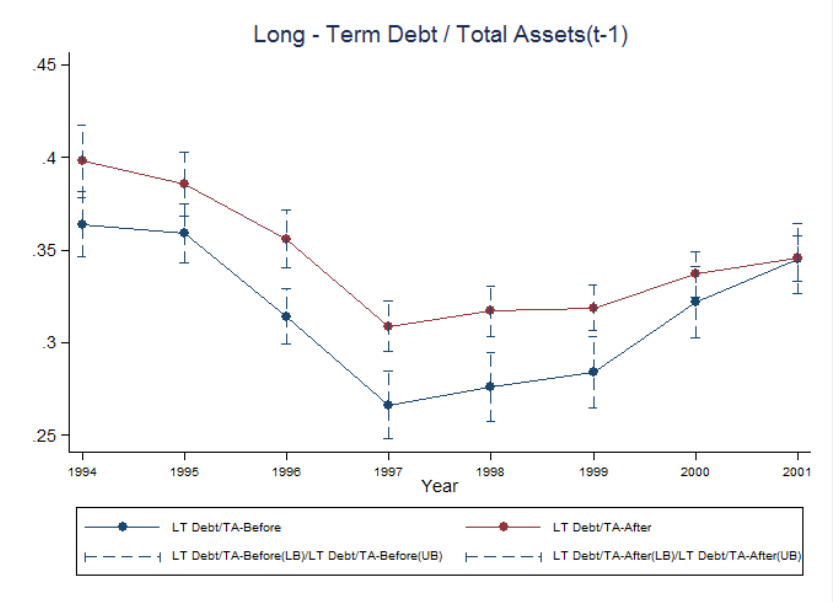


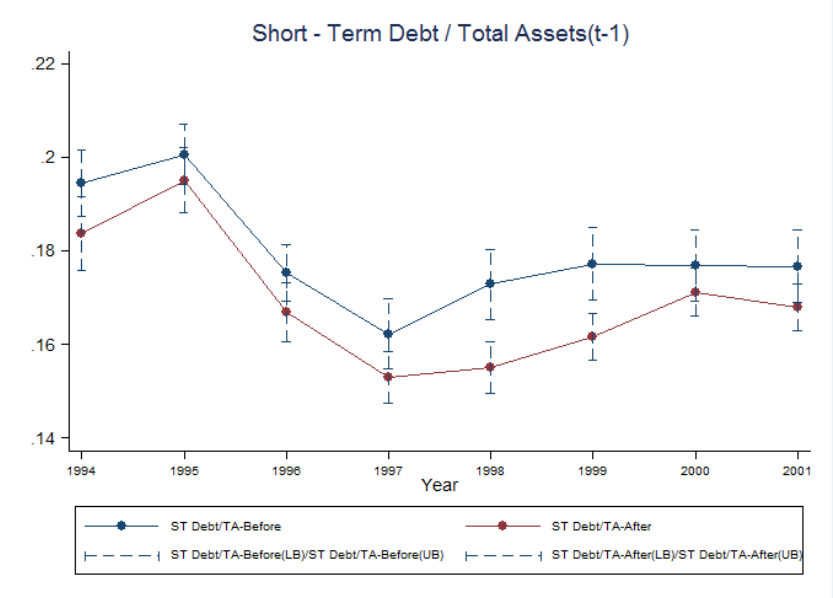
Figure 2: Debt Maturity and Contract Enforcement Reforms: Average Effect

The figures plot the mean and corresponding 95% confidence interval for (a) Long-Term Debt/Total Assets(LTD/TA), (b) Short-Term Debt/Total Assets(STD/TA), (c) Trade Credit/Total Assets (TC/TA) and (d) Short1 for the sample firms before and after the establishment functioning Debt Recovery Tribunals in firm's state of jurisdiction for the period 1994 to 2001.

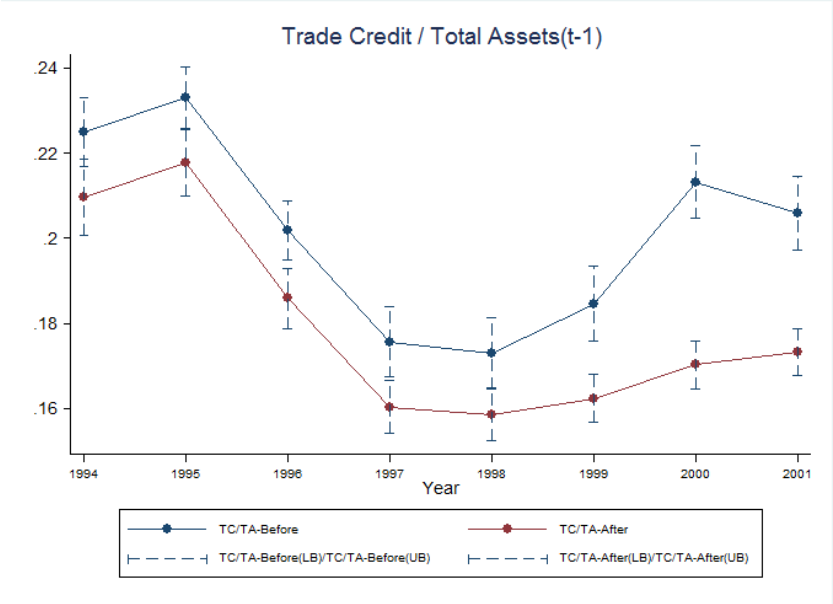
(a) Long-Term Debt/Total Assets



(b) Short-Term Debt/Total Assets



(c) Trade Credit/Total Assets



(d) Short 1