Do Debt Contract Enforcement Costs Affect Financing and Asset Structure?

Radhakrishnan Gopalan
Washington University in St. Louis

Abhiroop Mukherjee
Hong Kong University of Science and Technology

Manpreet Singh
Georgia Institute of Technology

Using the staggered introduction of fast-track debt recovery courts in India, we estimate the causal effect of a reduction in debt contract enforcement costs 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 lenders, have abnormally short debt maturity structures before the reform, and to smaller firms. Firms also reduce the number of banking relationships, and increase the proportion of long-term assets after the reform. (*JEL* F34, G32, G33, G38, K42)

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The globalization of financial markets has led to considerable interest in understanding how difficulties in financial contract enforcement affect firm financing. Lenders in many countries have to incur costs to enforce their contracts, and these costs can reduce the amount they recover when firms default. This, in turn, is likely to increase the cost of external debt finance, and, in extreme cases, can even lead to a complete breakdown of the market

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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, corporate bankruptcies in India take on average six years to resolve, and during that time firms enjoy a complete moratorium on all debt payments (Gopalan, Nanda, and Seru 2007). These enforcement costs can affect not only the overall availability of finance but also the composition of finance in terms of short-term versus long-term debt, and formal versus informal finance. In this paper, we use the establishment of Debt Recovery Tribunals (DRTs) in India as a quasi-natural experiment – one that reduced the cost of legal enforcement of debt contracts in different states without any accompanying change in the underlying law – to estimate the causal effect of enforcement costs on a firm's debt structure. We study the effect of enforcement costs on the firm's debt maturity choice, the number of lenders from which it borrows, the use of trade credit, and the maturity of assets in which it invests. The advantage of our setting – also used by Lilienfeld-Toal, Mookherjee, and Visaria (2012) and Visaria (2009) to study the effect of enforcement costs on the size of new loans and their interest rates – is that it offers an opportunity to study exogenous, within-country, staggered changes in enforcement costs, thereby allowing researchers to control for all country-level factors and estimate the causal effect of enforcement costs on firm financing.

The first strand of theoretical literature which motivates our analysis is the one that links enforcement costs to debt maturity choice. Douglas Diamond's presidential address to the American Finance Association in 2004 (Diamond 2004) is a good summary analysis of this literature. Diamond (2004) 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 as in Diamond and Dybvig (1983) – may provide incentives for the other lenders to monitor the firm and sustain external finance. This would predict that a reduction in enforcement costs should be accompanied by an increase in firm's debt maturity and a decrease in the number of lenders from which a firm borrows. The second strand of literature we refer to is the one on trade credit. This literature highlights the importance of informed lending (such as supplier credit) in weak enforcement environments. 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; Fabbri and Menichini 2010). To the extent such trade credit is costly (Petersen and Rajan 1994; De and Singh 2013), a decrease in enforcement costs will result in firms using less trade credit to finance their assets. We use our experimental setting to test these predictions.

To help Indian banks recover money from bad loans, in 1993, the Government of India (GoI) passed a national law to establish DRTs across the country, where banks and financial institutions could file suits against defaulted borrowers.

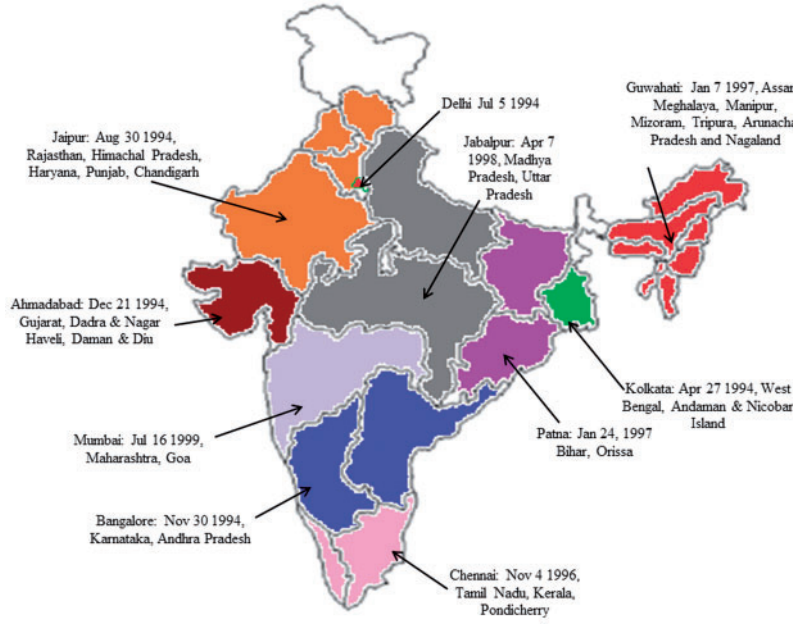

Figure 1
Establishment pattern of DRTs
The figure provides detailed information on the date, location and jurisdiction of DRTs established in different states of India under the Recovery of Debt Due to Banks and FIs (RDDB) Act, 1993. The arrows are targeted to states in which DRTs were established, and similar colored areas indicate the jurisdiction of a DRT.

Each DRT had jurisdiction over firms registered in a set of neighboring states, and individual states did not have the authority to choose whether 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. By 2000, all Indian states had access to a DRT. Figure 1 lists on a map of India the dates on which DRTs were established in different states. The delay due to the legal challenge provides us with plausibly exogenous variation in the presence of DRTs across Indian states. We exploit this variation and compare the behavior of firms in states that received access to a DRT early with those in states that received access later.

The assumptions we make for identification are twofold. First, we assume that DRTs significantly reduced contract enforcement costs. Visaria (2009) documents that debt recovery cases took much less time to be processed in a DRT than in a civilian court. Second, we assume that the delay in establishment of DRTs was exogenous to firm's debt and asset structures. 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 DRTs was caused by a legal challenge in one province from lawyers who were concerned with the DRTs circumventing the civilian courts. Moreover, as we show in Table 1 in the Internet Appendix (IA), the timing of the adoption of DRTs cannot be explained by observable differences in macroeconomic characteristics or average firm characteristics across states. Further evidence on the exogeneity of this setting is contained in Lilienfeld-Toal, Mookherjee, and Visaria (2012) and Visaria (2009).

We find that within four years following the implementation of a DRT, the proportion of total assets financed with long-term debt increases by 11.9%, while the proportion of assets financed with short-term debt decreases by 11.3% relative to their respective sample means. In sum, the ratio of short-term debt to total debt (*Short*) declines by 14.7% within four years after DRT establishment. Consistent with Diamond (2004), we also document a 9.1% decline in the odds ratio of a firm having multiple banking relationships after DRT. We find that our results are robust to a variety of alternative specifications, including the addition of various controls and industry-year fixed effects. Along with the change in debt maturity, we also document a lower reliance on trade credit, consistent with the prediction above. We find that trade credit usage (measured as $\frac{TradeCredit}{Assets}$) declines by 6.4% from its sample mean within four years after DRT.

In Diamond (2004), short-term debt is effective in addressing the problem of weak enforcement only if the firm borrows from multiple lenders who cannot coordinate their actions.[1] This would imply that the change in debt maturity following DRT implementation should be greater among firms that had been borrowing from multiple lenders before the reform. Consistent with this, we find that the increase (decrease) in long-term (short-term) debt after DRT implementation is confined to the sub-sample of firms that had multiple banking relationships in the pre-DRT period. Firms with multiple banking relationships experience a 18.7% decrease in *Short* in the four years after establishment of a DRT. On the other hand, firms with a single banking relationship in the pre-DRT period do not experience any significant change in any of our debt maturity variables.

Next, we examine whether the change in debt maturity following DRTs was greater among firms particularly constrained in accessing long-term financing, that is, firms that had abnormally short debt maturity structures before the reform. Our results support this hypothesis. Also, consistent with DRTs enabling

[1] 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, coordination may be difficult, and the threat of a co-lender holding out may prevent renegotiation, precipitating a lender run.

firms to access formal long-term finance, we find that the reduction in the usage of trade credit is also confined to this group of firms.

Next, we distinguish between small and large firms to test for a differential response. To the extent agency costs are greater for large firms, such firms could especially benefit from a reduction in enforcement costs and reduce their reliance on short-term financing and trade credit. On the other hand, implicit enforcement mechanisms, such as reputation concerns, may be less effective for small firms (Gopalan, Nanda, and Seru 2007), and hence it is also possible that these firms would benefit more from improvements in formal enforcement mechanisms. Our evidence is consistent with the latter view. We find that the increase in debt maturity and the decrease in trade credit is greater among small firms. For example, firms with below median book value of total tangible assets in the pre-DRT period experience a 9.4% decline in trade credit, while there is no significant change for larger firms.

Consistent with firms with multiple lenders and those with abnormally short debt maturity structures in the pre-DRT period benefiting most from the introduction of DRTs, we find the stock price of such firms increase (decrease) on the days with positive (negative) news about the likelihood of DRT implementation.

We also find that the transition from multiple to a single banking relationship is more likely to occur among firms with abnormally short debt maturity in the pre-DRT period. This is consistent with equilibrium predictions in Diamond (2004), where short maturity and multiple lenders *jointly* overcome enforcement problems. Consistent with our debt structure results, small firms are also more likely to transition to a single banking relationship.

If firms try to match the maturity of their assets and liabilities (Myers 1977; Stohs and Mauer 1996; Milbradt and Oehmke 2015), say, due to collateral requirements from lenders, the increase in long-term debt and the decrease 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 as a proportion of total assets increase by 3.5% compared with its sample mean, with a greater increase among small firms and those with abnormally short debt maturity structure in the pre-DRT period.

Overall, we make three main contributions in this paper. First, we use a quasi-natural experiment to estimate the *causal effect of enforcement costs* on firm financing. Consistent with Diamond (2004), our results highlight that reducing enforcement costs has a significant effect on firm's debt maturity and number of lenders. The increase in debt maturity occurs not only through an increase in the amount of long-term debt, but also through a simultaneous decrease in the amount of short-term debt. Second, we provide causal evidence that firms decrease reliance on trade credit when contract enforcement improves. This highlights that trade credit may be popular as an alternative source of finance,

inspite of its high cost, partly due to problems with enforcing formal "arms-length" financial contracts.[2] Overall, our evidence complements the findings in Lilienfeld-Toal, Mookherjee, and Visaria (2012), by showing that the increase in the size of long-term loans they document following DRT implementation is accompanied by a decrease in the amount of short-term debt and trade credit. This is important, because substitution between short-term and long-term debt is a major difference between Diamond (2004) and other models of debt (e.g., Lilienfeld-Toal, Mookherjee, and Visaria 2012; Vig 2013). Third, we also document that a reduction in debt contract enforcement costs is associated with a reduction in the number of banking relationships a firm has and an increase in the proportion of long-term assets on a firm's balance sheet. The latter result 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, we draw the reader's attention to the generalizability of our results to other contexts and countries. Although we conduct our study in India, we believe that our results are, to a large extent, 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 its ratio of private credit to GDP is 0.3, as compared to a world average of 0.418 (Djankov, McLiesh, and Shleifer 2007). India's creditor rights index value is 2, as compared with a world average of 1.787, and, on average, it takes 425 days for contract enforcement in India compared with a world average of 391 days (Djankov, McLiesh, and Shleifer 2007).

1. Related Literature

The three strands of literature related to our study are the papers that study law changes in India, the literature on debt maturity choice, and the literature on trade credit.

Our paper is related to Vig (2013), who studies the effect of the SARFAESI Act (Securitization and Reconstruction of Financial Assets and Enforcement of Security Interests Act 2002) on firm financing. In contrast to our findings, Vig (2013) finds that the reform was associated with a reduction in secured debt, total debt, debt maturity, and asset growth, and an increase in liquidity hoarding by firms. Strengthening creditor rights can either expand the contracting space (e.g., by allowing long-term debt contracts as in Diamond 2004) or increase the threat of premature liquidation, prompting firms to avoid debt. From the evidence in both papers, it appears that while the first force dominated following DRTs, the second dominated following SARFAESI Act.

[2] According to De and Singh (2013), the cost of trade credit for small firms in India can be as high as 23%.

The setting in our paper is similar to that in both Visaria (2009) and Lilienfeld-Toal, Mookherjee, and Visaria (2012) (LMV), although the question tackled is different. Visaria (2009) documents a decrease in delinquency and a decrease in loan interest rates (conditional on borrower quality) after DRT implementation. This is consistent with DRTs improving debt contract enforcement, an assumption we make. LMV show that following DRT implementation, the size of new loans is larger (smaller) among large (small) firms. LMV do not examine debt maturity and the model in their paper would predict an increase in the size of both short-term and long-term loans following DRT. Our focus, however, is on debt maturity, and we document an increase in the amount of long-term debt accompanied by a decrease in short-term debt. In addition, we examine the effect of DRT on the number of lenders from which a firm borrows, trade credit, and the types of assets in which a firm invests – variables that have generated considerable interest in recent theory, as well as in policy work, especially in developing countries.

Our paper is also related to recent papers that study debt structure in a cross-country setting (Qian and Strahan 2007; Bae and Goyal 2009). In comparison to these papers, we use a natural experiment to identify the *causal* effect of contract enforcement costs on debt maturity structure. 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 (2004). Moreover, we also examine the effect of enforcement on the *entire* debt structure of firms, *including trade credit* – a substantial part of firm financing in poor enforcement environments.[3]

The third strand of literature we add to is that on trade credit. While we discuss the theoretical literature in Section 2.1, the empirical papers that have studied the role of creditor protection include Petersen and Rajan (1997), Fisman and Love (2003), and Allen et al. (2012). The main distinguishing feature of our paper is the use of a natural experiment to establish the causal effect of legal enforcement costs on trade credit usage.

2. Mechanism and Background about the DRT Law

2.1 Outline of the mechanism

If contract enforcement is costly, upon borrower default, a single lender may choose to renegotiate debt as opposed to taking the borrower to court. Anticipating this, borrowers may divert firm cash flows with impunity in the knowledge that such diversion will not become public through a court process.

[3] Our paper also contributes to the general literature on debt structure choice. The theoretical literature – and empirical evidence – highlights the role of risk, asymmetric information, and asset liquidation values in determining the firm's debt maturity structure (Flannery 1986; Diamond 1991; Hart and Moore 1994; Berglöf and Von Thadden 1994; Benmelech 2005; Titman and Wessels 1988; Barclay and Smith 1995; Stohs and Mauer 1996; Guedes and Opler 1996; Berger et al. 2005; Goyal and Wang 2013; Saretto and Tookes 2013).

This is what Diamond (2004) calls "lender passivity," and it may, in extreme cases, ex ante preclude firm financing.

Diamond (2004) proposes a possible solution to this problem. If the firm borrows from multiple (say, two) lenders, then on default, it may face a credible threat of a court case as 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 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, create the right ex ante incentives for the borrower.

If enforcement becomes easier, the sustainability of financing does not depend as critically on having short-term debt from multiple lenders as before. If the short-term-debt-from-multiple-lenders solution involves costs, say, in terms of limiting the firm's ability to renegotiate better terms when its credit quality improves (Roberts and Sufi 2009), or exposing the firm to rollover risk (He and Xiong 2009), a decrease in enforcement cost should, ceteris paribus, make firms increase (decrease) the amount of long-term (short-term) debt in their financing mix and also reduce the number of lenders from which they borrow.[4] This also yields an interesting cross-sectional prediction: since short-term debt is a solution to weak enforcement only in the presence of multiple lenders, we expect the change in debt maturity to be concentrated among firms that borrow from multiple-lenders to begin with.

Theory also highlights the unique role of trade credit in helping firms overcome credit rationing due to information and agency problems (Schwartz 1974; Stiglitz and Weiss 1981). Not only can trade creditors hold up borrowers by withdrawing supply of raw materials, they also have an advantage when it comes to realizing value from the collateral (Fabbri and Menichini 2010), and may even be better informed about the borrower's future prospects. Thus, environments with high enforcement costs may increase a firm's reliance on trade credit. To the extent trade credit is an expensive source of finance (Petersen and Rajan 1994; De and Singh 2013), improvements in enforcement may be accompanied by a reduction in firms' reliance on trade credit.

In terms of other cross-sectional predictions, if there is heterogeneity among firms in terms of (unobserved) agency costs, arising from, say, the availability of internal funds (Holmstrom and Tirole 1997) or reputation with lenders (Gopalan, Nanda, and Seru 2007), then we expect firms with greater

[4] Apart from Diamond (2004), other papers have also identified the disciplining role of short-term debt, most notably Diamond (1991). Furthermore, other authors have also long recognized the ex ante benefits of borrowing from multiple lenders, for example, Bolton and Scharfstein (1996). Diamond (2004) puts these two together to highlight their *joint effect* in overcoming weak enforcement.

"perceived" agency problems to borrow short-term debt from multiple lenders and rely on trade credit. A decrease in enforcement costs should especially affect the debt structure of such firms. Since we do not observe *perceived* agency costs, we try to infer this from theoretical predictions on a firm's equilibrium debt structure. We classify firms with abnormally short debt maturity structures in the pre-DRT period as facing greater constraints due to weak enforcement. Thus, we expect these firms to experience a greater increase (decrease) in debt maturity (trade credit) on implementation of DRT.

Theoretically, changes in maturity structure and the use of trade credit following implementation of DRT may be more or less pronounced for small firms. To the extent agency costs may be greater for large firms, such firms should especially benefit from a reduction in enforcement costs and reduce their reliance on short-term financing and trade credit. On the other hand, to the extent that alternate enforcement mechanisms, such as reputation concerns, are less effective for small firms (Gopalan, Nanda, and Seru 2007), an improvement in formal enforcement should especially benefit small firms. From this viewpoint, one may expect the reduction in the use of short-term debt and trade credit to be greater for small firms. We design and employ tests to establish the empirical validity of these contrasting predictions.

The predictions from Diamond (2004) and the theories of trade credit are about the equilibrium financing mix, that is, the proportion of firm assets financed with a particular source of finance. In response to a fall in enforcement costs, a firm can change its financing mix and increase its debt maturity either by borrowing more long-term debt or by repaying short-term debt and trade credit or by simultaneously changing long-term debt, short-term debt and trade credit at different rates. Since data on incremental borrowing is not widely available and have to be inferred from the cash-flow statement (LMV), which is plagued by significant missing observations, we prefer to focus on debt listed on the firm's balance sheet to do our analysis. In our tests, we study the proportion of firm's assets financed with different financing sources and (changes in) the amount of the financing source on firm's balance sheet. Summarizing, we test Predictions 1–3.

Prediction 1. A decrease in enforcement costs should be accompanied by (1) an increase (decrease) in the amount of long-term (short-term) debt, (2) a decrease in the use of trade credit, and (3) a decrease in the number of lenders from which the firm borrows.

Prediction 2. The increase (decrease) in the amount of long-term debt (short-term debt) should be greater among firms that borrow from multiple lenders in the pre-DRT period.

Prediction 3. The increase (decrease) in the amount of long-term debt (short-term debt and trade credit) should be greater among (1) firms with abnormally short debt maturity in the pre-DRT period and (2) either large or small firms.

2.2 Background of the DRT law

Following economic reforms in India in the early 1990s, greater competition in the industrial sector along with stricter income recognition norms for banks 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% of the pending debt recovery cases in civil courts in 1985-1986 had been pending for longer than eight 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 Financial Institutions (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 streamlined procedures to allow cases to move through the process more quickly. Visaria (2009) contains a detailed description of the ways in which DRTs attempted to speed up the recovery process.[5] As documented in Table 3 in Visaria (2009), for a random sample of lawsuits 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 with the civil courts. To the extent that the early recovery did not impair the amount recovered, this is likely to decrease the loss incurred by lenders from loan defaults. Therefore, we interpret the introduction of a DRT in a state as a decrease in the cost of legal enforcement for firms in the treated regions.

The DRT law allowed the federal government to establish tribunals across the entire country and to determine their territorial jurisdiction. Importantly, individual states did not have the authority to choose whether to establish these tribunals and individual litigants did not have the authority to choose between a civil court and a DRT – the law required that all eligible open cases 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 response to a case filed by the Delhi Bar Association, the Delhi High Court ruled that the DRT law was not valid in 1994. This halted the establishment of new DRTs. The federal government appealed to the country's Supreme Court against this ruling, and,

[5] 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 a DRT. The DRT judges are usually retired civil court judges, familiar with standard legal procedure. The DRTs, much like Indian civil courts, allow for appeals against a judgment.

in 1996, the Supreme Court issued an interim ruling in favor of the law. This enabled the establishment of further DRTs. By 1999, most Indian states had received a DRT. Panel A of Table 1 in IA 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 31 in India. Thus, 1995 refers to the financial year ending on March 31, 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 the IA and find no such evidence.

3. Data and Summary Statistics

3.1 Data

We obtain data for our analysis from Prowess, a database constructed by the Center for Monitoring Indian Economy (CMIE), which has been used by a number of prior studies on Indian firms, including Bertrand, Mehta, and Mullainathan (2002), Gopalan, Nanda, and Seru (2007), and LMV. Prowess provides annual financial data and other descriptive variables for firms, including their industry classification, year of incorporation, and group affiliation. The data is in the form of 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. Prowess provides detailed information from the firm's balance sheet and income statements, including a detailed breakdown of the firm's liabilities, helping us identify the amount of short-term debt. According to the DRT law, a case can be assigned to a DRT located in the region in which the defendant resides, or where the cause of action arises, that is, the location where the defaulted loan is registered (Government of India 1988). Since loans are usually registered in the state in which the firm is located, we assign firms to DRT jurisdictions on the basis of the location of their registered office, which we also obtain from the same database.[6] We use the latest version of Prowess, which is free from survivorship bias, as highlighted by Siegel and Choudhury (2012). The lack of survivorship bias, along with the incremental expansion of firm coverage over time, results in a larger number of firm-year observations in our study compared with prior studies that use earlier vintages of Prowess.

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

[6] While our data does not allow us to observe changes in registered office locations, we do not believe many firms are likely to have changed the location of their registered office either to benefit from or to avoid exposure to DRT. When the RDDB Act was passed in Parliament, *all* states were supposed to get DRTs within a short time period. The stagger was produced by a court case. Given the uncertainty in the outcome of the case and the costs involved in changing registered offices, firms are unlikely to have moved their registered offices after the Delhi High Court's verdict.

governments, firms with less than three years of data with positive values for total assets and sales, and firms with leverage outside the [0,1] bound.[7] Finally, our daily stock market information also comes from Prowess.

3.2 Summary statistics

Table 1 provides summary statistics for the key variables used in our analysis. We have a total of 45,140 observations in our sample. We use three variables to measure the maturity of debt from banks and financial institutions: $\frac{LongTermDebt}{Assets}$, $\frac{ShortTermDebt}{Assets}$, and *Short*, where *Short* is the proportion of the book value of short-term debt to the book value of total debt. Along similar lines, we measure trade credit using $\frac{TradeCredit}{Assets}$. Short-term debt in Prowess is debt maturing within one year, while the typical trade credit contract involves an implicit maturity of three months (De and Singh 2013). We describe the construction of each variable in detail in Table A2. To prevent outliers from biasing our conclusions, we winsorize all variables of interest at the 1% and 99% levels and also constrain the variables that should lie between 0 and 1 to that range.[8]

The mean values of $\frac{LongTermDebt}{Assets}$, $\frac{ShortTermDebt}{Assets}$, and $\frac{TradeCredit}{Assets}$ are 21%, 15%, and 17%, respectively. The mean value of *Short* for our sample firms is 45%, and the median is 44%. To put the mean value of *Short* in context, Gopalan, Song, and Yerramilli (2014) 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 is 19.5%. To the extent that contract enforcement is more costly in India than in the United States, the higher value of *Short* among Indian firms offers preliminary evidence consistent with Diamond (2004). We also do not use any public bond data because public bonds were a negligible part of firm financing during the years of DRT implementation.[9]

The mean value of *Log(Total assets)* in our sample is 5.9, which translates into a book value of total assets of Rs. 3 billion. Firms in our sample are profitable, as seen from the mean value of $\frac{EBIT}{Sales}$ of 6%. Firms in India have a small amount of cash on their balance sheet as seen by the mean value of $\frac{Cash}{Assets}$ of 5%. On average, 35% 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 1.9, which is low compared with median interest coverage among Compustat firms of 4.6 reported in Gopalan, Song, and Yerramilli (2014). This indicates that the firms in our sample are highly levered. The proportion of fixed assets

[7] We include the last criteria because firms with book value of leverage greater than one are considered bankrupt in India and typically undergo debt restructuring with the Bureau of Industrial and Financial Restructuring (BIFR). The theories we test have less to comment about the change in debt structure for these firms. Gross leverage less than zero is likely a coding error, and hence we exclude these data points.

[8] To confine values to the 0-1 interval, we recode (46; 0) observations for $\frac{LongTermDebt}{Assets}$ (on either end), (0; 126) observations for $\frac{TradeCredit}{Assets}$, and (0; 46) observations for *Short*. We repeat our tests after dropping observations instead of recoding and obtain results consistent with those reported.

[9] For example, according to the Reserve Bank of India's Handbook of Statistics on the Indian Economy, there were only twelve bond issues by Indian corporates for the financial year 1998-1999.

Table 1
Summary Statistics

	N	Median	Mean	SD
$\frac{LongTermDebt}{Assets}$	45,140	0.16	0.21	0.19
$\frac{ShortTermDebt}{Assets}$	45,140	0.13	0.15	0.12
$\frac{TradeCredit}{Assets}$	45,140	0.13	0.17	0.14
Short	45,140	0.44	0.45	0.30
Long-term debt	45,140	65.30	617.08	5574.80
Short-term debt	45,140	50.70	317.68	1792.87
Total debt	45,140	137.20	934.76	6734.12
Trade credit	45,140	58.30	401.83	3502.22
DRT	45,140	1.00	0.88	0.32
$Size_{t-1}$	45,140	5.93	6.10	1.60
$\frac{EBIT}{Sales}_{t-1}$	45,140	0.07	0.06	0.31
$\frac{Cash}{Assets}_{t-1}$	45,140	0.03	0.05	0.08
$Tangibility_{t-1}$	45,140	0.33	0.35	0.20
$Interest\ coverage_{t-1}$	45,140	1.89	4.52	12.86
$\frac{FixedAssets}{Assets}$	42,746	0.42	0.43	0.22
Number of banks	27,928	2.00	3.19	3.07
Single	17,474	0.00	0.32	0.47
Abnormal	26,344	0.00	0.29	0.46
Large	26,344	1.00	0.50	0.50

This table reports descriptive statistics for our sample of firms. All variables are defined in Table A2. From the overall Prowess sample for 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 with leverage outside the [0,1] range.

to total assets is 43%. The median firm in our data borrows from two banks, as seen from the median value of *Number of banks*.[10]

From the mean value of *Single,* we find that 32% of sample firms borrow from a single bank in the year before DRT implementation in their state. *Abnormal* identifies firms with abnormally short debt maturity structures before the reform, that is, firms with below-median industry-adjusted $\frac{LongTermDebt}{Assets}$ and above-median industry-adjusted $\frac{ShortTermDebt}{Assets}$. *Large* identifies firms with above median book value of total assets. Our results are not sensitive to the cutoffs used for either *Abnormal* or *Large.*

4. Empirical Results

4.1 Investigating the timing of DRT establishment across states

In our empirical tests, we compare firms from states that got DRTs early to firms from states that got DRTs later. Our tests will correctly identify the causal effect of DRT implementation on financing only if DRT timing is not systematically related to state or firm characteristics. While the background of the DRT law and

[10] Note that we have information on the number of banking relationships for only a subsample of firms. Please see footnote 14 for details.

its implementation, discussed in Section 2.2, suggests that this is a reasonable assumption, to verify this, in the IA, we relate state characteristics to the timing of DRT using a hazard model (panel A of Table 1). We find that the extent of state-level judicial shortfall, that is, the ratio of unfilled judicial positions in the state scaled by the total number of approved positions, is the only variable weakly related to the timing of DRT – the states that received DRTs early had a greater fraction of judicial shortfall compared with the other states. We find no systematic differences in the average firm financing structure between the early and later DRT states (panel B). Since these results are similar to those reported in LMV, we report them in the IA.

4.2 Effect of DRT on debt structure

We begin our empirical analysis by documenting how firms' debt structure changes around DRT establishment. Our main dependent variables are $\frac{LongTermDebt}{Assets}$, $\frac{ShortTermDebt}{Assets}$, *Short*, and $\frac{TradeCredit}{Assets}$. Panels (A)-(D) of Figure 2 present the mean values of these four variables of interest, along with the 95% confidence intervals, for the period 1993-2001 (two years before any state received a DRT, to one year after all states had access to a DRT). The red (solid) line indicates the average value for firms in states with a DRT, and the blue (dash-dotted) line indicates averages for firms in states without a DRT. During the pre-DRT years (1993–1994), we include the first group of states that got a DRT in the financial year 1995 in the red (solid) line, to see whether there are any discernible differences (pre-trends) between these early-DRT states and their late-DRT counterparts. Since the DRTs were introduced in a staggered manner, we carefully account for the change in the composition of states in the two groups as more states got DRTs: for the years 1995–1999, whenever a state got a DRT, we remove it from the blue (dash-dotted) line and include it in the red (solid) line. For 2000 and 2001, only the last set of states to get a DRT remain as part of the blue (dash-dotted) line. We expect to see two patterns in our graphs: first, no pre-trends, that is, no significant difference between early and later DRT states pre-1995. Second, we expect the red (solid) line to diverge from the blue (dash-dotted) line as the red states get DRTs; eventually, we expect to see the two lines to converge back after 2000, when the few remaining states received access to DRTs.

The first thing to note from the figure is that all the four variables of interest, $\frac{LongTermDebt}{Assets}$, $\frac{ShortTermDebt}{Assets}$, *Short*, and $\frac{TradeCredit}{Assets}$, are statistically indistinguishable between the early and late DRT states during the pre-1995 period. This shows that there are no pre-trends in our data, which is important for the difference-in-difference analysis we conduct below. Second, the levels for each of these variables for firms in states that get a DRT starts to diverge from those of firms in states without a DRT after 1995. The difference between the levels of all variables between the treated and control states is statistically significant at the 5% level by 1998-1999. Finally, after all the states get access to a DRT in 2000, the lines start to converge back.

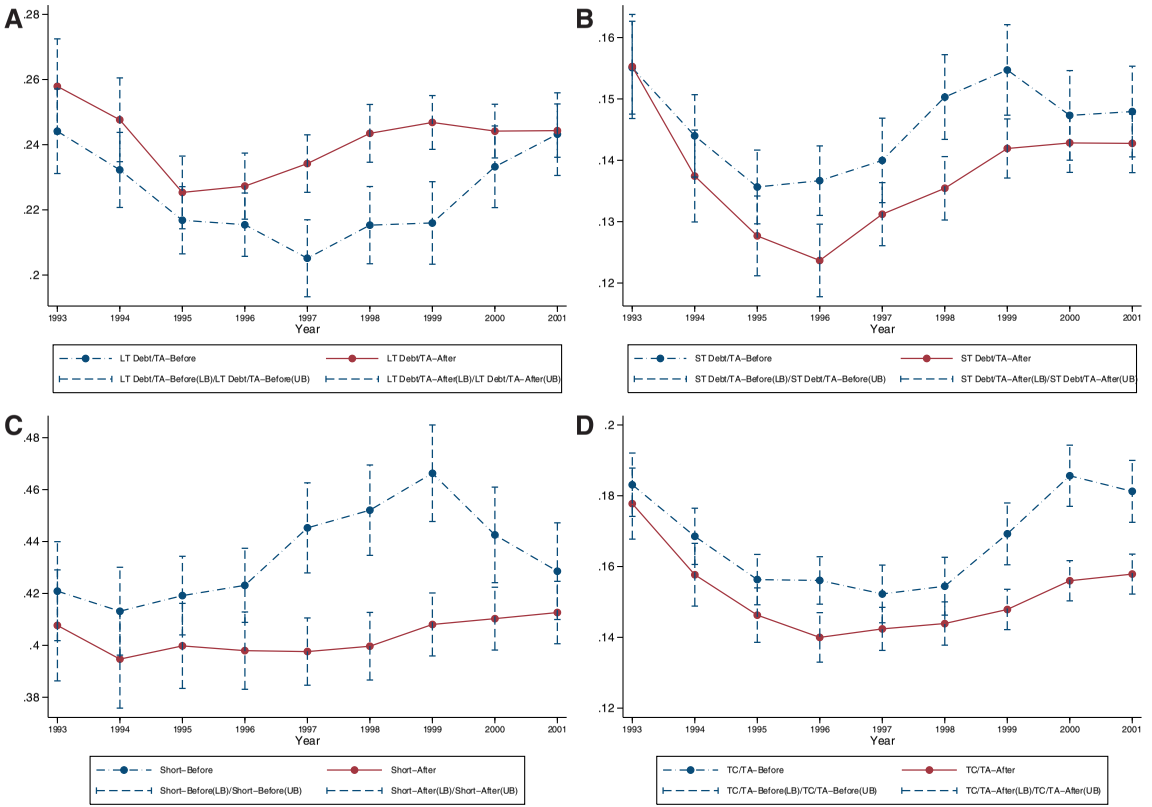

Figure 2
Debt maturity and contract enforcement reforms: Average effect

The figures show the means and corresponding 95% confidence intervals for (A) $\frac{LongTermDebt}{Assets}$, (B) $\frac{ShortTermDebt}{Assets}$, (C) *Short* and (D) $\frac{TradeCredit}{Assets}$ for the sample of firms from 1993-2001 (two years before any state received a DRT, to one year after all states had access to a DRT). The red (solid) line indicates the average value for firms in states with a DRT, and the blue (dash-dotted) line indicates averages for firms in states without a DRT. During the pre-DRT years (1993–1994), we include the first group of states that got a DRT in the financial year 1995 in the red (solid) line. As more states get DRTs from 1995–1999, we move it from the blue (dash-dotted) line to the red (solid) line. For 2000 and 2001, only the last set of states to get a DRT remain as part of the blue (dash-dotted) line.

Note that Figure 2 (C) indicates that, in 1997, *Short* increases among firms from states without a DRT, while there is no corresponding increase among firms from states with a DRT. While the differential response in the treated and control states is consistent with DRTs affecting debt maturity in the predicted direction, this pattern is interesting. One explanation for this pattern is that there was an overall increase in demand for debt capital around 1997. Firms in DRT states met this increase with a mix of short- and long-term debt, while firms in non-DRT states met it with short-term debt (due to the constrained availability of long-term debt for the latter group).[11] We control for such countrywide, as well as firm-specific, patterns in debt maturity in our multivariate difference-in-differences estimates with the inclusion of time and firm fixed effects.

We first test Prediction 1 by estimating the change in debt composition in the years preceding and following the establishment of a DRT. We do so by estimating the following fully saturated model:

$$y_{ijt} = \beta_0 + \sum_{s=-3}^{-2} \Gamma_s \text{Pre-DRT(s)}_{jt} + \sum_{s=0}^{12} \Gamma_s \text{DRT(s)}_{jt} + \gamma \times X_{it-1} + \delta_i + \delta_t + \varsigma_{s,t} + \varepsilon_{it}, \tag{1}$$

where the dependent variable y_{ijt} (i indexes firm, j is the state in which it is registered, and t the year of measurement) is either a measure of debt maturity or trade credit. *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 few firm-year observations more than two years before the first, and eleven years after the last 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 implementation. 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 before (after) the establishment of a DRT to the year immediately before its establishment. To conserve space, we present coefficients on DRT(t = −2) to DRT(t = +4) in the tables. We stop at DRT(t = +4) because the difference between early and late DRT states remains for at most five years (from 1995-2000), and most of our effects manifest within these five years.

The inclusion of firm fixed effects, δ_i, ensure that each indicator is estimated using only within firm variation in the dependent variable, and time dummies, δ_t, control for country-level trends. $\varsigma_{s,t}$ are state-year trends that help purge

[11] A possible reason for the increase in demand for external debt capital is the collapse of the public equity market in India in 1997 (Gopalan and Gormley 2013).

any underlying state-level linear trends in our variables of interest during the sample period (similar to LMV). In addition, we carefully control for firm size – which can potentially have nonlinear effects on maturity – using 100 dummy variables, one for each percentile of the size distribution. The standard errors are corrected for heteroscedasticity and autocorrelation, and are clustered at the individual state level (Bertrand et al. 2004).

Our identifying assumptions are twofold. First, we assume that DRTs significantly reduced contract enforcement costs. There is significant support for this assumption in Visaria (2009). Second, we assume that the delay in establishment of DRTs was exogenous to the financing structure of firms. The evidence in Figure 2, the tests in the IA (Table A1), and the lack of significance of the coefficient on *Pre-DRT(t = −2)* (discussed below) are all evidence in support of this assumption.

In panel A of Table 2 we provide the results of estimating (1) in our full sample. In Columns 1-4, we estimate the model without any controls. We find that the coefficients on *DRT*(t = +1) through *DRT*(t = +4) are positive and significant in Column 1, where $\frac{LongTermDebt}{Assets}$ is the dependent variable, and negative and significant in Columns 2 and 3, where $\frac{ShortTermDebt}{Assets}$ and *Short* are the dependent variables respectively. This shows that after implementation of DRT, there is an increase in the proportion of total assets financed with long-term debt and a *simultaneous decrease* in the proportion financed with short-term debt, leading to a strong overall increase in debt maturity (as measured by *Short*, our composite maturity variable). This is consistent with our univariate evidence and with the key prediction in Diamond (2004). Looking at the year after DRTs started functioning (coefficient on *DRT(t=+1)*), we find that relative to the year before establishment of a DRT, $\frac{LongTermDebt}{Assets}$ is higher by 0.011. In comparison, the mean value of $\frac{LongTermDebt}{Assets}$ in our sample is 0.21. Thus, our results indicate a 5.2% (0.011/0.21) increase in long-term debt as a proportion of total assets compared with the sample mean. Similarly, the negative and significant coefficient on *DRT (t=+1)* in Column 2 indicates a 3.3% (−0.005 versus sample mean of 0.15) decrease in short-term debt as a proportion of total assets, compared with the sample mean. In combination, both these changes result in a 7.6% (−0.034 versus sample mean of 0.45) decrease in *Short,* compared with the sample mean.

Note that the coefficient on *DRT(t = 0)* is significant in all the columns, and its magnitude suggests that about 50% of the eventual change in $\frac{LongTermDebt}{Assets}$ and about 23% of the eventual change in $\frac{ShortTermDebt}{Assets}$ occurs in the first year after DRT implementation. The immediacy of (a part of) the response is interesting and warrants some explanation. We believe there are two possible explanations for this. First, the coefficient on *DRT(t=0)* is estimated based on changes in debt composition both in the late and early DRT states in the year after DRT implementation relative to the year before DRT implementation. It could be the case that the changes happen much faster in the late DRT

Table 2
DRTs and debt composition, Baseline results

A. Scaled variables

	$\frac{LongTermDebt}{Assets}$	$\frac{ShortTermDebt}{Assets}$	*Short*	$\frac{TradeCredit}{Assets}$	$\frac{LongTermDebt}{Assets}$	$\frac{ShortTermDebt}{Assets}$	*Short*	$\frac{TradeCredit}{Assets}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DRT(t=−2)	0.0005	0.0007	0.0005	0.003	0.00007	0.0005	0.0006	0.003
	(0.005)	(0.001)	(0.006)	(0.002)	(0.005)	(0.001)	(0.006)	(0.002)
DRT(t=0)	0.011***	−0.004**	−0.031***	−0.001	0.011***	−0.004**	−0.031***	−0.001
	(0.004)	(0.002)	(0.007)	(0.003)	(0.003)	(0.002)	(0.007)	(0.003)
DRT(t=+1)	0.011**	−0.005*	−0.034***	−0.005*	0.012**	−0.005	−0.034***	−0.004*
	(0.005)	(0.003)	(0.012)	(0.003)	(0.005)	(0.004)	(0.012)	(0.002)
DRT(t=+2)	0.015**	−0.009**	−0.049***	−0.004	0.016**	−0.009*	−0.050***	−0.004
	(0.007)	(0.005)	(0.015)	(0.003)	(0.007)	(0.005)	(0.015)	(0.003)
DRT(t=+3)	0.022**	−0.014**	−0.061***	−0.008**	0.023**	−0.014**	−0.062***	−0.008**
	(0.009)	(0.006)	(0.021)	(0.003)	(0.010)	(0.006)	(0.021)	(0.003)
DRT(t=+4)	0.023***	−0.018**	−0.066***	−0.011***	0.025***	−0.017**	−0.066***	−0.010***
	(0.009)	(0.008)	(0.023)	(0.004)	(0.009)	(0.007)	(0.023)	(0.004)
$Tangibility_{t-1}$					0.136***	−0.037***	−0.172***	−0.038***
					(0.010)	(0.009)	(0.023)	(0.008)
$\frac{EBIT}{Sales}_{t-1}$					−0.043***	−0.014***	0.007	−0.025***
					(0.006)	(0.003)	(0.007)	(0.006)
$\frac{Cash}{Assets}_{t-1}$					−0.055***	−0.093***	−0.097***	−0.029**
					(0.018)	(0.010)	(0.021)	(0.014)
$Interest\ Coverage_{t-1}$					−0.0007***	−0.0007***	−0.0006**	−0.0002**
					(0.0001)	(0.00005)	(0.0003)	(0.00008)
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	Yes
State-year trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.61	0.572	0.552	0.659	0.624	0.581	0.555	0.663
Obs.	45,140	45,140	45,140	45,140	45,140	45,140	45,140	45,140
No. of firms	6,116	6,116	6,116	6,116	6,116	6,116	6,116	6,116

(*continued*)

Table 2
Continued

B: Unscaled variables

	Log(1+.)					
	Long-term debt	*Short-term debt*	*Trade credit*	*Long-term debt*	*Short-term debt*	*Trade credit*
	(1)	(2)	(3)	(4)	(5)	(6)
DRT(t=−2)	0.012	0.022	0.012	0.009	0.020	0.012
	(0.033)	(0.028)	(0.021)	(0.032)	(0.029)	(0.021)
DRT(t=0)	0.110***	−0.046	−0.030*	0.114***	−0.044	−0.030*
	(0.026)	(0.030)	(0.017)	(0.028)	(0.034)	(0.017)
DRT(t=+1)	0.124***	−0.065	−0.042*	0.125***	−0.066	−0.040*
	(0.043)	(0.045)	(0.023)	(0.043)	(0.049)	(0.023)
DRT(t=+2)	0.189***	−0.104*	−0.040	0.196***	−0.101	−0.040
	(0.051)	(0.061)	(0.034)	(0.053)	(0.062)	(0.035)
DRT(t=+3)	0.217***	−0.158**	−0.072**	0.215***	−0.163**	−0.070**
	(0.064)	(0.076)	(0.036)	(0.066)	(0.078)	(0.035)
DRT(t=+4)	0.221***	−0.194**	−0.070*	0.224***	−0.193**	−0.067*
	(0.073)	(0.096)	(0.042)	(0.078)	(0.096)	(0.041)
$Tangibility_{t-1}$				0.616***	−0.369***	−0.372***
				(0.051)	(0.115)	(0.047)
$\frac{EBIT}{Sales}_{t-1}$				−0.029	0.061*	−0.064**
				(0.033)	(0.031)	(0.028)
$\frac{Cash}{Assets}_{t-1}$				−0.871***	−10.342***	−0.539***
				(0.104)	(0.142)	(0.125)
Interest Coverage$_{t-1}$				−0.008***	−0.010***	0.0005*
				(0.0008)	(0.001)	(0.0003)
Size dummies	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
State-year trend	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.826	0.779	0.89	0.83	0.784	0.891
Obs.	45,140	45,140	45,140	45,140	45,140	45,140
No. of Ffirms	6,116	6,116	6,116	6,116	6,116	6,116

This table provides results from regressions estimating the effect of DRTs on firm's debt structure. We estimate the following regression equation for different dependent variables (y_{ijt}):

$$y_{ijt} = \beta_0 + \sum_{s=-3}^{-2} \Gamma_s \text{Pre-DRT(s)}_{jt} + \sum_{s=0}^{12} \Gamma_s \text{DRT(s)}_{jt} + \gamma \times X_{it-1} + \delta_i + \delta_t + \varsigma_{s,t} + \varepsilon_{it}.$$

Pre-DRT(s)(*DRT(s)*) is a dummy variable that takes a value one s years before (after) the establishment of a DRT in the firm's state, and zero otherwise. X_{it-1} is a set of borrower-specific time-varying control variables that include $\frac{EBIT}{Sales}_{t-1}$, $\frac{Cash}{Assets}_{t-1}$, $Tangibility_{t-1}$, *Interest Coverage*$_{t-1}$ and a set of 100 dummy variables that identify firm size percentiles based on $Size_{t-1}$. The coefficients on DRT(t=−2) to DRT(t=+4) and controls are reported. The model is fully saturated with the year immediately before the establishment of a DRT as the base category. The specification includes firm and time fixed effects and state-year trends. Standard errors are corrected for heteroscedasticity and autocorrelation and clustered at the state-level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The sample includes all non-missing observations for non-government, non-foreign, non-financial and non-utility firms from *Prowess*.

states given the experience with DRTs in the early states. Pent-up demand for debt financing can also go towards explaining the coefficient. Given the inefficiency of the debt recovery process prior to DRT establishment, banks (and borrowers) may have waited for DRT establishment before disbursing new loans.

Despite the significant coefficient on *DRT(t=0)*, we find that there is also a further monotonic increase in the size of the coefficients on DRT in Column 1. This suggests that some of the debt changes happen gradually. Four years after implementation of DRT, on average, long-term debt as a proportion of total assets is 2.3 percentage points higher relative to the year before DRT, which is a 11% increase relative to the sample mean. We observe a similar pattern for short-term debt in Column 2. When we examine $\frac{TradeCredit}{Assets}$ as the dependent variable, we find that there is a 6.4% (−0.011 versus sample average of 0.17) decrease in trade credit usage by the fourth year after DRT introduction. Consistent with a gradual change in trade credit usage, we find that the coefficients on the DRT dummies gradually decrease.

The gradual nature of the changes we document is consistent with the evidence in LMV, who also show a gradual increase in the size of new loans following implementation of DRT. LMV argue that the reason for the gradual change is the time involved in banks opening new branches to serve the increased demand. We believe there could be an additional reason for the gradual change. Since our subsequent results show that the changes in debt structure happen gradually even among firms with existing banking relationships, we believe the gradual changes may also be due to banks and firms learning about the actual (rather than promised) efficiency of DRTs.

In Columns 5-8, we repeat our tests after including control variables from prior literature (Barclay and Smith 1995; Berger et al. 2005; Guedes and Opler 1996; Stohs and Mauer 1996). To make sure our inclusion of control variables does not bias our estimates, we lag the control variables by a year. We control for firm profitability using $\frac{EBIT}{Sales}$, cash using $\frac{Cash}{Assets}$, and asset type using *Tangibility*. We measure *Tangibility* as the ratio of book value of property, plant and equipment to total assets. We also control for leverage using *Interest coverage*. All variables used in our analysis are defined in Table A2.

Comparing the results from Columns 5-8 to those in Columns 1-4, we find that inclusion of the control variables has a negligible effect on the size of the coefficients of interest. For example, the coefficient on DRT(t=+4) changes from -0.018 in Column 2 to −0.017 in Column 6. From the coefficients on the control variables, we find that firms with more tangible assets, less profitable firms, firms with less cash as a proportion of total assets, and firms with lower interest coverage have a higher $\frac{LongTermDebt}{Assets}$.

Interestingly, we find that the sign of the coefficients on the control variables are similar for short-term debt and trade credit (except that the coefficient on $\frac{EBIT}{Sales}$ is not significant for trade credit). Since the delay in DRT implementation is not correlated in a systematic manner with firm characteristics, the inclusion of control variables does not affect the size of the coefficient on the DRT dummies. To the extent the control variables help explain some residual variation in the outcome variable, their inclusion improves the efficiency of our estimates and makes our results more readily comparable to prior literature. Hence, in our subsequent tests, we present results that include control variables.

To put the economic magnitude of the effect in perspective, it is instructive to compare the effect of DRT with other well-known determinants of debt maturity choice. The effect of DRT on long-term debt is about the same as that of a 0.9 standard deviation change in tangibility (0.9*0.136*0.20= 0.0244), while it's effect on short-term debt, *Short*, and $\frac{TradeCredit}{Assets}$ is the same as that of a 2.2, 1.8 and 1.4 standard deviation change in tangibility, respectively. Furthermore, the magnitude of the effect from *DRT(t=+4)* is equal to at least a one-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. Although we estimate the marginal effect of the traditional determinants of leverage after controlling for firm and size-group fixed effects, to the extent that there is within firm and within size-group variation in the firm characteristic, marginal effects are consistently estimated. We also find that our results in panel A are robust along multiple dimensions (see Section 4.6 and Table 8 in the IA for a discussion of these robustness tests).

In panel B of Table 2, we measure the extent to which the changes in financing pattern observed in panel A are due to changes in the level of debt (the numerator of the dependent variables employed 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. As mentioned before, the predictions from Diamond (2004) are about the proportion of short-term debt firms use to finance their assets. When enforcement costs change, firms can change their financing mix either with or without incremental borrowing. Our tests in panel B are designed to understand the extent to which changes in the financing mix in panel A are due to changes in the amount of debt.

Our tests in panel B, Column 1, are similar to those in Table XX of LMV, but with a few important differences. First, our dependent variables are the amounts of long-term and short-term debt on firm's balance sheet, whereas the dependent variable in LMV is the gross amount of new long-term debt from the cash-flow statement with repayments coded as zero. Thus, they estimate the change in the size of new loans post-DRT. Changes in loan sizes may not translate to corresponding changes in loan amount on the balance sheet if loan repayments vary systematically post-DRT (see Section 4.4.1).

The results in Column 1 show that there is an immediate increase in the amount of long-term debt on firm's balance sheet after DRT compared with the year before. We further find that this increase is sustained for up to four years after DRT implementation. From Column 2, we find that there is a strong and sustained decrease in the amount of short-term debt on a firm's balance sheet after DRT. Our coefficients are economically significant. The coefficient on *DRT(t=+4)* in Column 1 indicates that four years after implementation of DRT there is a 22.1% increase in the amount of long-term debt on a firm's balance sheet, whereas there is a 19.4% decrease in the amount of short-term debt.

In Column 3 our dependent variable is *Log(1+ Trade credit),* and we find from the coefficient on *DRT(t=+4)* indicates that there is a 7% decrease in the level of trade credit four years after DRT implementation compared with the year before. Columns 4-6 report results after inclusion of control variables, and the results look similar. This evidence shows that at least some of the changes in debt structure are due to active changes in the amounts of the different types of debt.

4.3 Effect of DRT on the number of banks

In Table 3 we estimate a specification similar to (1) with the number of banking relationships as our main dependent variable. As the main distinction in Diamond (2004) is between single versus multiple banking relationships, we begin our analysis by differentiating between firms that borrow from a single bank and those that borrow from multiple banks, and use a probit model to test if firms are less likely to borrow from multiple banks after implementation of DRT. To avoid the incidental parameters problem (Neyman and Scott 1948), we do not include firm fixed effects. Instead, we include state fixed effects and continue to use time fixed effects and state-specific time trends as before. In Column 1, we do not include any controls, and in Column 2 we include the same controls as in Table 2. The negative and significant coefficients on the *DRT(s)* dummies indicate that fewer firms have multiple banking relationships after implementation of DRT. In terms of economic magnitudes, the coefficient on *DRT(t=+4)* indicates that, on average, the odds ratio of an average firm borrowing from multiple banks is 0.194 lower four years *after* DRT compared with the year *before* DRT (Column 1). This is a 9.1% decline in the odds ratio compared with the sample mean of 2.125.[12]

While the probit model is our preferred econometric specification given that the theory mainly distinguishes between single versus multiple banking relationships, we also present results of estimating an OLS (with firm fixed effects) and ordered logit model with the number of banking relationships as the dependent variable in Columns 3 and 4, respectively. The results in both columns are consistent with those from our probit model.

We find the results reported in Table 3 are robust to employing a logit model, inclusion of within-year industry fixed effects, inclusion of controls for state-level macroeconomic indicators and limiting the sample to firms in existence pre-DRT (please see Table 2 of the IA).

4.4 Cross-sectional tests

4.4.1 Debt structure. In Table 4 we test Prediction 2, and examine whether the changes in debt structure are more pronounced among firms that borrow

[12] From Table 1, row 17, we know that 32% of firms borrow from a single bank. This implies an odds ratio for a firm to borrow from multiple banks = (1 − 0.32)/0.32 = 2.125.

Table 3
Number of lenders

	Multiple banks		*Number of banks*	
	Probit	*Probit*	*OLS*	*Ordered logit*
	(1)	(2)	(3)	(4)
DRT(t=−2)	0.034	0.036	0.032	0.040
	(0.036)	(0.036)	(0.024)	(0.030)
DRT(t=0)	0.026	0.025	−0.048	−0.032
	(0.030)	(0.030)	(0.054)	(0.051)
DRT(t=+1)	−0.085	−0.085	−0.134*	−0.143**
	(0.053)	(0.053)	(0.079)	(0.072)
DRT(t=+2)	−0.067	−0.069	−0.169*	−0.111
	(0.055)	(0.057)	(0.101)	(0.087)
DRT(t=+3)	−0.142**	−0.144**	−0.278**	−0.227**
	(0.067)	(0.068)	(0.127)	(0.096)
DRT(t=+4)	−0.194**	−0.197**	−0.367**	−0.316***
	(0.083)	(0.084)	(0.148)	(0.119)
$Tangibility_{t-1}$		0.142	−0.319**	−0.278
		(0.138)	(0.136)	(0.189)
$\frac{EBIT}{Sales}_{t-1}$		−0.015	0.012	−0.017
		(0.045)	(0.039)	(0.053)
$\frac{Cash}{Assets}_{t-1}$		0.215	−0.854***	−0.005
		(0.264)	(0.302)	(0.372)
Interest Coverage$_{t-1}$		−0.0002	−0.005***	−0.003
		(0.002)	(0.001)	(0.003)
Size dummies	Yes	Yes	Yes	Yes
Firm FEs	No	No	Yes	No
State FEs	Yes	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
State-year trend	Yes	Yes	Yes	Yes
Psuedo R^2 /*Adj.* R^2	0.22	0.22	0.89	0.16
Obs.	27,928	27,928	27,928	27,928
No. of Firms	4,105	4,105	4,105	4,105

This table provides the results of regressions investigating the effect of DRTs on the number of banks from which a firm borrows. The dependent variable is either *Multiple banks*, a dummy variable that identifies firms with multiple banking relationships, or *Number of banks* (defined in Table A2). We include borrower-specific time-varying control variables $\frac{EBIT}{Sales}_{t-1}$, $\frac{Cash}{Assets}_{t-1}$, $Tangibility_{t-1}$ and *Interest Coverage*$_{t-1}$ and a set of 100 dummy variables that identify firm size percentiles based on $Size_{t-1}$. In Columns 1 and 2 we estimate a probit model with state fixed effects, state-year trends, and year fixed effects, in Column 3 we estimate an OLS model with firm and time fixed effects, and state-year trends, and in Column 4 we estimate an ordered logit model with state fixed effects, state-year trends, and year fixed effects. The model is fully saturated with dummies for years relative to DRT implementation year. The coefficients on DRT(t=-2) to DRT(t=+4) and controls are reported. Standard errors, reported in parentheses, are clustered at state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

from multiple banks in the pre-DRT period, as in Diamond (2004). To do this we estimate (1) after replacing each *DRT(s)* (and *Pre-DRT(s)*) dummies with interaction terms *DRT(s)* × *Single* and *DRT(s)* × *Multiple*, where *Single* is a dummy variable that identifies firms with one banking relationship in the year before the establishment of a DRT, and *Multiple* = [1- *Single*]. In this specification, we also include a full set of interaction terms between *Single* and the control variables and our 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 (1) firms for which we have information

Table 4
Cross-sectional tests, debt maturity

A. Multiple banking relationships

	$\frac{LongTermDebt}{Assets}$			$\frac{ShortTermDebt}{Assets}$			*Short*		
	Single (1)	*Multiple* (2)	*Diff* (2)-(1)	*Single* (4)	*Multiple* (5)	*Diff* (5)-(4)	*Single* (7)	*Multiple* (8)	*Diff* (8)-(7)
DRT(t=0)	−0.0002	0.024***	0.024**	0.009	−0.008***	−0.017*	−0.005	−0.041***	−0.036*
	(0.010)	(0.006)	(0.011)	(0.008)	(0.003)	(0.009)	(0.018)	(0.009)	(0.02)
DRT(t=+1)	−0.006	0.030***	0.036***	0.010	−0.009**	−0.019	−0.0002	−0.050***	−0.049
	(0.018)	(0.005)	(0.017)	(0.010)	(0.004)	(0.012)	(0.037)	(0.011)	(0.036)
DRT(t=+2)	−0.004	0.040***	0.044*	0.014	−0.009	−0.023	−0.001	−0.064***	−0.063
	(0.024)	(0.007)	(0.023)	(0.012)	(0.006)	(0.014)	(0.043)	(0.011)	(0.043)
DRT(t=+3)	−0.004	0.049***	0.053*	0.0004	−0.020***	−0.021	−0.022	−0.085***	−0.063
	(0.031)	(0.012)	(0.031)	(0.016)	(0.007)	(0.019)	(0.058)	(0.017)	(0.059)
DRT(t=+4)	−0.003	0.052***	0.055	0.004	−0.021**	−0.025	−0.019	−0.085***	−0.066
	(0.036)	(0.010)	(0.033)	(0.021)	(0.010)	(0.024)	(0.073)	(0.021)	(0.073)
Firm FEs		Yes			Yes			Yes	
Year FEs		Yes			Yes			Yes	
Controls		Yes			Yes			Yes	
State-year trend		Yes			Yes			Yes	
Adj. R^2		0.59			0.53			0.52	
Obs.		17,474			17,474			17,474	
No. of firms		1,520			1,520			1,520	

(*continued*)

on banking relationships and (2) 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.[13]

Our assumption in these tests is not that firms' choice of the number of banking relationships is random. To the contrary, following Diamond (2004), multiple banking relationships in the pre-DRT period may represent attempts by some firms to overcome difficulties in raising external finance. Hence, in equilibrium, this subsample represents constrained firms with a need to raise outside finance. A possible concern with our cross-sectional tests is that other differences across firms with single and multiple banking relationships could bias our estimates. To evaluate the seriousness of this issue, in Table 5 in the IA, we compare firms with single and multiple banking relationships. From our multivariate tests, we find that size is the only significant observable difference across these two groups of firms. We use nonparametric controls for firm size in our regressions to ensure that the difference in firm size does not bias our estimates.

[13] To estimate if confining the sample to firms with non-missing values for the number of banking relationships affects our conclusions, in Table 3 in the IA, we compare firms with missing values for banking relationships to those with nonmissing values. We find that firms with missing information on banking relationships are smaller, less profitable, have more long-term debt, and have higher leverage. We also find that these firms have marginally higher sales growth. In Table 4 in the IA, we re-estimate our baseline tests confining the sample to firms with nonmissing banking information and find that our estimates are similar to those from the full sample. This ensures that limited information availability on banking relationships does not unduly bias our estimates.

Table 4
Continued

B. Abnormally short debt maturity

	$\frac{LongTermDebt}{Assets}$			$\frac{ShortTermDebt}{Assets}$			*Short*			$\frac{TradeCredit}{Assets}$		
	Abnormal (Abn.) (1)	*1-Abn.* (2)	*Diff* (1)-(2)	*Abn.* (4)	*1-Abn.* (5)	*Diff* (4)-(5)	*Abn.* (7)	*1-Abn.* (8)	*Diff* (7)-(8)	*Abn.* (10)	*1-Abn.* (11)	*Diff* (10)-(11)
DRT(t=0)	0.033***	0.002	0.032***	−0.027***	0.006**	−0.032***	−0.088***	−0.005	−0.082***	−0.006	0.0009	−0.007
	(0.006)	(0.005)	(0.008)	(0.004)	(0.003)	(0.006)	(0.012)	(0.010)	(0.015)	(0.004)	(0.003)	(0.005)
DRT(t=+1)	0.062***	−0.008	0.069***	−0.040***	0.009**	−0.047***	−0.142***	0.011	−0.15***	−0.017***	0.0003	−0.018***
	(0.005)	(0.008)	(0.009)	(0.005)	(0.004)	(0.007)	(0.014)	(0.014)	(0.019)	(0.005)	(0.004)	(0.007)
DRT(t=+2)	0.081***	−0.006	0.087***	−0.053***	0.012**	−0.063***	−0.189***	0.015	−0.201***	−0.020***	0.002	−0.024***
	(0.007)	(0.009)	(0.009)	(0.007)	(0.006)	(0.008)	(0.016)	(0.017)	(0.021)	(0.006)	(0.004)	(0.008)
DRT(t=+3)	0.106***	−0.009	0.114***	−0.068***	0.007	−0.072***	−0.245***	0.011	−0.251***	−0.033***	0.003	−0.037***
	(0.010)	(0.012)	(0.013)	(0.011)	(0.008)	(0.013)	(0.024)	(0.022)	(0.029)	(0.010)	(0.005)	(0.011)
DRT(t=+4)	0.128***	−0.016	0.143***	−0.084***	0.012	−0.092***	−0.298***	0.034	−0.327***	−0.035***	−0.0005	−0.036***
	(0.011)	(0.014)	(0.015)	(0.012)	(0.010)	(0.015)	(0.027)	(0.025)	(0.032)	(0.012)	(0.007)	(0.013)
Firm FEs		Yes			Yes			Yes			Yes	
Year FEs		Yes			Yes			Yes			Yes	
Controls		Yes			Yes			Yes			Yes	
State-year trend		Yes			Yes			Yes			Yes	
Adj. R^2		0.60			0.54			0.53			0.64	
Obs.		26,344			26,344			26,344			26,344	
No. of firms		2,617			2,617			2,617			2,617	

(*continued*)

Table 4
Continued

C. Firm size

	$\frac{LongTermDebt}{Assets}$			$\frac{ShortTermDebt}{Assets}$			*Short*			$\frac{TradeCredit}{Assets}$		
	Large firms (1)	*Small firms* (2)	*Diff* (2)-(1)	*Large firms* (4)	*Small firms* (5)	*Diff* (5)-(4)	*Large firms* (7)	*Small firms* (8)	*Diff* (8)-(7)	*Large firms* (10)	*Small firms* (11)	*Diff* (11)-(10)
DRT(t=0)	0.009*	0.014***	0.006	−0.0002	−0.010***	−0.01**	−0.015*	−0.059***	−0.044**	−0.0006	−0.002	−0.001
	(0.005)	(0.004)	(0.007)	(0.002)	(0.003)	(0.005)	(0.008)	(0.007)	(0.011)	(0.003)	(0.002)	(0.004)
DRT(t=+1)	0.009*	0.021***	0.012	−0.003	−0.011*	−0.008	−0.021*	−0.067***	−0.046**	0.001	−0.011**	−0.013*
	(0.005)	(0.007)	(0.009)	(0.003)	(0.006)	(0.007)	(0.011)	(0.016)	(0.019)	(0.004)	(0.005)	(0.007)
DRT(t=+2)	0.018**	0.028***	0.01	−0.001	−0.018**	−0.017*	−0.029**	−0.089***	−0.059***	0.002	−0.011***	−0.013*
	(0.007)	(0.008)	(0.008)	(0.004)	(0.008)	(0.009)	(0.012)	(0.021)	(0.021)	(0.005)	(0.004)	(0.007)
DRT(t=+3)	0.017*	0.038***	0.021*	−0.009	−0.027**	−0.019	−0.043***	−0.115***	−0.072**	0.002	−0.017***	−0.018*
	(0.010)	(0.009)	(0.011)	(0.006)	(0.012)	(0.012)	(0.016)	(0.024)	(0.023)	(0.006)	(0.005)	(0.009)
DRT(t=+4)	0.025**	0.033**	0.008	−0.011	−0.028**	−0.018	−0.048**	−0.112***	−0.065**	−0.002	−0.019**	−0.016
	(0.010)	(0.013)	(0.014)	(0.007)	(0.013)	(0.014)	(0.019)	(0.029)	(0.03)	(0.007)	(0.007)	(0.01)
Firm FEs		Yes			Yes			Yes			Yes	
Year FEs		Yes			Yes			Yes			Yes	
Controls		Yes			Yes			Yes			Yes	
State-year trend		Yes			Yes			Yes			Yes	
Adj. R^2		0.59			0.53			0.53			0.64	
Obs.		26,344			26,344			26,344			26,344	
No. of Firms		2,617			2,617			2,617			2,617	

This table provides results from cross-sectional tests on debt maturity. Panel A reports results of regressions that investigate the differential impact of DRTs on firms with single versus 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. Panel B reports results of regressions that differentiate between firms with abnormally short debt maturity in the pre-DRT period. *Abnormal* (*1-Abnormal*) is a dummy variable that identifies firms with abnormally short maturity structures, that is, with a below-median industry-adjusted level of $\frac{LongTermDebt}{Assets}$ and an above-median industry-adjusted $\frac{ShortTermDebt}{Assets}$ in the year before the establishment of DRT in their state. Panel C reports the results from regressions that investigate the differential impact of DRTs on small and large firms. *Large* (*Small*) is a dummy variable equal to 1 if the firm's tangible assets are above (below) the sample median in the year before establishment of DRT in their state, and otherwise zero. Our sample is confined to firms that were present in the year before DRT implementation in their state. We repeat our tests in subsamples, and in the columns titled *Diff* we test whether the coefficients estimated for two group of firms are significantly different. Standard errors reported in parentheses are cluster adjusted at state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

The evidence in Columns 1 and 2 of Table 4, panel A, show that firms with multiple banking relationships in the pre-DRT period experience an increase in long-term debt, while there is no corresponding increase for firms with a single banking relationship. From Column 3 (titled *Diff*) we find that the coefficients on the two interaction terms are significantly different from one another for several years following DRT establishment. Almost mirroring these results are those in Columns 4 and 5, where we model short-term debt. Only firms that borrow from multiple lenders in the pre-DRT period reduce the proportion of short-term debt to total assets after establishment of DRT. This evidence is consistent with the predictions that follow from Diamond (2004). Our overall maturity measure, *Short*, also follows the same pattern. Moreover, our estimates are economically significant. We find that for firms with multiple banking relationships in the pre-DRT period, the amount of long-term debt increases by 23.6% (0.052 versus subsample mean of 0.22) while the amount of short-term debt decreases by 13.3% (−0.02/0.15).

Lack of significant changes in debt maturity for firms with single banking relationships in the pre-DRT period does not necessarily imply that these firms were unaffected by DRTs. Statistical power could be an issue here. Since we focus on firms that were in existence one year before DRT implementation in their state, we only have 512 firms (and 5,646 firm-year observations) with a single banking relationship in our test. This reduces statistical power. Moreover, our identification comes from the staggered nature of DRT implementation across states. The coefficients on the post-DRT dummies are estimated by comparing firms in early and late DRT states at similar (event) times after DRT implementation. To the extent that the late DRT states learn from the early states, or anticipate DRT implementation in their state, this might bias our estimates downward. Furthermore, our year fixed effects are likely to soak up much of the countrywide improvement in contract enforcement post-1999. To this extent, our estimates should be interpreted as a lower bound on the effect of DRT on firm's debt structure. Note that in this table, we do not examine trade credit, since the theories of trade credit have no prediction on how it should be related to the number of banking relationships.[14]

While the results in Table 4, panel A, are consistent with the prediction in Diamond (2004), they do not necessarily pin down the specific friction that drives Diamond's (2004) result, namely, *lack of coordination* among multiple lenders. To offer some evidence on this precise friction, we find news reports that indicate policy makers' concern with lack of coordination among multiple creditors in India during this time period. For example, the Deputy Governor of the Reserve Bank of India describes (Chakraborty 2012), "...difficulties faced by banks while restructuring their large exposures involving more than one

[14] As already discussed, suppliers have alternative means of contract enforcement, so the Diamond (2004) model does not necessarily apply to trade credit.

lender, under consortium/multiple banking arrangement. While it was easier for banks to negotiate the terms of restructuring of their own exposure with their customers, they found it difficult to co-ordinate their negotiation and monitoring efforts where restructuring involved multiple lenders." The IMF writes in its Country Report on India in 2002 (He 2002), "Different creditors often have different motivations and strategies of dealing with a delinquent borrower, and the lack of coordination among creditors has been cited as an important reason for the failure to reach loan workout agreements." These are indicative of a lack of coordination being an important issue among lenders in India during the time period that we study.

Next, in Table 4, panel B, we test Prediction 3 by examining if the change in debt maturity depends on firm's debt structure before DRT implementation. We expect firms that were the most severely constrained from using long-term debt pre-DRT– those with abnormally short debt maturity structures before the reform – to benefit the most following a reduction in enforcement costs. We define a firm's debt maturity structure as abnormally short term if it has below-median industry-adjusted long-term debt ($\frac{LongTermDebt}{Assets}$), and simultaneously, above-median industry-adjusted short-term debt ($\frac{ShortTermDebt}{Assets}$) in the year before DRT implementation. Note that we use simultaneous information on the firm's usage of long- and short-term debt to avoid leverage effects biasing our analysis. For example, if we define *Abnormal* as firms with below median long-term debt ($\frac{LongTermDebt}{Assets}$) without regard to their usage of short debt, we may pick up firms that have low leverage (i.e., low $\frac{TotalDebt}{Assets}$). Similar problems apply to defining *Abnormal* based solely on $\frac{ShortTermDebt}{Assets}$. Nevertheless, in Table 6 in the IA, we do our tests alternately firms with below and above-median $\frac{LongTermDebt}{Assets}$ and $\frac{ShortTermDebt}{Assets}$ and obtain results similar to the ones reported here.

We re-estimate (1) after replacing each *DRT(s)* (and *Pre-DRT(s)*) dummies with interaction terms *DRT(s)* × *Abnormal* and *DRT(s)* × [1- *Abnormal*]. *Abnormal* is a dummy variable that identifies firms with abnormally short debt maturity structures, as defined above. Columns 1-3 look at $\frac{LongTermDebt}{Assets}$; Columns 4-6 examine $\frac{ShortTermDebt}{Assets}$; Columns 7-9 examine *Short*; and Columns 10-12 present our evidence on $\frac{TradeCredit}{Assets}$. Note that the correlation between *Abnormal* and *Multiple* in our sample is very low (0.07). To this extent, our cross-sectional tests provide independent evidence about the change in debt maturity post-DRT.[15]

[15] The low correlation could arise due to two reasons. First, both *Multiple* and *Abnormal* only provide noisy proxies for identifying firms that are constrained in their ability to borrow long-term debt due to weak enforcement. There are reasons other than enforcement cost for why firms have multiple banking relationships. Second, the noise in our proxies also arises from the fact that short-term debt from multiple lenders is only one of the options that firms can use to overcome the inability to access external finance due to weak enforcement. Using more trade credit, internal cash, constraining growth by not exploiting all available investment opportunities are other options. The specific response a firm adopts depends on the costs and benefits of each alternative. However, as long as the noise in these proxies is not systematically related to DRTs, our tests remain valid. Further, because of the noisy nature of the proxies, we believe that each of our two tests has incremental value.

As predicted, we find that after implementation of DRTs, firms with abnormally short pre-reform debt maturity structures increase the amount of long-term debt and simultaneously reduce the amount of short-term debt and trade credit. Here, again, our estimates are economically significant. For example, after the establishment of a DRT, the amount of trade credit usage reduces by 21.9% (change of -0.035 compared with the subsample mean of 0.16), for firms with abnormally short debt maturity structures.

Next, we differentiate between small and large firms to explore if there is any difference in their response to DRT implementation. As mentioned in Section 2, one can make an argument for the results to go either way. To the extent that agency costs are greater for large firms, such firms may need the discipline provided by short-term debt from multiple lenders. On the other hand, to the extent that implicit enforcement mechanisms, such as reputation concerns, are weaker for small firms, these firms may face greater rationing in an environment of weak enforcement.

In Table 4, panel C, we differentiate between small and large firms by including interaction terms *DRT(s)* × *Small firms* and *DRT(s)* × *[1-Small firms*] in (1), where *Small firms* is a dummy variable that identifies firms with below-median tangible assets in the year prior to the establishment of a DRT in their state. We find that the correlations between *Multiple* and *Small firm* and that between *Abnormal* and *Small firm* are only −0.35 and −0.14, respectively. Thus, the tests in panels A and B do not necessarily inform us about the differential response of small firms to DRTs. Our results in Columns 1-3 indicate that while both large and small firms experience an increase in long-term debt, small firms experience a larger increase. From Columns 4-6, we find that only small firms experience a significant decrease in short-term debt. The combined increase in long-term debt and the decrease in short-term debt result in a significant decrease in *Short* for small firms compared with large firms (Column 9). Our estimates are economically large. We find that *Short* decreases by −0.112, which is 21.9% of the sample mean (0.51), for small firms. Consistent with the view that enforcement problems may particularly constrain *small firms'* access to bank credit, we find that the decline in trade credit comes exclusively from small firms. In terms of economic magnitude, we find that the average small firm reduces trade credit usage by 9.4% (= −0.017/0.18) in the third year following DRT introduction, compared with its sample mean. This is a sizable decrease and should also translate into substantial savings in interest costs, given the implicit annualized interest rate on trade credit can be as high as 23% for small firms (De and Singh 2013).

Using data similar to ours, LMV show that following DRT implementation the size of new long-term borrowings increase for large firms and decrease for small firms. The results in Table 4, panel C, appear to contradict the results in LMV. There are a number of differences between our tests and those in LMV, including the sample, the dependent variable, and the empirical specification, which can potentially explain the differences in results. However, we find that

Table 5
Cross-sectional tests, number of lenders

	Multiple banks(Probit)					
	Abnormal	*1-Abnormal*	*Diff*	*Large firms*	*Small firms*	*Diff*
	(1)	(2)	(1)-(2)	(4)	(5)	(5)-(4)
DRT(t=0)	0.010	0.020	−0.009	0.013	−0.095**	−0.108
	(0.048)	(0.032)	(0.067)	(0.045)	(0.042)	(0.08)
DRT(t=+1)	−0.146***	−0.072*	−0.073	−0.029	−0.204***	−0.176**
	(0.041)	(0.039)	(0.057)	(0.043)	(0.050)	(0.07)
DRT(t=+2)	−0.179**	−0.023	−0.157*	−0.004	−0.173***	−0.169*
	(0.075)	(0.045)	(0.096)	(0.072)	(0.064)	(0.102)
DRT(t=+3)	−0.330***	−0.091	−0.239***	−0.069	−0.313***	−0.244**
	(0.058)	(0.082)	(0.074)	(0.096)	(0.087)	(0.114)
DRT(t=+4)	−0.464***	−0.104	−0.36**	−0.077	−0.370***	−0.292***
	(0.109)	(0.093)	(0.149)	(0.113)	(0.099)	(0.115)
Firm FEs		Yes			Yes	
Year FEs		Yes			Yes	
Controls		Yes			Yes	
State-year trend		Yes			Yes	
Psuedo R^2		0.25			0.26	
Obs.		20,028			20,028	
No. of firms		1,520			1,520	

This table provides results from cross-sectional tests on number of lenders. In Columns 1-3 we differentiate between firms with abnormally short debt maturity and others. *Abnormal* (*1-Abnormal*) is a dummy variable that identifies firms with a below-median industry-adjusted level of long-term debt and an above-median industry-adjusted short-term debt in the year before the establishment of DRT in their state. In Columns 4-6 we differentiate between small and large firms. *Large*(*Small*) is a dummy variable equal to 1 if the firm's tangible assets are above (below) the sample median in the year before the establishment of DRT in their state, and otherwise zero. Our sample is confined to firms that were present in the year before DRT implementation in their state. We estimate separate regressions in the subsample, and in the column titled *Diff* we test whether the coefficients estimated for two group of firms are significantly different. Standard errors, reported in parentheses, are cluster adjusted at state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

the main reason is the difference in the outcome variables employed. For the most part, LMV model the gross amount of new long-term loans, and they construct this variable from the cash-flow statement. We, on the other hand, focus on the amount of long-term debt on the balance sheet. Apart from missing values in the cash-flow statement, the main difference in the dependent variables is that our measure nets out debt repayments – which is important since we are interested in analyzing debt maturity – while the variable used by LMV does not. Our analysis indicates that while large firms do increase the size of new loans in the post-DRT period, they also experience large loan repayments, with the result that the amount of long-term debt on the balance sheet does not increase more for large firms compared with small firms.

4.4.2 Number of lenders. Next, we examine cross-sectional patterns in the change in number of lenders post-DRT. In Columns 1-3 of Table 5, we estimate a probit model similar to that in Table 3, differentiating between firms with abnormally short debt maturity structures and the rest of the firms. As before, we do this by replacing the *DRT(s)* dummies with interaction terms *DRT(s)* × *Abnormal*, and *DRT(s)* × *[1-Abnormal]*, where *Abnormal* is defined as

before. The results from Column 3 show that firms with abnormally short debt maturity structures are more likely to move to a single banking relationship after implementation of DRTs, while there is no corresponding change in the propensity of other firms to borrow from a single bank. This evidence is consistent with the view that firms that were particularly constrained in their access to long-term debt pre-DRT consolidated their banking relationships after its implementation. In terms of economic significance, the coefficient on DRT(t=+4) indicates that, on average, the odds that a firm with an abnormally short debt maturity structure borrows from multiple banks versus a single bank is 0.36 lower four years after DRT, compared with what it was the year before DRT was implemented. This is a 21.1% reduction in the odds ratio compared with the sample mean[16].

In Columns 4-6 we differentiate between small and large firms, and, again, find that small firms are more likely to move to a single banking relationship after implementation of DRTs, compared with large firms.

4.5 Equity market response to DRT news and further evidence

If the debt maturity changes we document reflect relaxation of borrowing constraints in the post-DRT period, then DRTs should be greeted as good news, especially by shareholders of firms expected to benefit the most from the reform (maximally treated firms). In Table 6, we test this prediction by studying the stock price reaction of firms around the time of announcements about DRT establishment. Specifically, we examine if stock prices of firms with multiple creditors and those with abnormally short debt maturity structures in the pre-DRT period react positively (negatively) on days on which there was positive (negative) news about the likelihood of DRT implementation.[17] Our search of news articles helps us identify two days of positive news about DRTs: May 13, 1993, when the DRT Bill was introduced in Indian Parliament, and March 18, 1996, when the Supreme Court ruled in favor of DRTs allowing the process to continue. There was one day with surprising negative news about DRT implementation – July 25, 1994, when Delhi High Court announced its prima facie view that the DRT law was not valid.[18]

To measure abnormal returns, we sort stocks every day (independently) into five groups based on beta, size, and book-to-market (B/M), and form a benchmark portfolio of all stocks in the same beta, size, and B/M quintile. We define abnormal return as the return on the stock *minus* return on its benchmark portfolio. We then relate the daily abnormal returns on news days

[16] The odds that a firm with an abnormally low level of long-term debt borrows from a single bank is 1.7.

[17] We do not study the effect of firm size on the stock price reaction to DRTs because size may affect returns for reasons not related to DRTs, making it difficult to cleanly disentangle the DRT effect.

[18] The passage of the DRT bill in Parliament was a foregone conclusion once it was introduced (it met with no opposition), and hence we do not include that date in our analysis.

Table 6
News about DRTs and stock returns

	Day (-1,0)		*Day 0*		*Day (0,1)*	
	Positive news	*Negative news*	*Positive news*	*Negative news*	*Positive news*	*Negative news*
	(1)	(2)	(3)	(4)	(5)	(6)
Multiple (lagged, one year)	0.003***	−0.004	0.009***	−0.016*	0.0043***	−0.011
	(0.00005)	(0.010)	(0.0003)	(0.008)	(0.0009)	(0.012)
Abnormal (lagged, one year)	0.002***	0.0001	0.00002	−0.008	0.005***	−0.010
	(0.00009)	(0.008)	(0.0008)	(0.006)	(0.002)	(0.009)
R^2	0.003	0.0008	0.006	0.028	0.006	0.011
Obs.	755	184	755	184	754	184

This table provides results from regressions that investigate the effect of news related to DRTs on firm's stock returns. The dependent variable is *Abn_ret*, the difference between the return on the stock and the return on the benchmark portfolio. To construct the benchmark portfolio, every day we sort stocks (independently) into five groups based on beta, size, and book-to-market, and the benchmark portfolio consists of all stocks in the same beta, size and B/M quintile as the firm's stock. We consider following days as positive news days: (1) May 13, 1993: the day on which the DRT bill was introduced in Indian Parliament and (2) March 18, 1996: the day on which the Supreme Court ruled in favor of DRTs. We consider July 25, 1994, a negative news . This was the day on which the Delhi High Court declared its prima facie view that DRTs were not valid. In Columns 1, 3 and 5 we examine the returns on positive news days while in Columns 2, 4 and 6 we investigate returns for the negative news day. The standard errors are clustered by time (day). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

to firm characteristics associated with maximal treatment under DRTs using the following model:

$$\text{Abn\_ret}_{it} = \beta_0 + \Gamma_1 Multiple + \Gamma_2 Abn + \delta_t + \varepsilon_{it}, \quad (2)$$

where *Multiple* and *Abnormal* (lagged by a year) are as defined before, and δ_t are day fixed effects.[19] In this regression we cluster the standard errors by date.

If DRT implementation increases the market value of equity of firms with multiple creditors and those with abnormally short maturity structures, then we expect both Γ_1 and Γ_2 to be positive (negative) on the days with positive (negative) news about DRT implementation. The results in Table 6 are broadly consistent with our expectation. Focusing on the coefficient on *Multiple*, we find that the stock price of firms with multiple creditors on average increased by 43 basis points in a (0,1) day window around positive DRT news days, while it declined by 1.1% around the day with negative news. We find a similar pattern for firms with abnormally short debt maturity structures. The increase in stock prices of firms with abnormally short debt maturity structure indicates that the fall in interest rates on long-term debt documented by Visaria (2009) may not have been the only effect of DRTs. If that were the case, one would expect a larger stock price reaction for firms with more long-term debt. Our result, on the other hand, is more consistent with the predictions from Diamond (2004).

[19] While the average daily excess return for the entire sample – from which the benchmark portfolios are constructed – is by definition zero, our tests are conducted among a subsample of firms for which we have information on number of banking relationships in the previous year. Hence, we include day fixed effects to demean the daily abnormal returns within our subsample.

We also find that the results are robust to alternate event windows. Overall, our results are consistent with DRT implementation easing borrowing constraints for firms maximally exposed to DRTs.

Finally, Diamond's (2004) mechanism relies on high short-term debt and multiple lenders serving to commit lenders from not renegotiating debt in the face of financial distress. This, in turn, would imply that when such firms are in financial distress, private renegotiation is less likely to occur. Testing this directly is difficult because of a lack of data on private debt renegotiations. As an alternative, we hand-collect data from India's bankruptcy board – the Board for Industrial and Financial Reconstruction (BIFR) – and relate a firm's likelihood of registering with BIFR to its debt structure. We obtain the names of firms that file with the BIFR from their Web site and manually match it to our data set. The BIFR is a government agency, and in practice filing with BIFR is both a sign that private negotiation with the lenders has failed and that the lenders are moving court to recover their dues. Thus, we test to see if firms that borrow from multiple lenders and those with high short-term debt are more likely to register with BIFR. To conserve space, we present these results in Table 7 of the IA. Our results show that, controlling for leverage, firms that borrow from multiple banks and those that have high levels of short-term debt (as measured by *Short*) are significantly more likely to file for BIFR protection. This is consistent with such firms being less likely to privately renegotiate debt. However, we recognize that this evidence is only suggestive, as a firm's debt structure is endogenous and some unobserved factor may be correlated both with the firm's debt structure and its likelihood of registering with BIFR.

4.6 Robustness

In Table 7 we conduct a series of robustness tests for the results presented in panel A of Table 2. Here, we only present results of the tests with *Short* as the dependent variable. Relevant tests with $\frac{ShortTermDebt}{Assets}$ and $\frac{TradeCredit}{Assets}$ as the dependent variables are presented in Table 8 of the IA.

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 – 76 firm-year observations out of 45,140 – from our sample (Column 1).

Next, we repeat our tests after excluding the current portion of long-term debt from the numerator of *Short* 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 2). In the next robustness check, we include state-level macroeconomic indicators, including lagged values of *Log(State GDP)*, *State GDP growth*, *Share of bank credit*, and *Per capita credit* as additional regressors in (1). We find that our conclusions are robust to their inclusion (Column 3).

Table 7
Robustness

	Short					
	Excl. syndicated loans (1)	Excl. current portion of LTD (2)	Incl. state variables (3)	1993 sample (4)	Balanced sample (5)	Industry-year FEs (6)
DRT(t=−2)	0.0006	−0.0005	0.003	0.003	0.006	0.004
	(0.005)	(0.006)	(0.006)	(0.004)	(0.011)	(0.006)
DRT(t=0)	−0.032***	−0.027***	−0.034***	−0.035***	−0.022**	−0.032***
	(0.006)	(0.006)	(0.006)	(0.008)	(0.009)	(0.005)
DRT(t=+1)	−0.035***	−0.030***	−0.033***	−0.037***	−0.044***	−0.037***
	(0.011)	(0.010)	(0.010)	(0.012)	(0.015)	(0.007)
DRT(t=+2)	−0.050***	−0.045***	−0.058***	−0.054***	−0.044***	−0.053***
	(0.014)	(0.014)	(0.013)	(0.013)	(0.014)	(0.010)
DRT(t=+3)	−0.063***	−0.054***	−0.069***	−0.067***	−0.077***	−0.068***
	(0.019)	(0.019)	(0.018)	(0.019)	(0.019)	(0.013)
DRT(t=+4)	−0.067***	−0.060***	−0.081***	−0.077***	−0.100***	−0.073***
	(0.022)	(0.021)	(0.020)	(0.021)	(0.025)	(0.016)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	No
State-year trend	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.62	0.64	0.63	0.56	0.57	0.63
Obs.	45,064	45,140	44,891	18,292	6,283	45,140
No. of firms	6,116	6,116	6,111	1,802	415	6,111

This table reports results for various robustness checks on regression (1) for dependent variable *Short*. Column 1 reports results after excluding 76 firm-year observations with nonzero syndicated loans. In Column 2, we exclude the current portion of long-term debt from the numerator of *Short*. We include state-level macroeconomic variables, that is, lagged values of *Log (State GDP)*, *State GDP growth*, *Share of bank credit*, and *Per capita credit*, to our base specification and report the results in Column 3. In Column 4, we limit our sample to firms that existed at the beginning of the sample period. In Column 5, we examine a fully balanced panel (no entry or exit). In Column 6, we include within industry year fixed effects (with industry defined at the two-digit NIC code level). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

One may be interested to know the extent to which our results are due to firm entry and exit following DRT establishment as opposed to changes in debt structure among pre-existing firms. To isolate the intensive margin of our results, that is, the change in debt structure only for firms that exist prior to DRT implementation, we repeat our tests in a sample that only includes firms that were active in 1993. Results in this sample are very similar (Column 4). 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 5, we estimate our model on a balanced panel, that is, we do not allow for any entry or exit.[20] Here again we find that despite a significant reduction in sample size, the establishment of a DRT is accompanied by a significant reduction in Short.

[20] 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.

To allay the concern that industries with high values of short-term debt may disproportionately influence our results, we employ nonparametric controls for time-varying, industry-specific shocks. In particular, we repeat our tests after including industry-year fixed effects (with industry defined at the two-digit NIC code level), following Gormley and Matsa (2014), and find our results to be robust (Column 6). Finally, 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 in that period had a geographic stagger that even roughly coincided with that of the DRTs. Also note that the specification in column 6 above is likely to control for coincident economic reforms such as liberalization of foreign investment norms, which happened at an industry-year level.

As mentioned before, in Table 8 in the IA, we repeat the relevant robustness tests (inclusion of state macro variables, restricting the analysis to 1993 sample, and inclusion of industry-year fixed effects) for both short-term debt ($\frac{ShortTermDebt}{Assets}$) and trade credit ($\frac{TradeCredit}{Assets}$), and find consistent results.

4.7 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.

In Table 8 we document how the book value of fixed (long-term) assets changed around DRT establishment. We estimate a specification similar to (1) with $\frac{FixedAssets}{TotalAssets}$ as the dependent variable in Column 1. *Fixed assets* is the book value of (gross) land, buildings, plant and machinery. 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 2, panel A). The coefficients in column 1 indicate that there is an increase in the proportion of fixed assets after DRT establishment. Thus, coincident with a change in the liability mix, firms also appear to shift their asset composition towards fixed assets and away from current assets.[21]

In Columns 2-4, we look at how the changes in asset structure depend on the debt structure of the firms in question before DRT. In particular, we examine whether the firms that had abnormally short pre-DRT debt maturity respond differently. As before, we estimate (1) after replacing each *DRT(s)* (and *Pre-DRT(s)*) dummy with the interaction terms *DRT(s)* × *Abnormal* and *DRT(s)* × [1- *Abnormal*]. Again, our results indicate that the change in asset composition is stronger for firms with abnormally short debt maturity pre-DRT. In terms of economic magnitudes, $\frac{FixedAssets}{Assets}$ increases by 8.1% (0.035/0.43),

[21] LMV also document an increase in *Plant and Machinery* post-DRT, but they do not examine how fixed assets as a proportion of total assets changes. If firms simultaneously increase both fixed assets and current assets, then this would leave asset maturity unchanged even in the face of an increase in fixed assets.

Table 8
Asset Structure

	$\frac{FixedAssets}{Assets}$						
	All Firms	*Abnormal*	*1-Abnormal*	*Diff* (2)-(3)	*Large Firms*	*Small Firms*	*Diff* (6)-(5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
DRT(t=−2)	−0.002	0.002	−0.004	0.006	−0.008	0.002	00.01
	(0.003)	(0.004)	(0.005)	(0.008)	(0.007)	(0.008)	(0.007)
DRT(t=0)	0.002	0.008	0.0008	0.007	−0.004	0.018***	0.022*
	(0.002)	(0.005)	(0.003)	(0.004)	(0.005)	(0.007)	(0.011)
DRT(t=+1)	0.003	0.014**	−0.005	0.019**	−0.011	0.022***	0.033**
	(0.004)	(0.006)	(0.004)	(0.007)	(0.008)	(0.007)	(0.013)
DRT(t=+2)	0.008	0.020***	0.0006	0.019**	−0.0008	0.026***	0.026**
	(0.005)	(0.007)	(0.004)	(0.008)	(0.008)	(0.008)	(0.013)
DRT(t=+3)	0.011	0.033***	−0.004	0.038***	−0.007	0.035***	0.042**
	(0.008)	(0.010)	(0.007)	(0.012)	(0.010)	(0.010)	(0.016)
DRT(t=+4)	0.015*	0.035***	−0.003	0.038***	−0.003	0.036***	0.039**
	(0.008)	(0.010)	(0.007)	(0.012)	(0.012)	(0.009)	(0.018)
Firm FEs	Yes		Yes			Yes	
Year FEs	Yes		Yes			Yes	
Controls	Yes		Yes			Yes	
State-year trend	Yes		Yes			Yes	
Adj. R^2	0.79		0.82			0.82	
Obs.	42,746		25,398			25,398	
No. of Firms	5,704		2,505			2,505	

This table provides results from regressions investigating the effect of DRTs on firm's asset structure. We estimate a model similar to (1) with $\frac{FixedAssets}{Assets}$ as the dependent variable. Fixed assets include Gross Land, buildings, plant and machinery. The control variables included are: $\frac{EBIT}{Sales}_{t-1}$, $Interest\ Coverage_{t-1}$ and a set of 100 dummy variables that identify firm size percentiles based on $Size_{t-1}$. In Columns 2-4 we differentiate between firms with abnormally short debt maturity and others. *Abnormal* (*1-Abnormal*) is a dummy variable that identifies firms with a below-median industry-adjusted level of long-term debt and an above-median industry-adjusted short-term debt in the year before the establishment of DRT in their state. In Columns 5 and 6, we differentiate between small and large firms. *Large* (*Small*) is a dummy variable equal to 1 if the firm's tangible assets are above (below) the sample median in the year before the establishment of DRT in their state, and otherwise zero. Standard errors, reported in parentheses, are clustered at state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

four years after DRT implementation for firms with abnormally short debt maturity, while there is no significant change for the other group. In Columns 5-7 of Table 8 we differentiate between large and small firms, and, consistent with our prior results, find that the increase in the proportion of fixed assets is concentrated among small firms.

In Table 9 of the IA, we further show that all of the changes in $\frac{FixedAssets}{Assets}$ comes from changes in *Plant and Machinery*, and not through changes in *Land and Buildings*. In Table 10 of the IA, we show that these asset structure changes are robust along various dimensions. They are robust to the inclusion of various state macroeconomic variables, industry-year fixed effects, an alternative definition of fixed assets (which includes transport equipment in this category), using ($\frac{Debt}{Total\ Assets}$) instead of *Interest coverage* to control for leverage, and including lagged sales growth as a control for investment opportunities.

Overall, our evidence shows that a legal reform that allows firms to tilt their financing toward more long-term debt also affects the composition of their assets. Firms that increase their long-term debt following the reform also increase their relative investment in long-term assets.

5. Conclusion

How can we sustain external finance in an environment in which 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. 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 setting allows us to examine differential time trends (which we do not find). Our detailed firm-level data also helps us understand how short-term debt and the number of lenders go hand-in-hand to help overcome weak enforcement.

Consistent with Diamond (2004), we find that firms reduce the proportion of short-term debt after getting access to a DRT. This reduction occurs especially among firms that borrow from multiple lenders in the pre-DRT period, firms with abnormally short debt maturity structures in the pre-DRT period, and smaller 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 indicative of improvements in contract enforcement enabling firms to move 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 toward long-term assets.

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.

Table A1
DRT establishment dates

City of DRT	Date	Jurisdiction	Financial year	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	1,264
		Total pre-DRT firms		3,690

The table 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.

Table A2
Description of variables

Variable name	Description
Total debt	Total borrowing (*Prowess* variable) *excluding* borrowing from the 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	Ratio of short-term debt to total debt
DRT	Dummy variable that takes a value one for firms in the jurisdiction of a functioning DRT and zero otherwise
Size	Natural logarithm of the book value of total assets (in units of Rs. 10 million)
$\frac{EBIT}{Sales}$	Ratio of earnings before interest and taxes to sales
$\frac{Cash}{Assets}$	Ratio of cash and marketable securities to the book value of total assets
Tangibility	Ratio of the book value of land, buildings, plant, and machinery to total assets
Interest coverage	Ratio of earnings before interest and taxes over interest paid
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 the establishment of a DRT in their state
Abnormal	Dummy variable that identifies firms with abnormally short maturity structures. We define a firm's maturity structure as abnormally short-term if it has below-median industry-adjusted level of long-term debt and above-median industry-adjusted short-term debt in the year before the establishment of DRT in their state
Large	Dummy variable that identifies large firms in the year before the establishment of a DRT in their state, based on median tangible assets

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