Renegotiation and the Contractibility of Accounting Information: Evidence from a Bankruptcy Reform[*]

Pietro Bonetti[‡]
University of Padova
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Abstract

I study whether the ex post ability to renegotiate debt contracts increases ex ante incentives to make financial statements more contractible. For identification, I exploit the introduction of a recent bankruptcy law in Italy which substantially facilitated the renegotiation of outstanding debt, and court congestion as a source of cross-sectional variation in the enforceability of debt contracts. I address endogeneity concerns by relying on discontinuous changes in court congestion across similar contiguous municipalities located across judicial district borders, along with instrumental variable estimation. I find that after the introduction of the law, firms in more efficient courts increase the contractibility of accounting information as compared to firms in less efficient courts. Consistent with theory, cross-sectional analyses suggest that the effect is larger for firms with less collateral to pledge, with under-investment problems, and those borrowing from banks that are more prone to liquidity shortages. In addition, I find that the change in the properties of accounting information is accompanied by an increase in arm's length contracting. Taken together, I provide evidence on how legal institutions that govern debt renegotiation when a debtor is in financial distress affect the contracting environment and the properties of accounting information.

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[‡] University of Padova, via del Santo 33, Padova Italy 35123. Email: pietro.bonetti@unipd.it

1. Introduction

A large literature discusses how legal institutions protecting creditor rights affect debt contracting and the use of accounting information in debt contracts.[1] Less well understood, however, are the specific avenues through which legal institutions that regulate financial contracts shape reporting and disclosure choices (Leuz and Wysocki, 2015). Understanding the nature of these channels is of primary importance, as it allows the identification of the economic forces underlying firms' reporting choices. In this paper, I examine whether a bankruptcy reform that facilitates the renegotiation of debt contracts affects incentives to make financial statements more contractible.[2]

Bankruptcy laws define the rules that govern debt renegotiation when a firm is in financial distress, thus regulating the conflicts of interest between creditors and debtors over continuation / liquidation decisions. However, the way these ex post conflicts are resolved influences the ex ante behavior of the parties even when a firm is far from distress (Hart, 1995). For example, the literature on US Chapter 11 suggests that the absence of a structured renegotiation procedure provides strong repayment incentives, but might involve ex post inefficiencies in terms of excessive liquidations of viable firms (Shleifer and Vishny, 1992). In turn, this threat of premature liquidation increases the ex ante cost of disclosing bad news in a timely manner that would accelerate the transfer of control rights (Povel, 1999), or leads firms to lower renegotiation costs by relying on concentrated and relationship-based debt structures (Gertner and Scharfstein, 1991).

To study how renegotiation considerations affect the contractibility of accounting information, I exploit the introduction of the Italian Bankruptcy Law (henceforth, the "Law") in 2005 which, in the spirit of US Chapter 11, has substantially facilitated private (i.e., out-of-court)

[1] La Porta et al. (1998), Ball et al. (2000), Leuz et al. (2003), Ball et al. (2008b).

[2] I define the contractibility of accounting information in broad terms as the extent to which financial statements capture deterioration in credit quality on a timely basis and the extent to which external parties (courts) can enforce the information conveyed by financial statements (Watts, 2003; Ball and Shivakumar, 2005; Ball et al., 2008a; Christensen and Nikolaev, 2012).

and in-court debt restructuring agreements. Both procedures were strongly limited in Italy before the Law was introduced. For example, in-court reorganizations were subject to strict reimbursement requirements, while private restructuring agreements faced the risk of nullification in the case of subsequent bankruptcy by means of extensive claw-back provisions. The Law abolished the reimbursement requirements and strongly limited the impact of the claw-backs. These legal changes represented a significant shift from a liquidation-based regime, where firms in financial distress were more likely to be liquidated piecemeal, to a reorganization-based regime, where distressed firms were more likely to be kept on as a going concern (Djankov et al., 2008).

The increase in the ex post ability to renegotiate debt contracts following the introduction of the Law will create incentives to make financial statements more contractible through three non-mutually exclusive channels. First, by facilitating the renegotiation of debt contracts in cases of financial distress and, thus, allowing the ex post efficient continuation of the firm, the Law will decrease the likelihood that a borrower will be exposed to the risk of premature liquidation due to coordination failures upon the disclosure of bad news (Hertzberg et al., 2011) or following the transfer of control rights (Hart, 1995; Hart and Moore, 1995; Chava and Roberts, 2008). Other things being equal, this will decrease the cost of timely reporting bad news in financial statements (Heinkel and Zechner, 1993; Povel, 1999), which, in turn, makes financial statements more contractible (Watts, 2003; Ball and Shivakumar, 2005; Nikolaev, 2010; Christensen and Nikolaev, 2012). At the margin, the decrease in the cost of making financial statements more contractible will make it more viable to shift the lending approach from a concentrated relationship based on soft information to a more decentralized transactional based on financial statements (Ball et al., 2008a). Second, an increase in the ability to renegotiate debt contracts when performance deteriorates increases the effectiveness of accounting-based covenants as trip wires (Smith and

Warner, 1979; Berlin and Mester, 1992; Dichev and Skinner, 2002), lowering the need to collect costly soft information in monitoring the borrower (Diamond, 1984; Rajan, 1992). Given that covenants are written based on accounting variables through which the parties agree to shift control rights (Aghion and Bolton, 1992; Hart and Moore, 1995; Christensen and Nikolaev, 2012), this change in the contracting environment will create incentives to make financial statements more contractible. Third, an increase in the ability to renegotiate debt contracts lowers the cost of expanding the number of lenders (Dewatripont and Maskin, 1995; Bolton and Sharfestein, 1996). Given that multiple lenders have fewer incentives to gather soft information (Berger et al., 2005), the demand for hard and contractible information will increase.

The identification of the effect of the Law on the contractibility of accounting information is challenging, given that the Law applies to all firms in my sample. In order to generate variation in the treatment effect, I draw on theory that suggests that the impact of a reform facilitating the renegotiation of debt contracts depends on the quality of court enforcement (Dakolias, 1999; Jappelli et al., 2005; Djankov et al., 2008; Gennaioli, 2013). Firms incorporated in inefficient courts (i.e., busy, with a large number of pending cases and lengthy procedures) are less likely to be affected by the Law. Indeed, lenders will compare the liquidation value with the continuation value when they vote on the reorganization plan or when they make the continuation / liquidation decision in private renegotiations. Efficient courts make firms' opportunistic behavior during the reorganization more costly, increasing the size of resources that can be credibly pledged to convince lenders to renegotiate. Thus, efficient courts increase the recovery rate in case of continuation for any given liquidation value, which implies that the viability of renegotiations increases in the efficiency of the court. As a result, the effect of the Law on the contractibility of financial statements is likely to be stronger when courts are more efficient and less crowded.

Using a sample of private firms from 2002 to 2008, I exploit the adoption of the Law in 2005 as a source of plausibly exogenous[3] variation in the ability to renegotiate debt contracts, and cross-sectional variation in court congestion (i.e., pending cases per judge at the beginning of 2005) as a proxy for court efficiency. The empirical strategy is akin to a difference-in-differences design where the identification assumption is that the paths in reporting practices for firms incorporated in less and more efficient courts would not be systematically different in the absence of the Law. To address the concern that firms incorporated in different courts would have experienced different trends in the absence of the Law, I control for heterogeneity across judicial districts[4] by restricting the sample to neighboring municipalities located across court judicial borders, and employing firm and border-year fixed effects, so that the identifying variation is restricted to discontinuous changes in court efficiency between two adjacent judicial districts. This approach is in the spirit of a spatial discontinuity design where the identification assumption is that crossing a judicial border affects only court efficiency while all other factors that can affect the outcome variable vary smoothly at the border.[5]

Court efficiency can be bundled with other economic or institutional factors that vary discontinuously at judicial borders, however, and thus explain why firms may react differently to the Law. I address this concern in two ways. First, I control for heterogeneity across districts by including pre-Law court-level variables that correlate with financing needs and the characteristics of the banking industry. Second, I propose as an alternative empirical strategy an instrumental

[3]The timing of the Law is likely to be exogenous to firms in my sample (i.e., private firms). The bankruptcy code regulating public firms was first modified after the Parmalat Scandal in 2003, when the Government bailed out Parmalat through an executive order (DL. 47/2003). After the Government intervention, the Parliament started to work on a reform of the entire bankruptcy code.

[4] Hereafter, I use the terms court and judicial district interchangeably.

[5] While firms cannot engage in forum shopping, they can manipulate the forcing variable if in expectation of gaining benefits from the Law they systematically move towards more efficient courts. While this is possible in the long-run, in the short-run the moving costs are likely to be too large to systematically affect my estimates. However, I restrict the sample to firms that do not change the judicial district of incorporation over the sample period in additional specifications.

variable approach based on the increase in court congestion in judicial districts along the Italian drug trafficking routes driven by the plausibly exogenous rise in the opium seizure rate after the War in Afghanistan. The cultivation of opium was banned in Afghanistan by the Taliban in 2000 but restored at the beginning of the war in 2001, and the opium import rate in Italy experienced a substantial jump immediately afterwards. Anecdotal evidence suggests that courts along the drug transportation routes faced a substantial increase in workload due to the jump in the opium seizure rate (Relazione sulla Sicurezza - DCSA, 2004). The underlying idea of the instrument is that the increase in court congestion driven by the jump in the opium seizure rate is unrelated to the existing local judicial efficiency at the time of the Law, and that it affects the contracting environment only through the effect on court congestion by the way of increasing the number of legal cases.

I measure the contractibility of accounting information using timely loss recognition (henceforth, *"TLR*") for two reasons. First, *TLR* increases the contractibility of financial statements as it accelerates the transfer of control rights when a firm approaches financial distress. Second, *TLR* increases the frequency of the disclosure of bad news, rising the risk of early liquidation if there are impediments to efficient renegotiation. Thus, the Law is expected to have first-order effects on the cost of *TLR*. I further consider changes in accounting practices that can potentially affect *TLR*, such as write-offs, bad debt expenses, and other extraordinary expenses.

I find that after the Law was introduced, firms incorporated in more efficient courts experience a larger increase in *TLR* relative to firms incorporated in less efficient courts. The magnitude of the effect is stable across model specifications and robust to the instrumental variable estimation. I further show that the change in reporting practices occurs around the time of the treatment, while I do not find evidence of differential trends in the years leading up the Law. The results hold when I use alternative proxies for the contractibility of accounting information.

Next, I explore heterogeneity in the average effect. I find that the increase in *TLR* is larger for firms with less collateral to pledge, which are more likely to address contracting costs through the state-contingent allocation of control rights (Berger and Udell, 1990; Skinner, 1993; Christensen and Nikolaev, 2012). Consistent with theory that suggests that renegotiation costs shape the trade-off between coordination and hold up concerns (Dewatripont and Maskin, 1995; Bolton and Scharfstein, 1996), I find that firms in the middle of the pre-reform distribution of number of banks experience the larger increase in *TLR*. The effect is also larger for financially constrained firms, and for firms borrowing from banks that are more prone to liquidity shortages (Detragiache et al., 2000). Lastly, I show that neither differences in the structure of the banking system nor changes in the expected recovery rate are likely to be major drivers of my results.

To explore the mechanism behind the results, I study the change in lending, and how it maps onto the change in reporting practices. If the Law has increased incentives to make financial statements more contractible, then firms will find it more cost beneficial to increase the pool of lenders or switch to transactional lenders (Petersen and Rajan, 2002; Berger et al., 2005), which are more likely to rely on financial statements and state-dependent contracts in monitoring. Consistent with this reasoning, I find that the increase in *TLR* occurs among firms that maintain or increase the level of debt. Most importantly, I find that the supply of loans provided by large banks relative to local banks increases to a greater extent in more efficient courts than in less efficient courts. This effect is unlikely to be driven by shocks in the structure of the banking industry, as the change in the ratio of local (big) bank branches to the total number of branches between more and less efficient courts is statistically insignificant and economically negligible. Lastly, I show that a potential change in the riskiness of corporate loans, as reflected in borrowers' default rates and banks' loan loss provisions, is unlikely to be the major driver of my results.

This paper contributes to the literature in several ways. First, I contribute to the literature on the economic consequences of the laws that protect creditor rights, and, in particular, on the interaction between bankruptcy laws and contract design (Acharya and Subramanian, 2009; Vig, 2013; Rodano et al., 2014; Ponticelli, 2014). While the accounting literature has provided a broad cross-country picture of the relationship between creditor rights and financial reporting (Ball et al., 2000; Bushman and Piotroski, 2006; Ball et al., 2008b), my focus on a specific institutional mechanism (i.e., the ability to renegotiate debt contracts) allows to identify a precise contractual avenue by which legal institutions affect accounting choices. Second, I add to the literature on the link between accounting information and debt contracting, and, in particular, on how renegotiation considerations affect the properties of accounting information (Armstrong et al., 2010; Christensen et al., 2015). I exploit quasi-experimental variation in renegotiation costs, and show that an increase in the ability to renegotiate debt contracts fosters borrowers' incentives to reveal bad news in financial reports on a timely basis. Third, I contribute to the nascent literature on the role of courts in contracting (Lerner and Schoar, 2005; Gennaioli, 2013). This literature argues that the ability to write complex and state-contingent contracts depends on the extent to which courts can verify the accounting variable over which the parties agree to shift control rights. Courts' bias and inefficiencies generate an enforcement risk that leads parties to rely on simple contracts and relationship-based monitoring with little role for accounting information. While this literature suggests the existence of a compelling link between enforcement risk and reporting practices, I provide the first evidence of its empirical relevance. Even if a large literature has explored the effect of enforcement on accounting choices (Ball et al., 2000; Leuz et al., 2003; Jayaraman, 2012; Aghamolla and Li, 2015), I focus on a within-country variation and show that the effect of a reform that facilitates the renegotiation of debt contracts depends on the quality of court enforcement.

2. Institutional background

Although Italy is generally considered as a country that provides strong rights to creditors (La Porta et al., 1998), historically it had one of the lowest recovery rate within the OECD countries (World Bank, 2005). The literature argued that one of the reasons for the low recovery rates was the legal constraints embedded in the Bankruptcy Code, which failed to keep financially but not economically distressed firms as a going concern (Djankov et al., 2008).[6]

Several institutional features strongly limited the ability of the contracting parties to achieve in-court and out-of-court debt restructuring agreements until the bankruptcy reform was adopted. First, the viability of in-court reorganizations (i.e., *Concordato Preventivo*) was subject to the full payment of secured creditors and the payment of at least the 40 percent of the loans held by unsecured creditors. Second, the Law did not regulate the seniority status of the loans provided during the reorganization phase. There was legal uncertainty about the extent to which the courts could enforce the super-seniority status granted to loans obtained during the procedure (i.e., debtor in possession financing), which in turn lowered lenders' incentives to provide funds during the reorganization.[7] Third, private debt restructuring agreements (i.e., *Accordo Stragiudiziale*) faced the risk of nullification by the court in the case of subsequent bankruptcy due to extensive claw-back provisions. All these factors contributed to make in-court and out-of-court agreements unlikely before the Law was adopted. As a consequence, the majority of financially distressed firms ended up in the bankruptcy procedure under the direction of a court-nominated trustee (Fabiani, 2005). A combination of poor trustee incentives (i.e., her compensation was unrelated to

[6] If the value of the assets is higher when a firm is preserved as a going concern rather than sold piecemeal (as it is likely to be at the beginning of the financial distress), then legal rules and other institutional features that restrain the ability to restructure outstanding debt when a firm is close to distress would lead to low average recovery rates (Djankov et al., 2008).

[7] Even if the debtor in possession financing constitutes a violation of the absolute priority rule which leads to some ex ante inefficiencies (Hart, 1995; Bebchuk, 2002), it gives lenders incentives to provide financing during the reorganization and thus increases the ex ante viability of the reorganization plan (Gertner and Scharfstein, 1991).

the recovery rate) and lack of creditor control made it lengthy and very inefficient (Djankov et al., 2008). As a result, the assets of financially distressed firms were more likely to be sold piecemeal than preserved as a going concern, decreasing the proceeds used to repay creditors.

The Italian Parliament passed the Law[8] in the first quarter of 2005 (Law 80/2005). The objective of the Law was to provide a source of relief for financially but not economically distressed firms, and to increase creditors' recovery rates. The Law modified several legal provisions to foster the viability of in-court and out-of-court debt restructuring agreements. First, it abolished the reimbursement requirements for both secured and unsecured creditors. Second, the Law changed the reorganization voting rule. Under the new formulation, a plan is accepted if the majority of the creditors representing the majority of the outstanding debt vote in favor of it (i.e., unanimity is no longer required). Third, the Law introduced the provision that when a plan cannot be agreed upon, the court can force the adoption of a plan, if the judge believes that creditors will not be any worse off under a reorganization plan than they would be under any alternative solution (i.e., cram down). Fourth, the Law modified the conditions under which a firm can voluntary file for a reorganization plan. Under the new formulation, "the debtor does not have to be insolvent in order to qualify for reorganization" (Law 80/2005). Fifth, the Law allowed the court to enforce the super-senior status granted to loans obtained during the execution of the reorganization plan (i.e., debtor in possession financing). Sixth, the Law introduced an automatic stay on all creditors' claims (i.e., no creditor is allowed to seize collateral) starting from the filing date of the reorganization procedure.[9] Finally, the Law strongly facilitated private debt restructuring

[8] The bankruptcy code regulating public firms was first reformed at the end of 2003 through a Government executive order (DL 347/2003) that was triggered by the urgency of bailing out Parmalat. After that legal intervention, the Parliament started to work on a more comprehensive reform of the entire bankruptcy code. The code has been subsequently amended in 2006, 2009, and 2012.

[9] This represents a key difference with US Chapter 11. Under the US legislation, a debtor has the exclusive right to propose a reorganization plan within 120 days from the Chapter 11 filing date. Under the Italian legislation, a debtor has to submit the plan when she files for the reorganization procedure. This institutional difference likely reduces the borrower's bargaining power over the procedure and her incentives to engage in risk shifting during the procedure.

agreements by introducing the provision that any transaction made during the execution of a restructuring plan certified by an independent expert and approved by the court could not be challenged in the case of subsequent bankruptcy.[10]

The Italian setting is particularly suitable from a research design perspective for three additional reasons. First, firms cannot engage in forum shopping but have to file reorganization and bankruptcy procedures in the court of incorporation.[11] Second, given the low average debt enforcement and high risk of banks' liquidity crisis, Italian firms tend to borrow from multiple banks, which increases coordination costs in cases of financial distress (Detragiache et al., 2000). Third, there is a substantial variation in lending techniques across and within regions (De Biaso et al., 2006), which suggests that the demand for accounting information is potentially elastic across regions and not only in the more developed geographical areas.

3. Related literature and predictions

3.1 The economics of bankruptcy laws

The objective of bankruptcy laws that facilitate the renegotiation of debt contracts, such as US Chapter 11, is to address common pool problems when a firm is in financial distress (Jackson, 1986). Rather than renegotiating debt contracts and solve debt overhang problems (Myers, 1977), creditors have incentives to initiate a run on the firms' assets (Diamond and Dybvig, 1983; Gertner and Scharfstein, 1991). Common pool problems arise from the fact that individual creditors have little incentive to grant concessions in a renegotiation, and instead will hold out in anticipation that

[10] The first two provisions facilitated the feasibility of the procedure by giving contracting parties the right to deviate from the absolute priority rule and, by simplifying the voting schemes, to overcome free riding issues. The third provision addressed hold out and free riding problems by giving courts the right to force the adoption of the plan in cases where it does not make any creditor worse off than the creditor would be under any alternative solution. The fourth provision increased the space of the circumstances under which a firm can file for the reorganization procedure, allowing a prompt reaction to financial distress. The fifth provision resolved the legal uncertainty around the status of loans provided in execution of the plan, lowering debt overhang problems.

[11] In addition, if a firm changes the court of incorporation in the year before the declaration of insolvency, the authority over the procedure still remains in the court in which the firm was previously incorporated.

other creditors will provide the concessions needed to ensure the success of the renegotiation. Since all creditors have similar incentives, renegotiation is likely to fail (Roe, 1987; Asquith et al., 1994). Bankruptcy laws that facilitate the renegotiation of debt contracts protect the assets of financially distressed firms by forcing creditors to act in a cooperative fashion, thus allowing the ex post efficient continuation of viable but financially distressed firms (Djankov et al., 2008).

3.2 Predictions

An important role of accounting information in debt contracting is to facilitate the state-dependent allocation of control rights which regulates the borrower-lender conflict over continuation / liquidation decisions (Aghion and Bolton, 1992; Hart and Moore, 1998). State-dependent contracts allow lenders to lower moral hazard problems (Smith and Warner, 1979; Berlin and Mester, 1992; Dichev and Skinner, 2002; Kaplan and Stromberg, 2003) without engaging in the active relationship-based monitoring associated with concentrated debt, and, thus, allow risk sharing (Diamond, 1984; Diamond, 2007). The literature documents that financial statements that reflect deterioration of a borrower credit quality on a timely basis are more contractible as they support the use of debt covenants as trip wires (Leftwich, 1983; Beatty et al., 2008; Nikolaev, 2010; Christensen and Nikolaev, 2012), allow more decentralized and transactional lending based on arm's length monitoring, and, in turn, relax borrowers' financial constraints (Ball et al., 2008a; Bharath et al., 2008; Gormley et al., 2012). More specifically, extant research shows that greater contractibility of financial statements is associated with lower borrowing costs, more transactional lending, more diffuse loan syndicate structures, where the lead arranger retains a smaller fraction of the syndicated loan, and lower switching costs.[12]

[12] Basu (1997), Ahmed et al. (2002), Zhang (2008), Wittenberg-Moerman (2008), Bharat et al. (2008), Ball et al. (2008b), Costello and Wittenberg-Moerman (2011), Gormley et al. (2012), Breuer et al. (2015).

However, disclosing bad news on a timely basis accelerates the transfer of control rights to creditors, giving rise to hold up concern and to the risk of premature liquidation if there are impediments to efficient renegotiation (Leuz, 2001; Gigler et al., 2009; Li, 2013). Coordination problems among creditors magnify the risk of premature liquidation when a firm is in financial distress, as each individual creditor has the incentive to call the loan or seize the collateral, if she fears other creditors will do the same (Diamond and Dybvig, 1983; Morris and Shin, 2004). Such fears would become self-fulfilling, since the disorderly liquidation of assets and the consequent disruption to a project are more likely to lead to failure of the project. The recent trend around the world of reforms to bankruptcy laws in the spirit of US Chapter 11 can be chalked up to the acknowledgement of the importance of coordination failures (Franks et al., 2015).

The introduction of the Bankruptcy reform, by facilitating the in-court and out-of-court renegotiation of debt contracts, is expected to create incentives to increase the contractibility of accounting information through three non-mutually exclusive ways. First, by facilitating the ex post renegotiation of debt contracts in the case of financial distress, the Law decreases the likelihood that the borrower will be exposed to the risk of premature liquidation due to coordination failures upon the disclosure of bad news (Hertzberg et al., 2011) or due inefficient bargaining following the transfer of control rights (Beneish and Press, 1993; Chava and Roberts, 2008; Li et al., 2015). All things being equal, this will decrease the cost of disclosing bad news in financial statements in a timely manner[13] (Baird, 1991; Heinkel and Zechner, 1993; Povel, 1999) which, in turn, makes financial statements more contractible (Watts, 2003; Ball and Shivakumar, 2005; Nikolaev, 2010; Christensen and Nikolaev, 2012). Second, an increase in the ability to

[13] DeAngelo et al. (1994) show that accounting choices of firms in financial distress primary reflect acknowledgement of their financial troubles rather than attempts to avoid covenant violations. My results are consistent with their findings, and further show that the ability to renegotiate debt contracts is a force that concurs to shape firms' incentives to disclose bad news in a timely fashion.

renegotiate debt contracts when financial conditions deteriorate allows to achieve ex post efficient outcomes following the transfer of control rights, improving the effectiveness of debt covenants as trip wires (Smith and Warner, 1979; Berlin and Mester, 1992; Dichev and Skinner, 2002). Accounting-based covenants are monitoring mechanisms which address moral hazard problems by giving creditors the right to call the loan after a violation. Even if the literature has documented that covenant violations are common and occur before a firm is in danger of a payment default (Skinner and Dichev, 2002; Chava and Roberts, 2008), their effectiveness as monitoring mechanisms depends on the ability of the contracting parties to renegotiate debt contracts after a violation. Indeed, lenders use the option of waiver of a violation to renegotiate the debt agreement and potentially require stronger contractual restrictions, like more collateral and higher interest rates. Before the Law came into force, private renegotiations that followed a covenant violation faced the risk of nullification by the courts in the case of a firm's subsequent bankruptcy due to extensive claw-back provisions. The Law limited the impact of claw-back provisions and facilitated private debt renegotiations, increasing incentives to rely on covenants in monitoring.[14] Given that accounting-based covenants require a contractible signal through which the parties agree to shift control rights (Nikoalev, 2010; Christensen and Nikolaev, 2012; Dou, 2015), the higher effectiveness of covenants as a monitoring mechanism will create a demand for contractible accounting information. Third, a decrease in renegotiation costs will make it less costly to expand the pool of lenders (Dewatripont and Maskin, 1995; Bolton and Sharfestein, 1996) to reduce, for example, liquidity risk (Detragiache et al., 2000) or hold up concerns (Rajan, 1992). Given that

[14] Let's assume that before the introduction the Law, the lender responded to a covenant violation by increasing the interest spread, or demanding additional collateral. If these transactions occurred in the 12 months leading up to the declaration of insolvency, the court-appointed trustee had the authority to claw-back them. In addition, the trustee had the authority to claw-back all the transactions occurring in the 24 before the declaration of insolvency, if she provided proof that the beneficiary was aware of the firm's financial distress. Under the new legislation, if these transactions are made during the execution of a restructuring plan certified by an independent expert and by the court, then the trustee cannot set these transactions aside.

multiple lenders will have fewer incentives to gather soft information in monitoring the borrower (Stein, 2002; Berger et al., 2005), the demand for hard and contractible information will increase.

4. Data and variables measurement

4.1 Data

I draw data from several sources. I collect accounting data on Italian private firms from the AIDA database provided by Bureau Van Dijck (BVD). I use the annual CD-ROMs supplied by BVD to retrieve non-static information on industry and municipality of incorporation. Since firms cannot engage in forum shopping under the Italian Law, the municipality of incorporation determines which court has the authority to deal with the legal proceedings that pertain to a firm.

I restrict the analysis to private firms with at least EUR 1 million in total assets and sales over the period 2002-2008. These sample requirements are justified on three grounds. First, public firms were exposed to several regulatory changes during the sample period (e.g., IFRS adoption, reform of the governance code in 2003). Second, my identification strategy relies on variation across courts and thus requires a sufficient number of observations for as many judicial districts as possible. Because Italian public firms are incorporated in only a few big cities, their use is unfeasible for my analyses. Third, firms with less than EUR 1 million in total assets and sales are sparsely covered in the AIDA–BVD database. I further exclude firms in the financial and utility industries (SICs 6000-6999 and 4000-4499). This leaves a starting sample of 836,734 firm-years. Next, I limit the sample to firm-year observations with the following available data item: total assets, sales, book value of equity, net income, and accruals. I truncate these variables above the 99th percentile and below the 1st percentile. Finally, I drop firms that were involved in mergers and acquisitions during the sample period (Hribar and Collins, 2002). Imposing these data restrictions leaves a sample of 242,471 firm years of 36,938 unique firms.

The second source of data is the Italian Ministry of Justice, which provides information on the number of judges per court, pending cases per judge at the beginning of each year, new legal cases, closed cases within a year, administrative staff per court, and the length of legal procedures by type of legal proceeding for each court.[15] The database provides also the list of municipalities under the authority of a given court, allowing the match between courts and firms. Each court in the Italian judicial system administers civil, bankruptcy, and criminal cases. The geographical organization of the judicial system over the sample period is based on 165 judicial districts. It has been mainly determined by historical reasons and still resembles the structure shaped in 1865 after the Italian unification. Since then, no existing district has been removed.[16] I was able to retrieve data for 163 courts. The third source of data is the Italian Institute of Statistics, which provides information on demographic, social, and economic characteristics for each judicial district. Finally, I use data from the Mediocredito Centrale Survey for information on firms' financial constraints and number of lenders from 2003.[17]

4.2 Court efficiency

I follow extant literature on judicial performance (Dakolias, 1999; Djankov et al., 2008; Ponticelli, 2014) and measure court efficiency using the negative of the average work backlog per judge, computed as minus one times the logarithm of the number of pending cases in a civil court at the beginning of 2005 over the number of civil judges in that court.[18]. Table 1, panel A reports the descriptive statistics of the judicial variables for 2005. Italian courts have, on average, 54

[15] All court-level data can be downloaded from http://www3.istat.it/dati/catalogo/.
[16] A major revision of the geographical organization of the judicial system took place in 2012 (Law 155-156/2012) which suppressed 31 courts. Over the previous 50 years, only 11 new courts had been established (the last six in the 1990s).
[17] The Mediocredito Centrale Survey provides a representative sample of Italian manufacturing firms. The sample is stratified by industry, geographical area, and size for firms with from 10 to 500 employees, while it includes all firms with more than 500 employees (See Detragiache et al., 2000 for a full description of the data).
[18] The congestion of the civil courts is measured considering bankruptcy cases. In additional specifications, I use the length of the bankruptcy procedures at the beginning of 2005 as a measure of court efficiency. However, I can retrieve information on the length of the bankruptcy procedures only for 116 courts.

judges and 208 staff members. However, both distributions are highly skewed to the left. The average congestion rate is 674 pending cases per judge. The average length of civil cases is 800 days, while the length of bankruptcy procedures is 2,891 days. Both metrics present substantial variation with a standard deviation of 238 for civil proceedings, and 651 for bankruptcy cases. Figure 1 presents the distribution of court congestion across Italy where courts are separated in deciles of the average backlog per judge. Figure 1 provides visual evidence that variation in court congestion tends to follow the north-south route. However, even though less congested courts are in the north while more congested courts are in the south, there is still considerable variation in court congestion across and within regions.

[Table 1, panel A here]

[Figure 1 here]

Figure 2 reports the distribution of firms across Italian courts. Even though the majority of firms are incorporated in the north, there is reasonable variation across and within regions.

[Figure 2 here]

4.3 Contractibility of accounting information

I use timely loss recognition (*TLR*) as the main proxy for the contractibility of accounting information. I measure *TLR* following Ball and Shivakumar (2005) using the non-linear relationship between operating cash flows and accruals. The model is as follows:

$$Accruals_{it} = \beta_1 DCFO_{it}\,(CFO_{it} < 0) + \beta_2\, CFO_{it} + \beta_3\, DCFO_{it}\,(CFO_{it} < 0) \times CFO_{it} + \varepsilon_{it}$$

The dependent variable ACC_{it} is accruals computed as $[(\Delta CA_{it} - \Delta Cash_{it}) - (\Delta CL_{it} - \Delta STD_{it}) - DEP_{it}]$ scaled by total assets for firm *i* at the beginning of year *t*, where ΔCA is the change in current assets, $\Delta Cash$ is the change in cash and bank balances, ΔCL is the change in current liabilities, ΔSTD is the change in short term debt, and *DEP* is depreciation expense. *CFO* is the operating

cash flows (scaled by total assets at the beginning of the year), computed as the difference between net income and *Accruals*, scaled by total assets at the beginning of the year. *DCFO* is an indicator variable equal to one if *CFO* is negative, and zero otherwise. The timelier a firm is in the recognition of losses, the stronger the positive correlation between accruals, (*ACC*) and operating cash flows (*CFO*) when cash flows are negative. Thus, the level of timely loss recognition increases in the coefficient β_3. In additional specifications, I consider accounting practices that can potentially affect *TLR*, such as write-offs of fixed assets scaled by gross fixed assets, bad debt expenses over total receivables, and extraordinary expenses over sales.

5. Identification strategy

I exploit the adoption of the bankruptcy reform in 2005 to estimate the effect of an increase in the ability to renegotiate debt contracts on firms' incentives to increase the contractibility of accounting information. Because the Law is at country-level, it applies to all firms in my sample. However, given that the effect of the Law on the behaviors of the contracting parties is likely to be predicated on the effective enforcement by courts (Dakolias, 1999; Djankov et al., 2008; Ponticelli, 2014), I exploit cross-sectional variation in court congestion at the beginning of 2005 to provide a counterfactual of its impact. The underlying assumption is that firms incorporated in more congested courts are less likely to be affected by the legal change and, thus, I can use court efficiency as a source of variation in the effect of the bankruptcy reform. I thus propose the following difference-in-differences specification:

$$y_{it} = \alpha_i + \alpha_t + \varphi Post_t \times Log(Court\ Efficiency_j) + \eta_{it} \quad (1)$$

The dependent variable, *y*, is a proxy for the contractibility of accounting information for firm *i* incorporated in judicial district *j* in year *t*. α_i is a firm fixed effect which captures time-

invariant unobservable heterogeneity across firms,[19] while α_t is a year fixed effect to account for common macro shocks. *Post* is a dummy variable equal to one after 2005, zero otherwise. *Log(Court Efficiency)* is the negative of the logarithm of the number of pending cases per judge at the beginning of 2005. The right-hand side coefficient of interest, φ, corresponding to the interaction between *Post* and *Log(Court Efficiency)*, is the DID estimator of the effect of the Law on the contractibility of accounting information. It measures whether firms incorporated in more efficient courts experience a greater increase in the contractibility of accounting information after the introduction of the Law relative to firms in less efficient courts.[20]

The DID estimator in equation (1) relies on a common trends assumption and assumes no selection on transitory shocks. However, there are many reasons to believe that in my setting, at the very least, the first assumption would not hold. First, firms incorporated in judicial districts that differ in the level of court efficiency may be exposed to differential growth shocks that could correlate with firms' reporting practices. Second, court congestion is not randomly assigned across municipalities. Firms incorporated in judicial districts that differ in the level of efficiency are likely to be systematically heterogeneous along several margins. For example, firms in less efficient courts may be smaller, more likely to be financially constrained, or rely more on relationship lenders than firms incorporated in more efficient courts (Diamond, 2004; Jappelli et al., 2005; Giacomelli and Menon, 2013). Such heterogeneity may lead to differential financing trends that

[19] Note that when I measure the contractibility of accounting information using the Ball and Shivakumar (2005) model, firm fixed effects capture time-invariant unobservable heterogeneity across firms only for the level of accruals and not for the level of *TLR*.
[20] Given that in the Ball and Shivakumar model, the coefficient of interest is given by the interaction between cash flows (*CFO*) and a dummy that marks negative cash flows (*DCFO*), when I estimate equation (1) using the Ball and Shivakumar model, the treatment is given by the coefficient on the interaction term: *Post* × *Log(Court Efficiency)* × *CFO* × *DCFO*. I thus include in the equations the following variables: *CFO, DCFO, CFO* × *DCFO, Post* × *Log(Court Efficiency)* × *CFO, Post* × *Log(Court Efficiency)* × *DCFO*, and a full set of court fixed effects interacted with *CFO, DCFO, CFO* × *DCFO*. I also included year fixed effects interacted with *CFO, DCFO, CFO* × *DCFO*. Accordingly, the following variables get subsumed by the fixed effect structure: *Post, Log(Court Efficiency), Post* × *CFO, Post* × *DCFO, Post* × *CFO* × *DCFO, Log(Court Efficiency)* × *CFO, Log(Court Efficiency)* × *DCFO, Log(Court Efficiency)* × *CFO* × *DCFO.*

correlate with the supply and demand for accounting information, or with heterogeneous reporting choices' reactions to the same growth shock.[21]

I address these concerns in the following ways. First, I control nonparametrically for differences across courts by restricting the sample to firms incorporated in municipalities located across judicial borders and by replacing year fixed effects with border-year fixed effects. In this way, the treatment effect is identified by comparing within-firm changes across neighboring municipalities along a judicial border between two adjacent courts. Since firms incorporated in municipalities on the sides of a judicial district border are likely to be exposed to similar growth shocks and trends, this approach lowers the concern of omitted variable bias driven by local shocks at the municipality level. However, even if firms incorporated in municipalities on the sides of a judicial border are exposed to similar local shocks, their reactions may be different if court efficiency correlates with cross-judicial district heterogeneity in the distribution of firms, industries, or characteristics of the banking structure that lead to a greater demand for accounting information. For example, if court efficiency facilitates transactional lending, following the same growth shock, firms incorporated in more efficient courts might increase the contractibility of accounting information to a greater extent than firms in less efficient courts. However, if these factors do not exhibit discontinuities at the judicial borders, then my empirical strategy, which restricts the identifying variation to discontinuous changes in court efficiency between two adjacent judicial districts, lowers the concern that my estimates are biased by omitted correlated

[21] Differences in levels across firms or shocks that correlate with court efficiency but not with the paths of the contractibility of accounting information over the sample period will not affect my estimates, given they get differenced out in the difference-in-differences. The same goes for differences in levels or shocks which are orthogonal to court efficiency. On the other hand, differences across judicial districts that correlate with the dynamic of the dependent variable over the sample period through the court enforcement channel or through channels that are somehow bundled with court enforcement lead the parallel trend assumption to not hold. For example, a negative growth shock that affects firms' liquidation value would result in differential levels of timely loss recognition depending on the efficiency of the court, if court enforcement affects the extent to which parties rely on accounting information to estimate the liquidation value of firms' assets.

local factors which vary smoothly over space. This approach is in the spirit of a spatial discontinuity design (Card and Krueger, 1992) in which the identifying assumption is that crossing a judicial border reflects a discrete jump in court efficiency while all the other factors that determine the outcome of interest are continuous at the border.[22] Figure (3) shows how the identification strategy is implemented. In a nutshell, I restrict the sample to municipalities on the sides of judicial borders (i.e., colored municipalities), and include a fixed effect for each border-year. I thus propose the following difference-in-differences specification, where I restrict the sample to firms incorporated in municipalities at the sides of judicial district borders.[23]

$$y_{it} = \alpha_i + \alpha_{kt} + \varphi Post_t \times Log(Court\ Efficiency_j) + \gamma_1 Post_t \times X_j + \gamma_2 Post_t \times X_i + \eta_{it} \quad (2)$$

In equation (2), I replace the year fixed effects with border-year fixed effects (α_{kt}), implying that the coefficient of interest, φ, capturing the effect of the Law, is identified by exploiting discontinuous changes in court efficiency within a judicial border. I further control parametrically for residual heterogeneity across districts, and firms within a judicial district, by including two vectors of control variables, X_j and X_i, into equation (2) at the court and firm level. I measure the controls at the end of 2004 to avoid bad control problems, and I have them interact with the *Post* dummy to allow the relative coefficients to vary between the pre- and post-reform period. I include

[22] Court jurisdictions do not overlap with other administrative classifications, even if in most of the cases judicial district borders coincide with regional and provincial ones. In these cases, discontinuities unrelated to court efficiency might be introduced. However, this is a concern only if these discontinuities are correlated with court efficiency. A feature of the Italian institutional setting makes this eventuality unlikely. Courts are fully autonomous from the local administrative authorities (i.e., regions and provinces), which in turn do not have executive or legislative powers that could directly explain variation in economic activities, development of the banking system, or enforcement. Provinces are mainly endowed with environmental, road maintenance, and primary schooling duties. On the other, regions are entitled with some executive and legislative powers over economic activities. However, any time-invariant difference across Italian regions gets differenced out by my fixed effects structure.

[23] I identify 369 border-groups. To identify municipalities on a given border, I employ an automated procedure using a Geographical Information System (GIS) as in Giacomelli and Menon (2013). For each municipality I require the GIS to identify all jurisdiction polygons at zero distance from the municipality border. Border municipalities are those with two or more jurisdictions at zero distance. If there is more than one contiguous jurisdiction, only one is selected based on the distance between the municipality and jurisdiction centroids. Each border group includes all municipalities sharing the same couple of zero-distance jurisdiction.

the following court-level variables: logarithm of the average household income, logarithm of population, logarithm of population density, logarithm of number of bank branches, unemployment rate, and logarithm of the agricultural share of GDP. Firm-level controls include size, measured as total sales, leverage, measured as the ratio between total debt and total assets, and the percentage change in sale. I cluster standard errors at court-level to allow any arbitrary serial- and cross-correlation within courts, thus using only variation across the 163 courts to compute the standard errors.

There are still two sources of concern. First, while firms cannot engage in forum shopping, they can change their location, i.e., they can manipulate the forcing variable. For example, if firms experiencing a growth shock around 2005 expect to benefit more by moving to less congested courts, then my estimates might be upward biased. While this might be plausible in the long run, it is unlikely to hold in the short run, as changing the judicial district of incorporation involves substantial costs and time. However, I address this concern by restricting the sample to firms that do not change their court of incorporation over the sample period. Another source of concern is that variation in court efficiency could be correlated with other cross-district differences in the distribution of firms, industries, or banking structures that vary *discontinuously* at the border. To address this concern, I propose an instrumental variable strategy based on the increase in court congestion driven by the exogenous jump in the intensity of the opium seizure rate over the Italian drug trafficking routes after the War in Afghanistan (see Section 6.7).

6. Results

6. 1 Descriptive statistics

Table 1, panel C presents the descriptive statistics on the firm-level variables. The average total assets is 7.2 million and the median is 3.6 million. Average sales is 6.7 million, with a median

of 4.6 million. Total assets and sales are highly right-skewed. Leverage is on average 68.1% of total assets with a median of 72.5%. Accruals are, on average, 2.6% of total assets.

[Table 1, panel C here]

Table 2, columns (1) – (6) reports the breakdown of the sample with respect to the median of court efficiency. On average, more efficient courts are located in districts that are more populated, and that have richer households, an economy which relies less on agriculture, and more developed credit markets. In addition, and most importantly for my identification strategy, more efficient courts are associated with larger recovery rates, with faster civil and bankruptcy procedures, and with a greater incidence of reorganization procedures, while the difference in the number of bankruptcy cases between more and less efficient courts is negligible. However, the heterogeneity in credit market and economic development across courts highlights the importance of my border design, which tries to reduce as much as possible differences across judicial districts.

[Table 2 here]

6.2 Main OLS results

I first provide graphical evidence of the trends in *TLR*. This analysis intends to explore the existence of any pre-Law differential trends across courts, and to gauge the timing of the effect. I estimate the *TLR* coefficient (i.e., *CFO* × *DCFO*) for each border-court-year using only firms in municipalities at the borders of a judicial district. Then, I create two non-overlapping groups based on the within judicial border court efficiency distribution. Specifically, I assign a court-border-year *TLR* coefficient to the high court efficiency group if the coefficient is relative to the court with the higher level of efficiency within the border. Figure 4 plots the court-year *TLR* coefficients separately for the high and low court efficiency group. The graph shows that even if the level of *TLR* is higher in more efficient courts than in less efficient courts, the dynamic is reasonably

parallel before the adoption of the Law. Most importantly, the difference in *TLR* between more and less efficient courts widens after the introduction of the Law with a jump around 2005.

[Figure 4 here]

Table 3, panel A reports the average effect of the Law on *TLR*. In columns (1) – (5), the coefficient of interest, *Post* × *CFO* × *DCFO* × *Log(Court Efficiency)*, is positive and significant at the 1% level across model specifications. This result implies that firms in more efficient courts experience a larger increase in *TLR* after the Law than firms in less efficient courts. The magnitudes are comparable irrespective of the fixed effects structure and estimation sample. In particular, the magnitudes and significance levels are unchanged when I restrict the sample to firms incorporated in municipalities located across judicial district borders, and employ border-year fixed effects [columns (2) and (4) – (5)]. In addition, the magnitudes and precision of the estimates are not affected when I control for initial court-level characteristics [columns (3) – (5)]. Restricting the sample to firms that do not change location over the sample period does not alter the overall inference, which lowers the concern that my estimates are biased by forum shopping [column (5)]. In columns (6) – (7), I report the results from the estimation of equation (2), where the dependent variable is the *TLR* coefficient (i.e., *CFO* × *DCFO*) estimated for each municipality-year. In these specifications the treatment of interest is given by *Post* × *Log(Court Efficiency)*, and it is estimated by exploiting only within-border variation in court efficiency between two adjacent judicial districts.[24] Consistent with the above results, I find that firms incorporated in more efficient courts experience a larger increase in *TLR* after the Law relative to firms in less efficient courts.

In table 3, panel B, I look at changes in accounting practices that potentially result in an increase in *TLR*. I report only the results for the border sample with border-year fixed effects,

[24] In these specifications, the dependent variable is measured at municipality-year level. Therefore, I replace firm fixed effects with court fixed effects. Using municipality fixed effects does not affect the overall inference.

which is my preferred specification. I find that the introduction of the Law is associated with a change in reporting practices that contribute to *TLR*. Specifically, I find that after the introduction of the Law, firms in more efficient courts increase the write-offs of fixed assets scaled by gross fixed assets [columns (1) and (4)], bad debt expenses over total receivables [columns (2) and (5)], and extraordinary expenses over sales [columns (3) and (6)].

[Table 3 here]

6. 3 Cross-sectional tests and alternative explanations

In this section, I examine heterogeneity in the average effect. The underlying idea of these analyses is that it is unlikely that the Law has uniform effects across firms. Rather, a reform that facilitates the renegotiation of debt contracts will affect reporting practices according to how this shift affects the cost-benefit trade-off that underlies a firm's accounting choices.

I first examine whether the magnitude of the change in *TLR* depends on the extent to which a firm can pledge collateral. Firms with less collateral to pledge are more likely to address contracting costs through the state contingent allocation of control rights than firms with more collateral to pledge (Berger and Udell, 1990; Skinner, 1993; Christensen and Nikolaev, 2012).[25] As a result, the former group is expected to react more strongly to a reform which, by facilitating renegotiation, affects the ability to use debt covenants as trip wires (Dichev and Skinner, 2002). I thus estimate equation (2) separately for each quartile of the pre-reform asset tangibility distribution. Even if noisy, asset tangibility is a widely used proxy for the ability to pledge collateral (Vig, 2013). I compute asset tangibility as the ratio between gross property, plant, and equipment and total assets at the end of 2004. Table 4 reports the estimation results. I find that the

[25] This does not imply that accounting information is not useful for monitoring the value of collateral (Minnis and Sutherland, 2015). Rather, firms with more collateral to pledge will be less sensitive to a legal change that decreases renegotiation costs than firms with less collateral to pledge, as the former group, by pledging collateral, makes the value of the lenders' claims less sensitive to the underlying performance.

effect of the Law on *TLR* is larger among firms with less collateral to pledge, specifically for firms in the second and third quartile of the tangibility distribution [columns (2) – (3)].

[Table 4 here]

Next, I turn to examine heterogeneity related to firms' number of banking relationships, financial constraints, and bank fragility. Data on the number of lenders and financial constraints comes from the Mediocredito Centrale Survey for the year 2003.[26] Data on bank fragility comes from the Bank of Italy Data Warehouse.

In deciding the number of lenders, a firm will trade off coordination and hold up concerns. By decreasing the risk of coordination failure in cases of financial distress, the Law will likely increase the optimal number of lenders. In turn, this will increase incentives to make financial statements more contractible, as long as multiple lenders are more likely to rely on hard, contractible information in monitoring (Berger et al., 2005). I thus examine whether the effect of the Law depends on a borrower's number of banking relationships in the pre-reform period. I form three groups based on whether a firm has: (i) one bank, (ii) two to three banks, or (iii) more than three banks. Table 5, columns (1) – (3) show that firms in the middle of the pre-reform number of lender distribution experience the larger increase in *TLR*, while firms in the upper tail and firms borrowing from only one bank experience a negligible change in reporting practices. While firms in the upper tail of the distribution were already more likely to rely on financial statements in contracting (Ball et al., 2008a), firms borrowing from one bank may face too severe adverse selection problems when they try to switch lenders (Sharpe, 1990; Rajan, 1992), or may have very low growth opportunities to begin with (Detragiache et al., 2000).

[26] The drawback of using that the Survey is that it covers only 4,239 *unique* firms, so that I cannot rely on the border design.

Firms that borrow from banks that are more prone to liquidity shocks will be more sensitive to the Law (Diamond, 1991; Detragiache et al., 2000). If there is a positive probability that the current lender faces liquidity problems, then the borrower will find it optimal to increase the number of lenders to reduce the likelihood of not being able to finance positive *NPV* projects. However, increasing the number of lenders increases renegotiation costs which, in turn, fosters the risk of coordination failure when a firm is close to distress (Gerter and Scharfstein, 1991; Bolton and Scharfstein, 1995). If the Law has lowered the cost of contracting with multiple lenders, then firms in districts where banks are more likely to face liquidity shortages are expected to increase the contractibility of accounting information to a greater extent (Petersen and Rajan, 2002; Berger et al., 2005). Following Detragiache et al. (2000) I measure bank fragility at district-level using the ratio of nonperforming loans to total assets. The idea is that banks with a high share of non-performing loans are more prone to liquidity shocks. Then, I split the sample into two groups based on the sample median. Given that I measure bank fragility at district-level, I cannot employ the border-design with border-year fixed effects. Consistent with theory, table 5, columns (4) – (5) show that the increase in *TLR* is larger in districts where banks are more prone to liquidity shocks.

The trade-off between coordination and hold up concerns also affects the extent to which a firm suffers underinvestment problems (Detragiache et al., 2000). If, in the pre-reform period, firms with concentrated debt structures had to pass out positive *NPV* projects because of banks' liquidity shortages (Detragiache et al., 2000), then the Law, by decreasing the cost of contracting with multiple lenders, will have a larger effect on financially constrained firms. Consistent with the above intuition, I find that the effect is stronger for firms declaring to be financially constrained in the pre-reform period [columns (6) – (7)].

[Table 5 here]

Firms dependent on external financing are more likely to be sensitive to a legal change which facilitates debt renegotiation. To explore the empirical relevance of this argument, I break down the sample using the Rajan and Zingales (1998) industry classification. I measure external financing dependence using data from US firms for the year 2004 under the assumption that industry-level external financing needs are homogenous across countries. Then, I split the sample into two groups according to the sample median of the external financing dependence measure. Consistent with the prediction that firms in industries which rely more on external financing are more sensitive to a decrease in renegotiation costs, I find that the increase in *TLR* is larger in financially dependent industries [columns (1) – (2)].

Bank competition is likely to shape the effect of the Law. On the one hand, in districts with fierce interbank competition, lenders have fewer incentives to specialize in the acquisition of specific information and thus to enter into a relationship with opaque borrowers to begin with (Petersen and Rajan, 1995; Stein, 2002). Thus, the demand for accounting information is likely to be more elastic where interbank competition is stronger, leading the Law to have a larger effect in more competitive districts. On the other hand, in districts with high interbank competition, the information environment is likely to be already rich (Boot and Thakor, 2000), leading the Law to have smaller effects in more competitive districts. I compute bank competition using the number of bank branches over the number of firms in a judicial district for the year 2004. Then, I split the sample into two groups according to the sample median. Given that the variation in the partitioning variable is at the district-level, I do not employ the border design with border-year fixed effects. Table 6, columns (3) – (4) report the estimation results. Even if the increase in *TLR* is slightly larger in more competitive districts than in less competitive districts, the difference in magnitude is negligible. This evidence suggests that differences in the structure of the banking system across

judicial districts do not strongly shape the demand for accounting information in my setting, and that I am not simply picking heterogeneity in credit market development.

An alternative channel through which the Law may affect reporting practices is an increase in financing driven by an increase in the expected recovery rate. If this channel concurs to explain my results, then firms with less redeployable assets should be more affected by the Law. The rationale is that less redeployable assets are much more difficult to be sold across industries, so they have lower liquidation values (Shleifer and Vishny, 1992) and are associated with lower recovery rates in cases of distress (Asquith et al., 1994). As a result, by facilitating the ex post efficient continuation of a firm, the effect of the Law on the expected recovery rate is likely to be larger for firms with less redeployable assets. To test the empirical relevance of this channel, I compute asset redeployability at industry-level following Kim and Kung (2013). I use the Bureau of Economic Analysis input-output table for the year 2004 (which breaks down the capital expenditures of 123 industries into 180 asset categories), and measure the across-industry redeployability of an asset by computing the proportion of industries in which the asset is used. Then, I compute the industry-level redeployability index as the value-weighted average of each asset redeployability score. Hence, a higher value of the index implies that the assets used in a given industry can be sold easily. Then, I split the sample into two groups according to the median of the redeployability score. Table 6, columns (5) – (6) report the results. The increase in *TLR* is only slightly larger for firms in the low assets redeployability group than for firms in the high assets redeployability group. While only suggestive, this evidence indicates that a change in the expected recovery rate is unlikely to be the major driver of my results.

[Table 6 here]

6. 4 Change in lending

To explore the mechanism behind the change in reporting practices, I examine the change in lending, and how this maps onto the change in reporting practices. All things being equal, if the Law increases incentives to make financial statements more contractible, then firms will find it more beneficial, at the margin, to increase the pool of lenders or switch to transactional lenders.

I first examine whether the increase in *TLR* is accompanied by an increase in debt. I thus split the sample into terciles of the change in debt between 2004 and 2006, scaled by total assets at the beginning of 2004. Firms belonging to the lowest tercile are assigned to the *debt reduction* group, firms belonging to the middle tercile are assigned to the *no debt change* group, and firms belonging to the top tercile are assigned to the *debt increase* group. Then, I estimate equation (2) separately for each tercile. Table 7 reports the estimation results. I find that the increase in *TLR* is concentrated among firms that maintain or increase the level of debt, while the coefficient on *TLR* for firms in the *debt reduction* group is close to zero and not statistically significant.

To explore whether this result is simply the consequence of concurrent shocks to firms' growth opportunities, which, in turn, affects reporting practices, I conduct a falsification test in which I counterfactually anticipate the year of the treatment by two years (i.e., 2003) and split the sample in terciles of the change in debt between 2002 and 2004 over total assets in 2002. Given that my treatment of interest is the differential effect of the Law on *TLR* between more and less efficient courts, any growth opportunities shock will bias my results only if it differentially affects firms through a court enforcement channel. However, without a change in the ex post ability to renegotiate debt contracts, this is not obvious or ex ante unclear. Columns (4) – (5) present the estimation results from this falsification exercise. I find only negligible evidence that firms experiencing a positive change in debt increase the level of *TLR.* The *TLR* coefficient for firms in

the *debt increase* placebo group is positive but economically small and below the conventional level of significance [column (4)]. In column (5), I explore this issue further by not constraining the *TLR* coefficient for the *debt increase* placebo group on court efficiency. The coefficient on *TLR* is positive, significant, and only slightly lower in magnitude than the treatment in column (3). Taken together, this falsification test suggests that growth opportunity shocks correlated with the timing of the reform are unlikely to be the major driver of my results. Indeed, while an increase in debt positively correlates with *TLR*, it does not differentially affect firms in courts that differ in the level of efficiency without a change in the ability to renegotiate debt contracts.

[Table 7 here]

Next, I examine the changes in lender behavior. If the Law has fostered incentives to make financial statements more contractible, then, at the margin, I expect the supply of loans provided by large banks, which are less likely to rely on soft information for monitoring purposes (Stein, 2002; Berger et al., 2005), to increase relative to loans supplied by small banks. To explore the empirical relevance of this prediction, I collect data on corporate loans at quarter-court-bank size level from the Bank of Italy Data Warehouse. Then, I regress the ratio of corporate loans provided by big (small) banks to total loans on the interaction between *Post* × *Log (Court Efficiency)*, quarter-year and court fixed effects. Table 8 reports the estimation results. I find that the supply of corporate loans provided by large banks increases to a greater extent in more efficient courts than in less efficient courts [column (1)], while the change in the supply of loans provided by small banks is close to zero and not statistically significant [column (2)]. The change in the supply of loans is not driven by the denominator as long as the effect of the Law on the ratio of loans provided by large (small) banks to the average total loans in the pre-reform period is positive (close to zero) and significant (insignificant) [columns (3) – (4)]. This effect is also unlikely to be driven by a

change in the structure of the banking industry over the sample period, as the change in the ratio of small (large) bank branches to the total number of branches between more and less efficient courts is not statistically nor economically significant [columns (5) – (8)].

[Table 8 here]

6. 5 Change in lenders' risk taking

A plausible alternative explanation is that my results are driven by a relaxation of the financial constraints of less creditworthy firms. When expecting larger recovery rates in the case of insolvency, lenders may increase credit extensions to risky borrowers. This might increase the frequency of losses, which in turn affects the proxies I employ to measure the contractibility of accounting information. If this is true, then the data should show an increase in the percentage of defaults, and banks' loan loss provisions, with a larger effect in more efficient courts. To explore this alternative explanation, I collect quarterly-court data from the Bank of Italy Data Warehouse on default rates and banks' loan loss provisions on corporate loans. I propose two alternative specifications. First, I use default rates and banks' loan loss provisions on household loans as counterfactual, as they are not affected by the Law but are likely to be subject to similar economic trends and changes in banks' business model. This specification permits the use of court-year-quarter fixed effects, as it relies on within-court variation between corporate and household loans. I thus regress default rates (banks' loan loss provisions) on an indicator variable ($Post_{corporate}$) marking observations for quarters after March 2005 *and* corporate loans. I include court-year-quarter fixed effects and a dummy marking the type of loan (i.e., corporate versus household). Standard errors are clustered at court-level. Table 9, columns (1) and (3) report the estimation results. For both of the dependent variables, the magnitude of the treatment is close to zero.

Collectively, I do not find evidence of an increase in the incidence of defaults or in loan loss provisions after the introduction of the Law.

In the previous test, I kept the variation in court efficiency constant. In the second specification, I focus on corporate loans only, and test for the existence of differential patterns in default rates and loan loss provisions across courts. I thus regress default rates (banks' loan loss provisions) in a given court-quarter on the interaction between *Post* and *Log(Court Efficiency),* court and quarter-year fixed effects as well as the full set of control variables. Standard errors are clustered at court-level. Table 9, columns (2) and (4) report the estimation results. I do not find evidence that the change in default rates and loan loss provisions on corporate loans differs between more and less efficient courts after the Law. The coefficient on the interaction between *Post* and *Log(Court Efficiency)* is economically negligible and statistically insignificant across the various specifications.

[Table 9 here]

6. 6 Dynamic of the treatment and falsification analysis

In this section, I carry out a battery of additional analyses. First, I estimate the dynamic of the treatment. This analysis permits to investigate the validity of the parallel trends assumption, and further reinforce the interpretation of the main results. I thus modify equation (2) by counterfactually anticipating (postponing) the coefficient on the treatment, *Post,* by one and two years. Table 10, columns (1) – (4) report the estimation results. Consistent with figure 4, I do not find evidence of differential trends in *TLR* across courts in the years leading up to the Law [columns (1) – (2)], as the placebo coefficients, $Post_{t-1}$ and $Post_{t-2}$, are small in magnitude and not statistically significant. On the other hand, I find that the magnitude of the treatment declines but remains positive and statistically significant as I move away from the adoption of the Law

[columns (3) – (4)]. This evidence suggests that the change in reporting practices occurs around the time of the treatment, and that it does not reverse in the post-reform period.

Then, I conduct a falsification test in which I replace court efficiency with two variables that correlate with a judicial district sensitivity to growth shocks. If court efficiency simply reflects the degree of economic development, then my results may just be driven by the way that districts with differences in the level of development react to local growth shocks. I thus replace the court efficiency variables with: (i) the level of human capital (share of workers with at least secondary education); (ii) GDP per capita. Both variables are intended to capture the degree to which a given district is sensitive to growth shocks (Becker et al., 2015). The results are reported in table 10, columns (5) – (6). I do not find evidence that *TLR* increases to a larger extent in more developed districts or with a larger endowment of human capital, suggesting that court efficiency does not simply pick up heterogeneity in economic development.

[Table 10 here]

6. 7 Instrumental variable

A further threat to the validity of my design is that the results might be driven by omitted correlated variables that vary discontinuously at the judicial borders. To address this concern, I propose an instrumental variable strategy based on the simple idea that court congestion is a positive function of the number of the legal proceedings within a court (Priest, 1989). In particular, I propose an IV strategy based on the unexpected increase in court congestion in judicial districts along the Italian drug trafficking routes that was driven by the jump in the opium seizure rate (seizure rate, hereafter) after the War in Afghanistan. The opium production level reached the minimum point in 2000 after the Taliban enforced a ban on opium farming that decreased opium production by 99 percent. After the beginning of the war, cultivation was restored and the opium

import rate in Italy grew by 120 percent in the next three years (Ministry of Internal Affairs, 2005). Anecdotal evidence suggests that courts in districts along the drug transportation routes faced an increase in workload due to the jump in the seizure rate (Relazione sulla Sicurezza - DCSA, 2004).

The exclusion restriction relies on the idea that the increase in the seizure rate affects the contracting environment only through the effect on court congestion by way of the increase in the number of legal cases. While it is unlikely that an increase in the seizure rate directly affects that contracting environment, this change can however be correlated with unobserved heterogeneity in administrative and judicial efficiency across judicial districts which, for example, affect drug detection rates. One feature on the Italian institutional setting is helpful in that regard. Indeed, the trafficking routes go through the entire country, so that the instrument does not just capture cross-sectional heterogeneity in the pre-war level of administrative efficiency across regions.

A second concern is related to the relevance of the instrument. The increase in the seizure rate directly affects the congestion of the criminal section of a court. However, law scholars argue that the efficiency of a court is mostly an organizational issue (Barbato, 2015) such that the congestion of a section can have negative spillover effects on the other sections. In addition, in small courts, a judge is likely to follow cases in different disciplines such that the instrument could affect all the sections. Finally, the efficiency of a judge depends on the workload of the staff. If the increase in the seizure rate affects the work backlog of the staff, this will have side consequences on the work backlog of judges who have only civil / bankruptcy competences. I thus collect data on the seizure rate from the State Police website and use its variation between 2000 and 2004 to instrument court congestion at the beginning of 2005. I use as dependent variable the *TLR* coefficient (i.e., $CFO \times DCFO$) estimated for each court-year. Since seizure rate varies at court-level, I estimate the *TLR* coefficient considering the overall sample.

I first provide evidence on how the relevance of the instrument varies with court size and location. I thus estimate the first stage separately for quartiles of court size and then plot the first stage coefficients on seizure rate along with the relative *T*-statistics. Figure (5), panel A reports the magnitudes of the coefficients on seizure rate for each quartile in the bars, while the line plots the relative *T*-statistics. Figure (5), panel A shows that the strength of the instrument declines monotonically with court size. Next, I estimate the first stage separately for courts along the trafficking routes. Coherent with the underlying idea of the instrument, figure (5) panel B shows that the first stage coefficient on seizure rate is larger in courts along the trafficking routes.

Table 11 presents the IV results. Column (1) reports the OLS results in the same sample as the IV. The coefficient of interest, given by the interaction between *Post* × *Log(Court Efficiency),* is positive and significant. The magnitude and the precision of the estimates are not affected once I control for pre-court characteristics [column (2)]. In columns (3) – (5) I report the first stage and reduced form regressions. The coefficient on seizure rate is negative and highly significant (-0.006; *T*-stat: 5.03), implying that an increase in the seizure rate is associated with a decrease in court efficiency. The *F*-test for the excluded instrument is 14.29, suggesting that the instrument is not weak in comparison to standard critical values (Murray, 2006). Columns (6) – (7) report the IV results. The magnitudes on the treatment of interest, although smaller, are comparable to the OLS results, suggesting that the OLS coefficients are slightly upward biased, potentially driven by the underlying correlation between economic and credit market development and court efficiency.

[Figure 5, panels A and B here]

[Table 11 here]

7. Conclusion

I study whether a legal change that facilitates the renegotiation of debt contracts affects ex ante incentives to make financial statements more contractible. I exploit the adoption of a

bankruptcy law in Italy which made the renegotiation of debt contracts much easier in cases of distress and the ability of courts to enforce debt contracts as a source of cross-sectional variation. I find that firms in more efficient courts experience a greater increase in the contractibility of accounting information relative to firms in less efficient courts. Coherent with theory, I document that the change in accounting practices is accompanied by an increase in arm's length contracting.

There remain two threats to my results. First, the Law has likely increased incentives to strategically default (Hart and Moore, 1998; Rodano et al., 2014). The decrease in the credibility of the threat to liquidate a firm after a bad performance will likely rise the value of the option to default in expectation to negotiate better conditions. Lenders will anticipate this and price protect, demand more bargaining power, or try to realign ex ante incentives through more capital covenants. Borrowers will commit to not default by making the relative option less appealing. Given that the strategic default channel arises from the inability of the parties to commit to not renegotiate, covenants designed to allocate control rights are unlikely to address this friction, as their use will foster renegotiation rather than discouraging it. Thus, even if these changes in contractual terms can affect the properties of accounting numbers,[27] it is unclear how they will directly affect reporting practices. Second, the results could be driven by other contemporaneous reforms. However, any concurrent reform will affect my results only if it differentially affects firms in less and more efficient courts,[28] which is either implausible or ex-ante unclear.

[27] For example, one can argue that an increase in the number of lenders is a token to deter strategic default (Bolton and Scharfstein, 1996), which, indirectly, affects the properties of accounting information for debt contracting.

[28] IFRS reporting is still not allowed for SMEs, however, large private firms had the option to adopt IFRS since 2006. To address the concern that my results may be driven by a change in accounting standards, I restrict the sample to firms not switching to IFRS. However, only 4 percent of the firms in my sample report under IFRS, On the other hand, Basel II was enacted at the end of the 2007 (Bank of Italy, 2006). Finally, the role of courts in enforcing tax laws is minimal, and only in the case of serious fraud. Tax enforcement is administrated by executive agencies at the regional level (i.e., *Agenzia delle Entrate*), so that there is no geographic overlap between the jurisdiction of courts and the jurisdiction of the regional tax authority.

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Appendix A: Variable definitions

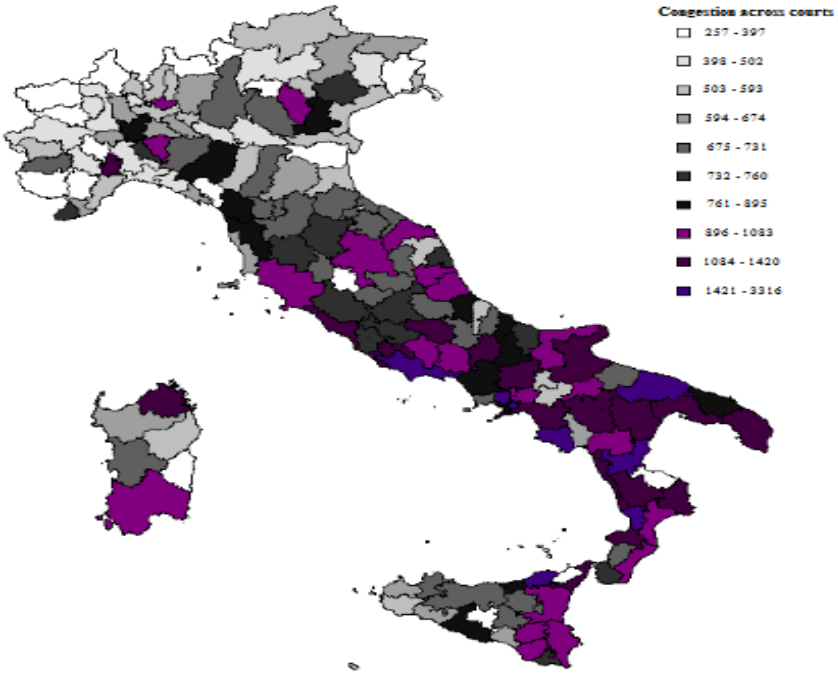
Variables used in calculating *TLR*	
Accruals / Total Assets	Accruals computed as $[(\Delta CA_{it} - \Delta Cash_{it}) - (\Delta CL_{it} - \Delta STD_{it}) - DEP_{it}]$ scaled by total assets for firm *i* at the beginning of year *t*, where ΔCA is the change in current assets, $\Delta Cash$ is the change in cash and bank balances, ΔCL is the change in current liabilities, ΔSTD is the change in short term debt, and *DEP* is depreciation expense.
CFO / Total Assets	Operating cash flows (scaled by total assets at the beginning of the year), computed as the difference between net income and Accruals, scaled by total assets at the beginning of the year.
DCFO	Dummy variable equal to one if *CFO / Total Assets* is negative, and zero otherwise.

Judicial district variables	
Court Efficiency	Negative of the average backlog per judge, computed as the negative of the logarithm of the number of pending cases in civil courts at the beginning of 2005 over the number of judges in that court.
Judges per court	Number of judges per court at the beginning of 2005.
Staff per court	Number of staff employed in a court at the beginning of 2005.
Length of civil cases	Duration of civil cases in days.
Length of bankruptcy procedures	Duration of bankruptcy procedures in days.
Recovery rate	Average recovery rate for secured creditors.
Number of reorganization cases	Number of reorganization procedures in a court.
Number of bankruptcy cases	Number of bankruptcy cases in a court.
Population	Population per judicial district.
Population density	Population density per judicial district.
Household income	Average annual household income per judicial district.
Unemployment	Unemployment rate per judicial district.
Agricultural share in GDP	Ratio between GDP from agricultural activities over GDP per judicial district.
Number of branches	Number of bank branches per judicial district.

Other variables

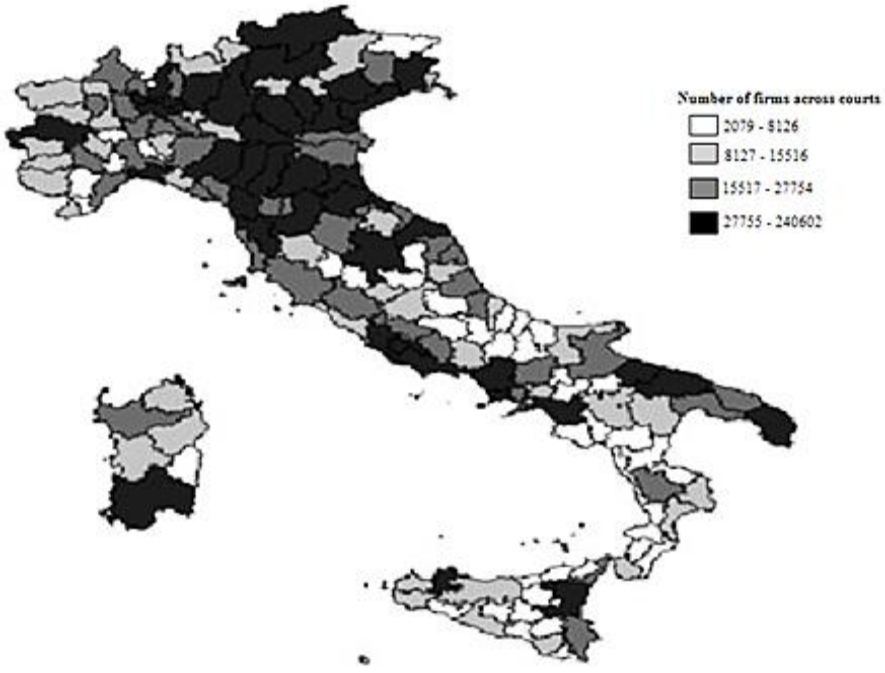
Sales	Total sales.
LEV	Ratio of total debt to total assets.
Growth	Percentage change in sales.
Write-offs / Gross Fixed Assets	Ratio of write-offs to gross fixed assets.
Bad Debts / Total Receivables	Ratio of bad debts to total receivables.
Extraordinary Expenses / Sales	Ratio of extraordinary expenses to sales.
Tangibility	Ratio of gross property, plant, and equipment to total assets.
Number of lenders	Number of banking relationships per firm in 2003.
Financially constrained	Dummy equal to one if a firm declares to be financially constrained in the year 2003, zero otherwise.
Financial dependence	Dummy equal to one if a firm belongs to an industry dependent industry according to the Rajan and Zingales (1998) industry classification, zero otherwise.
Bank fragility	Dummy equal to one if the ratio of banks' non-performing loans to total assets at judicial district for the year 2004 is above the sample median, zero otherwise.
Bank competition	Dummy equal to one if the number of branches over the number of firms in a judicial district for the year 2004 is above the sample median, zero otherwise.
Asset redeployability	Dummy equal to one if an industry belongs to a high asset redeployability industry (Kim and Kung, 2013), zero otherwise.
Loans	Corporate loans provided by big and small banks at judicial district-quarter level.
Default rates	Default rate at judicial district-quarter level.
Loan loss provisions	Loan loss provisions over gross loans at judicial district-quarter level.
Seizure rate	Change in the opium seizure rate between 2000 and 2004 at judicial district level.

Figure 1: Court efficiency across judicial districts



This figure displays the congestion of courts across judicial districts for the year 2005. Court congestion is the number of pending cases per judge. Data comes from the Italian Ministry of Justice.

Figure 2: Distribution of firms across judicial districts



This figure displays the number of firms by judicial district of incorporation for the year 2001. Data on firms' location comes from the Italian Agency of Statistics.

Figure 3: Example of identifying variation in the border-design

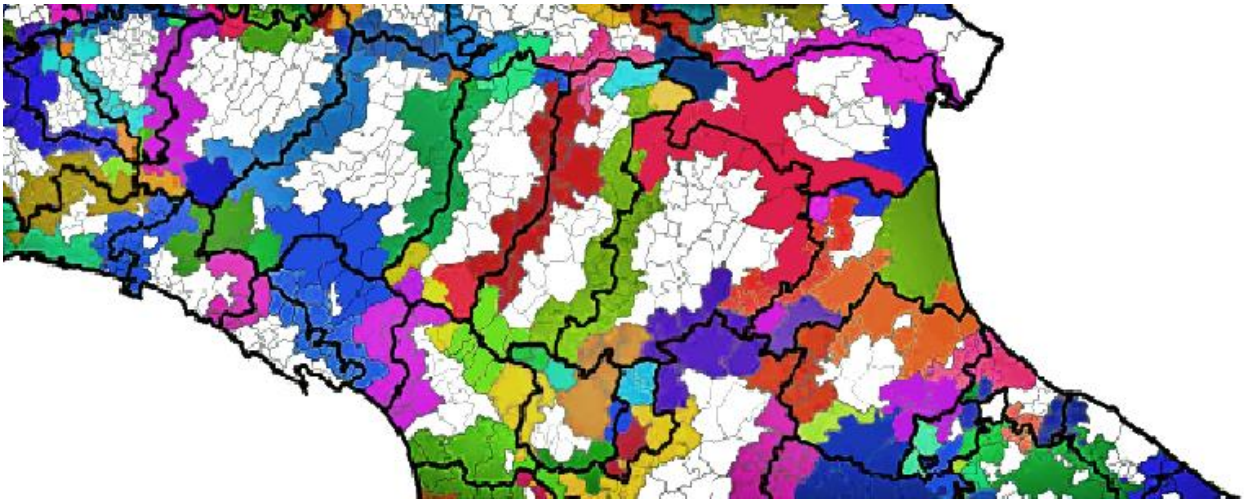


Figure 3 reports a sample of municipalities from the Emilia-Romagna region. The bold lines indicate judicial borders, while the thin lines indicate municipality borders. The colored municipalities represent the municipalities at the judicial districts' borders.

Figure 4: Trends in *TLR* across judicial districts

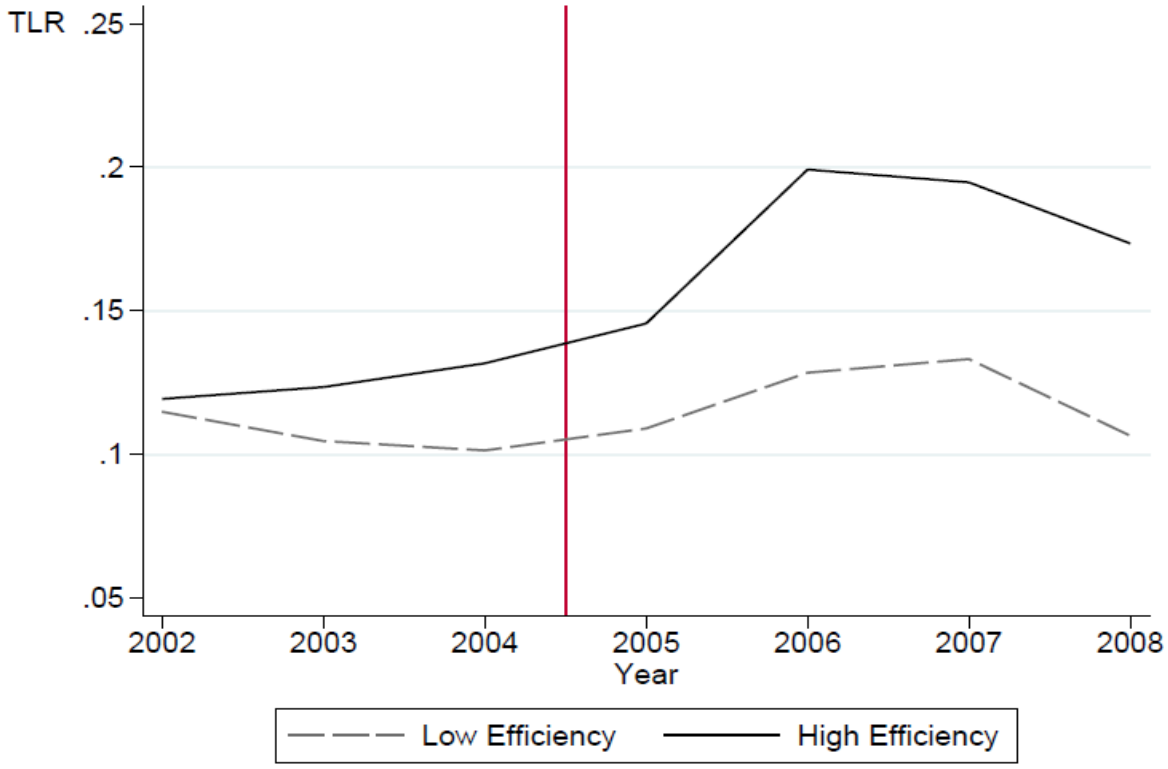


Figure 4 displays the trends in *TLR* over the sample period, separately for high and low congested courts. *TLR* coefficients (i.e., *CFO* × *DCFO*) are estimated for each border-court-year using only firms incorporated in municipalities at the borders of a judicial district. Then, I create two non-overlapping groups based on the within judicial border court congestion distribution, and assign a court-border-year *TLR* coefficient to the high court efficiency group if the coefficient is relative to the court with the higher level of efficiency within that border.

Table 1: Descriptive statistics

Panel A: Descriptive statistics on courts						
	N	Mean	p25	p50	p75	SD
Congestion	163	674	502	715	819	336
Judges per court	163	54	12	31	73	99
Staff per court	163	208	75	129	267	309
Length civil cases	163	800	684	769	884	238
Length of bankruptcy procedures	134	2,891	2,253	2,873	3,347	651
Recovery rate	134	0.295	0.126	0.291	0.393	0.165
Panel B: Descriptive statistics at court-level on economic and demographic characteristics						
Population / 1000	163	897.548	290.689	458.924	891.571	1,311.783
Population density	163	524	177	272	527	613
Household income	163	26.940	23.491	28.191	29.693	3.611
Unemployment	163	5.515	3.1	4.03	5.85	4.606
Agricultural share in GDP	163	0.022	0.005	0.018	0.029	0.019
Number of branches	163	650	209	409	761	697
Panel C: Descriptive statistics at firm-level						
CFO / Total Assets	242,471	0.052	-0.026	0.044	0.128	0.177
Accruals / Total Assets	242,471	-0.026	-0.092	-0.026	0.046	0.171
DCFO	242,471	0.354	0	0	1	0.468
Sales / 1000	242,471	6,729	2,380	4,638	7,068	11,728
Total Assets / 1000	242,471	7,198	2,224	3,581	7,404	13,077
LEV	242,471	0.681	0.544	0.725	0.851	0.212
Growth	242,471	0.121	-0.025	0.048	0.152	0.378
Write-offs / Gross Fixed Assets	97,939	0.031	0.000	0.003	0.011	1.616
Bad Debts / Total Receivables	97,939	0.008	0.003	0.005	0.006	0.039
Extraordinary Expenses / Sales	97,939	0.005	0.000	0.003	0.010	0.076

This table presents the summary statistics for the court-level variables (panel A and panel B), and firm-level variables (panel C) used in the analyses. See Appendix A for variable definitions.

Table 2: Descriptive statistics by court efficiency

	Low Efficiency			High Efficiency		
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	p50	SD	Mean	p50	SD
Court-level statistics						
Population / 1000	791,870	651,235	3,149	937.378	829,629	3,422
Population density	426.448	272.02	505.426	537.408	272.92	623.022
Household income (annual)	25.711	26.194	3.271	27.889	28.501	2.078
Unemployment	6.965	3.92	5.879	4.096	4.33	2.018
Agricultural share GDP	0.026	0.024	0.018	0.018	0.008	0.018
Number of branches	567	398	497	653	473	701
Judges per court	47	31	104	56	33	93
Staff per court	255	129	341	208	124	273
Length of civil cases	933	825	214	669	684	181
Length of bankruptcy procedures	2,986	2,999	641	2,474	2,268	664
Recovery rate	0.258	0.241	0.164	0.334	0.314	0.157
Number of reorganization cases per year	45	56	69	63	60	128
Number of bankruptcy cases per year	96	98	185	87	86	106
Number of observations	81	81	81	82	82	82
Firm-level statistics						
CFO / Total Assets	0.054	0.046	0.174	0.052	0.042	0.181
Accruals / Total Assets	-0.027	-0.026	0.166	-0.026	-0.025	0.176
Sales / 1000	7,311	3,835	13,557	6,140	3,455	11,799
Total Assets / 1000	7,521	3,667	14,920	6,821	3,449	13,742
LEV	0.668	0.712	0.215	0.693	0.739	0.207
Growth	0.109	0.045	0.357	0.133	0.053	0.401
Number of observations	121,235	121,235	121,235	121,236	121,236	121,236

This table presents summary statistics of the court-level (panel A) and firm-level (panel B) variables used in the analyses. High (low) efficient courts are those below (above) the median of the pending cases per judge at the beginning of 2005. See Appendix A for variable definitions.

Table 3: Effect of the Law on the contractibility of accounting information

Panel A: Average effect on TLR

	(1) Accruals	(2) Accruals	(3) Accruals	(4) Accruals	(5) Accruals	(6) *TLR*	(7) *TLR*
Post×CFO×DCFO×log(Court Efficiency)	**0.011*** **	**0.012*** **	**0.009*** **	**0.008*** **	**0.009*** **		
	[0.002]	**[0.003]**	**[0.003]**	**[0.003]**	**[0.003]**		
Post×CFO×log(Court Efficiency)	-0.001	-0.001	-0.001	-0.001	-0.002		
	[0.002]	[0.002]	[0.001]	[0.001]	[0.001]		
Post×DCFO×log(Court Efficiency)	-0.000*	-0.000	-0.001***	-0.001***	-0.001***		
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]		
Post×log(Court Efficiency)	0.003***	0.004***	0.005***	0.005***	0.005***	**0.060****	**0.061****
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	**[0.025]**	**[0.029]**
CFO	-0.910***	-0.956***	-0.873***	-0.996***	-1.098***		
	[0.018]	[0.017]	[0.018]	[0.017]	[0.064]		
DCFO	0.008***	0.010***	0.016***	-0.005**	0.004		
	[0.002]	[0.003]	[0.002]	[0.003]	[0.009]		
DCFO×CFO	-0.077***	-0.018	-0.076**	-0.007	-0.014		
	[0.023]	[0.025]	[0.034]	[0.031]	[0.125]		
Post×Sales			-0.002***	-0.003***	-0.002***		
			[0.000]	[0.000]	[0.000]		
Post×LEV			0.000***	0.000***	0.000***		
			[0.000]	[0.000]	[0.000]		
Post×Growth			-0.008***	-0.009***	-0.012***		
			[0.001]	[0.001]	[0.001]		
Post×log(Population)			0.000	0.000	0.000		-0.001
			[0.000]	[0.000]	[0.000]		[0.002]
Post×log(Population density)			-0.001	-0.001	-0.001		-0.001
			[0.001]	[0.001]	[0.001]		[0.001]
Post×log(Household income)			0.001	0.001*	0.001		0.000
			[0.001]	[0.001]	[0.001]		[0.003]
Post×log(Unemployment)			-0.001	-0.000	-0.000		-0.007
			[0.000]	[0.001]	[0.001]		[0.005]
Post×log(Agricultural share GDP)			-0.001	-0.001	-0.001		-0.004
			[0.001]	[0.001]	[0.001]		[0.003]
Post×log(Number branches)			0.000	-0.000*	-0.000		0.002
			[0.000]	[0.000]	[0.000]		[0.001]

Sample	Overall	Border	Overall	Border	Border No change location	Border Municipality level	Border Municipality level
Firm Fe	Yes	Yes	Yes	Yes	Yes	No	No
Year Fe	Yes	No	Yes	No	No	No	No
Year Fe × (CFO, DCFO, CFO×DCFO)	Yes	Yes	Yes	Yes	Yes	No	No
Border-Year Fe	No	Yes	No	Yes	Yes	Yes	Yes
Court Fe	No	No	No	No	No	Yes	Yes
Court Fe × (CFO, DCFO, CFO×DCFO)	Yes	Yes	Yes	Yes	Yes	No	No
Controls	No	No	Yes	Yes	Yes	No	Yes
Controls × (CFO, DCFO, CFO×DCFO)	No	No	Yes	Yes	Yes	No	No
Number of clusters	163	163	163	163	163	163	163
Observations	242,471	119,870	242,471	119,870	112,346	14,897	14,897
R-squared	0.827	0.828	0.834	0.843	0.846	0.806	0.839

This table reports OLS estimates from equation (2). Coefficients of interested are in bold. Columns (1) and (3) report the results for the overall sample, while columns (2), (4), and (5) report the results for the border sample only. In column (5) I restrict the sample to firms which do not change their location over the sample period. In columns (6) – (7), I report OLS estimates from the estimation of equation (2) at municipality-year level. The dependent variable is the *TLR* coefficient (i.e., $CFO \times DCFO$) estimated for each municipality-year. See Appendix A for variable definitions. Reported below the coefficients are standard errors clustered at court-level.

*, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Panel B: Average effect on other negative accruals

	(1) Write-offs / Gross Fixed Assets	(2) Bad Debts / Total Receivables	(3) Extraordinary Expenses / Sales	(4) Write-offs / Gross Fixed Assets	(5) Bad Debts / Total Receivables	(6) Extraordinary Expenses / Sales
Post×log(Court Efficiency)	**0.005*** **	**0.006*** **	**0.002*** **	**0.005** **	**0.004** **	**0.002** **
	[0.001]	**[0.002]**	**[0.000]**	**[0.002]**	**[0.002]**	**[0.001]**
Post×Sales				-0.002***	-0.003***	-0.002***
				[0.000]	[0.000]	[0.000]
Post×Lev				0.000***	0.000***	0.000***
				[0.000]	[0.000]	[0.000]
Post×Growth				-0.008***	-0.009***	-0.013***
				[0.001]	[0.001]	[0.002]
Post×log(Population)				0.000	0.000	0.000
				[0.000]	[0.001]	[0.000]
Post×log(Population density)				-0.002	0.001	0.001
				[0.001]	[0.002]	[0.002]
Post×log(Household income)				0.001	0.001	0.001
				[0.001]	[0.002]	[0.002]
Post×log(Unemployment)				0.001	0.002	0.003*
				[0.001]	[0.002]	[0.001]
Post×log(Agricultural share in GDP)				-0.001	0.001	0.000
				[0.001]	[0.002]	[0.002]
Post×log(Number branches)				-0.001	-0.002*	-0.001
				[0.001]	[0.001]	[0.001]
Sample	Border	Border	Border	Border	Border	Border
Firm Fe	Yes	Yes	Yes	Yes	Yes	Yes
Border-Year Fe	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	163	163	163	163	163	163
Observations	97,939	97,939	97,939	97,939	97,939	97,939
R-squared	0.458	0.439	0.452	0.458	0.439	0.452

This table reports OLS estimates from equation (2). The dependent variables are reported at the top of the respective columns. Coefficients of interested are in bold. All the models are estimated by considering the border sample. See Appendix A for variable definitions. Reported below the coefficients are standard errors clustered at court-level.

*, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively

Table 4: Effect of the Law on the contractibility of accounting information by asset tangibility

	$Tangibility_{q=1}$	$Tangibility_{q=2}$	$Tangibility_{q=3}$	$Tangibility_{q=4}$
	(1)	(2)	(3)	(4)
	Accruals	Accruals	Accruals	Accruals
Post×CFO×DCFO×log(Court Efficiency)	**0.006***	**0.012*****	**0.016*****	**-0.005**
	[0.003]	**[0.003]**	**[0.004]**	**[0.004]**
Post×CFO×log(Court Efficiency)	-0.003*	-0.004**	-0.007**	-0.003
	[0.002]	[0.002]	[0.003]	[0.003]
Post×DCFO×log(Court Efficiency)	-0.001***	-0.001	-0.001**	0.000
	[0.001]	[0.001]	[0.001]	[0.001]
Post×log(Court Efficiency)	0.004*	0.005***	0.004***	0.005***
	[0.002]	[0.001]	[0.001]	[0.001]
CFO	-1.292***	-0.891***	-1.099***	-1.099***
	[0.337]	[0.232]	[0.295]	[0.316]
DCFO	-0.015	0.036	0.018	-0.025
	[0.048]	[0.031]	[0.033]	[0.034]
DCFO×CFO	0.029	0.097	-0.300	-0.249
	[0.332]	[0.331]	[0.367]	[0.349]
Sample	Border	Border	Border	Border
Firm Fe	Yes	Yes	Yes	Yes
Year Fe	No	No	No	No
Year Fe × (CFO, DCFO, CFO × DCFO)	Yes	Yes	Yes	Yes
Border-Year Fe	Yes	Yes	Yes	Yes
Court Fe	No	No	No	No
Court Fe × (CFO, DCFO, CFO × DCFO)	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × (CFO, DCFO, CFO × DCFO)	Yes	Yes	Yes	Yes
Number of clusters	163	163	163	163
Observations	27,432	33,655	29,518	29,051
R-squared	0.845	0.844	0.842	0.817

This table reports OLS estimates from equation (2). Coefficients of interested are in bold. Column (1) reports the results for firms belonging to the first quartile of the tangibility distribution. Column (2) reports the results for firms belonging to the second quartile of the tangibility distribution. Column (3) reports the results for firms belonging to the third quartile of the tangibility distribution. Column (4) reports the results for firms belonging to the fourth quartile of the tangibility distribution. Tangibility is measured at 2004 as the ratio between property, plant, and equipment, and total assets. All the models are estimated by considering the border sample. See Appendix A for variable definitions. Reported below the coefficients are standard errors clustered at court-level.
*, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Table 5: Effect of the Law on the contractibility of accounting information by number of lenders, bank fragility, and financial constraints

	Number of lenders			Bank fragility		Financially constrained	
	[1]	(1,3]	>3	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Accruals	Accruals	Accruals	Accruals	Accruals	Accruals	Accruals
Post×CFO×DCFO×log(Court Efficiency)	**0.001**	**0.007****	**0.003**	**0.004**	**0.012*****	**0.005**	**0.009****
	[0.002]	**[0.003]**	**[0.005]**	**[0.004]**	**[0.003]**	**[0.004]**	**[0.004]**
Post×CFO×log(Court Efficiency)	-0.001	-0.002	-0.002	-0.005	-0.003	-0.003	0.003
	[0.009]	[0.009]	[0.005]	[0.003]	[0.002]	[0.007]	[0.010]
Post×DCFO×log(Court Efficiency)	-0.000	-0.001	0.001	-0.001	-0.000	0.001	0.001
	[0.001]	[0.001]	[0.001]	[0.000]	[0.000]	[0.000]	[0.002]
Post×log(Court Efficiency)	0.004*	0.001	0.003	0.007**	0.002	0.003	0.004**
	[-0.003]	[0.002]	[0.002]	[0.003]	[0.002]	[0.004]	[0.002]
CFO	-0.447***	-0.493***	-0.775***	-0.974***	0.737***	-0.495***	-1.331***
	[0.093]	[0.088]	[0.062]	[0.020]	[0.079]	[0.066]	[0.190]
DCFO	0.047	0.071	0.016**	0.010***	-0.001	0.085***	-0.087***
	[0.059]	[0.051]	[0.008]	[0.004]	[0.003]	[0.029]	[0.023]
DCFO×CFO	0.090	0.340	0.000	0.009	-0.180***	0.870	-0.376
	[0.948]	[1.363]	[0.104]	[0.031]	[0.039]	[0.752]	[0.229]
Sample	Overall	Overall	Overall	Overall	Overall	Overall	Overall
Firm Fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fe × (CFO, DCFO, CFO × DCFO)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Courts × (CFO, DCFO, CFO × DCFO)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × (CFO, DCFO, CFO × DCFO)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	90	153	146	76	78	137	109
Observations	3,765	9,765	9,630	99,055	120,698	18,043	4,104
R-squared	0.629	0.812	0.839	0.855	0.841	0.812	0.886

This table reports OLS estimates from equation (2). Coefficients of interested are in bold. Columns (1) – (3) break down the sample by the number of banks a firm borrows from in 2003. Columns (4) – (5) break down the sample by whether a firm is incorporated in a judicial district in which banks are prone to liquidity shocks. Columns (6) – (7) break down the sample by whether a firm declares to be financially constrained in 2003. All the models are estimated by considering the overall sample. See Appendix A for variable definitions. Reported below the coefficients are standard errors clustered at court-level.
*, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Table 6: Effect of the Law on the contractibility of accounting information by industry and courts' characteristics

	Financial dependence		Bank competition		Asset redeployability	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
	Accruals	Accruals	Accruals	Accruals	Accruals	Accruals
Post×CFO×DCFO×log(Court Efficiency)	**-0.003**	**0.008*** **	**0.008*** **	**0.011** **	**0.011** **	**0.009** **
	[0.004]	**[0.003]**	**[0.003]**	**[0.004]**	**[0.005]**	**[0.004]**
Post×CFO×log(Court Efficiency)	-0.003	-0.006**	-0.003	-0.005*	-0.005	-0.003
	[0.003]	[0.003]	[0.003]	[0.003]	[0.004]	[0.003]
Post×DCFO×log(Court Efficiency)	-0.001	-0.002***	-0.001***	-0.002**	-0.001	-0.002***
	[0.001]	[0.002]	[0.000]	[0.0001]	[0.001]	[0.001]
Post×log(Court Efficiency)	0.003*	0.005*	0.004**	0.005**	0.002	0.005**
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
CFO	-0.888***	-0.992***	-0.992***	-1.235***	-0.934***	-1.026***
	[0.022]	[0.025]	[0.025]	[0.091]	[0.047]	[0.021]
DCFO	-0.001	-0.004	-0.004	0.003	0.016***	-0.046***
	[0.004]	[0.004]	[0.003]	[0.004]	[0.005]	[0.004]
DCFO×CFO	-0.157***	-0.037	-0.0372	-0.155***	0.039	-0.079**
	[0.041]	[0.026]	[0.026]	[0.039]	[0.081]	[0.035]
Sample	Border	Border	All	All	Border	Border
Firm Fe	Yes	Yes	Yes	Yes	Yes	Yes
Year Fe	No	No	Yes	Yes	No	No
Year Fe × (CFO, DCFO, CFO×DCFO)	Yes	Yes	Yes	Yes	Yes	Yes
Border-Year Fe	Yes	Yes	No	No	Yes	Yes
Court Fe × (CFO, DCFO, CFO×DCFO)	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Controls × (CFO, DCFO, CFO×DCFO)	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	134	148	74	75	134	135
Observations	46,220	44,571	100,571	141,299	44,368	40,428
R-squared	0.834	0.853	0.853	0.834	0.827	0.832

This table reports OLS results from the estimation of equation (2). Columns (1) – (2) break down the sample by whether a firm belongs to an industry with low (high) external financial dependence (Rajan and Zingales [1998]). Columns (3) – (4) break down the sample by whether a firm is incorporated in a judicial district with low (high) bank competition. Columns (5) – (6) break down the sample by whether a firm belongs to an industry where assets are less (more) redeployable. See Appendix A for variable definitions. Coefficients of interest are in bold. Reported below the coefficients are standard errors clustered at court-level.

*, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels.

Table 7: Change in debt and the contractibility of accounting information

	$\Delta Debt_{2004/06t=1}$	$\Delta Debt_{2004/06t=2}$	$\Delta Debt_{2004/06t=3}$	$\Delta Debt_{2002/04t=3}$	$\Delta Debt_{2002/04t=3}$
	(1)	(2)	(3)	(4)	(5)
	Accruals	Accruals	Accruals	Accruals	Accruals
Post×CFO×DCFO×log(Court Efficiency)	**0.004**	**0.009*** **	**0.011** **		
	[0.005]	**[0.002]**	**[0.005]**		
Post×CFO×log(Court Efficiency)	-0.006**	-0.003	-0.005*		
	[0.003]	[0.002]	[0.003]		
Post×DCFO×log(Court Efficiency)	-0.001***	-0.001**	-0.002***		
	[0.000]	[0.000]	[0.001]		
$Post_{2003}$×CFO×DCFO×log(Court Efficiency)				**0.004**	
				[0.003]	
$Post_{2003}$×CFO×log(Court Efficiency)				-0.002	
				[0.003]	
$Post_{2003}$×DCFO×log(Court Efficiency)				0.000	
				[0.001]	
$Post_{2003}$×CFO×DCFO					**0.007*** **
					[0.002]
$Post_{2003}$×CFO					-0.003
					[0.003]
$Post_{2003}$×DCFO					-0.001***
					[0.000]
Sample	Border	Border	Border	Border	Border
Firm Fe	Yes	Yes	Yes	Yes	Yes
Year Fe × (CFO, DCFO, CFO×DCFO)	Yes	Yes	Yes	Yes	Yes
Border-Year Fe	Yes	Yes	Yes	Yes	Yes
Court Fe × (CFO, DCFO, CFO×DCFO)	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Controls × (CFO, DCFO, CFO×DCFO)	Yes	Yes	Yes	Yes	Yes
Number of clusters	157	157	162	162	162
Observations	40,089	40,073	38,059	33,262	33,262

The table reports estimates from equation (2) for terciles of the delta debt between 2004 and 2006 [columns (1) – (3)]. In columns (4) – (5) I counterfactually anticipate the treatment to 2003, and estimate equation (2) for terciles of the delta debt between 2002 and 2004. In columns (1) – (5), I do not report the coefficients on *$Post_t$×log(Court Efficiency)*, *CFO*, *DCFO*, and *CFO×DCFO* for sake of brevity. See Appendix A for variable definitions.
Standard errors are clustered at court level.*, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels.

Table 8: Lending behavior and the structure of the banking industry

	(1) Loans large banks / total loans	(2) Loans small banks / total loans	(3) Loans large banks / pre-reform average total loans	(4) Loans small banks / pre-reform average total loans	(5) Branches large banks / total branches	(6) Branches small banks / total branches	(7) Branches large banks / pre-reform average # branches	(8) Branches small banks / pre-reform average # branches
Post×log(Court Efficiency)	**0.033*** **	**-0.001**	**0.036*** **	**-0.004**	**0.001**	**0.002**	**0.003**	**0.003**
	[0.012]	**[0.125]**	**[0.009]**	**[0.134]**	**[0.009]**	**[0.003]**	**[0.014]**	**[0.003]**
Post×log(Population)	0.000**	0.000	0.000**	0.000	-0.011	0.010	-0.011	0.010
	[0.000]	[0.000]	[0.000]	[0.000]	[0.021]	[0.020]	[0.021]	[0.020]
Post×log(Population density)	0.000***	0.000**	0.000***	0.000**	-0.077	0.027	-0.077	0.027
	[0.000]	[0.000]	[0.000]	[0.000]	[0.064]	[0.024]	[0.064]	[0.024]
Post×log(Household income)	-0.000**	-0.000	-0.000**	-0.000	-0.131*	0.019	-0.131*	0.019
	[0.000]	[0.000]	[0.000]	[0.000]	[0.069]	[0.042]	[0.069]	[0.042]
Post×log(Unemployment)	0.000	-0.000	0.000	-0.000	-0.004	0.005	-0.004	0.005
	[0.000]	[0.000]	[0.000]	[0.000]	[0.039]	[0.025]	[0.039]	[0.025]
Post×log(Agricultural share GDP)	-0.000	-0.000	-0.000	-0.000	-0.006	0.002	-0.006	0.002
	[0.000]	[0.000]	[0.000]	[0.000]	[0.051]	[0.025]	[0.051]	[0.025]
Post×log(Number branches)	0.000**	0.000*	0.000**	0.000*				
	[0.000]	[0.000]	[0.000]	[0.000]				
Court Fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter Fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	135	135	135	135	135	135	135	135
Observations	3,710	3,710	3,710	3,710	3,710	3,710	3,710	3,710
R-squared	0.802	0.825	0.804	0.812	0.978	0.980	0.980	0.986

This table reports OLS estimates from court-quarter regressions of *Ys* on *Post × log(Court Efficiency)* and control variables. Dependent variables are indicated at the top of each column. See Appendix A for variable definitions. Reported below the coefficients are standard errors clustered at court-level.
*, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Table 9: Lenders' risk taking

	(1) Default Rates	(2) Default Rates	(3) Loan Loss Provisions	(4) Loan Loss Provisions
Post*Corporate*	**-0.002**		**0.001**	
	[0.043]		**[0.014]**	
Post×log(Court Efficiency)		**0.090**		**0.000**
		[0.073]		**[0.010]**
Post×log(Population)		0.027		0.009
		[0.042]		[0.012]
Post×log(Population density)		-0.031		-0.021
		[0.057]		[0.013]
Post×log(Household income)		0.058		0.013
		[0.107]		[0.015]
Post×log(Unemployment)		0.075		-0.000
		[0.094]		[0.013]
Post×log(Agricultural share GDP)		0.005		0.001
		[0.048]		[0.008]
Post×log(Number branches)		0.013		-0.007
		[0.116]		[0.015]
Type of loans	Corporate & Household	Corporate	Corporate & Household	Corporate
Year-Quarter Fe	No	Yes	No	Yes
Year-Quarter-Court Fe	Yes	No	Yes	No
Court Fe	No	Yes	No	Yes
Loan Type Fe	Yes	No	Yes	No
Number of clusters	135	135	135	135
Observations	5,407	2,631	5,407	2,631
R-squared	0.156	0.084	0.122	0.222

Table 9, columns (1) and (3) report OLS estimates from court-quarter-loan type regressions of default rates (loan loss provisions) on *Post*$_{corporate}$. *Post*$_{corporate}$ is a dummy variable equal to one for observations for quarters after March 2005 and corporate loans. Household loans are used as counterfactual. Columns (2) and (4) report the results from the estimation of court-quarter regressions of default rates (loan loss provisions) on *Post* × *log(Court Efficiency*) using corporate loans only. Dependent variables are indicated at the top of each column. See Appendix A for variable definitions. Reported below the coefficients are standard errors clustered at court-level.
*, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

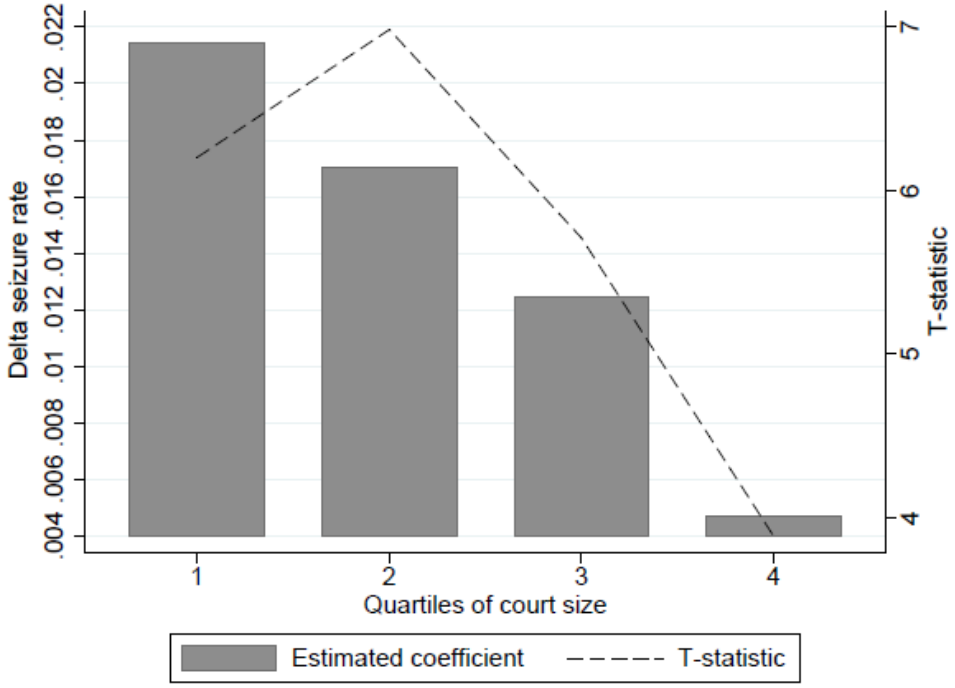
Table 10: Dynamic of the treatment and falsification

	(1) Accruals	(2) Accruals	(3) Accruals	(4) Accruals	(5) Accruals	(6) Accruals
$Post_{t-2}$×CFO×DCFO×log(Court Efficiency)	0.000					
	[0.003]					
$Post_{t-1}$×CFO×DCFO×log(Court Efficiency)		0.002				
		[0.006]				
$Post_{t+1}$×CFO×DCFO×log(Court Efficiency)			0.007**			
			[0.003]			
$Post_{t+2}$×CFO×DCFO×log(Court Efficiency)				0.005*		
				[0.003]		
Post×CFO×DCFO×log(Human Capital)					0.001	
					[0.014]	
Post×CFO×DCFO×log(GDP per capita)						0.003
						[0.007]
Sample	Border	Border	Border	Border	Overall	Overall
Firm Fe	Yes	Yes	Yes	Yes	Yes	Yes
Year Fe	No	No	No	No	Yes	Yes
Year Fe × (CFO, DCFO, CFO×DCFO)	Yes	Yes	Yes	Yes	Yes	Yes
Border-Year Fe	Yes	Yes	Yes	Yes	No	No
Court Fe × (CFO, DCFO, CFO×DCFO)	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	163	163	163	163	132	132
Observations	119,870	119,870	119,870	119,870	242,471	242,471

This table reports the results from the estimation of equation (2) in which the year of the treatment is counterfactually anticipated (postponed) by one and two years. In columns (5) – (6) I replace the court efficiency variable with the following placebo variables: (i) the level of human capital at court-level (share of workers with at least secondary education); (ii) GDP per capital at court-level. For sake of brevity, I report only the coefficients of interest even if the models are estimated including all the interaction terms. See Appendix A for variable definitions. Reported below the coefficients are standard errors clustered at court-level.
*, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

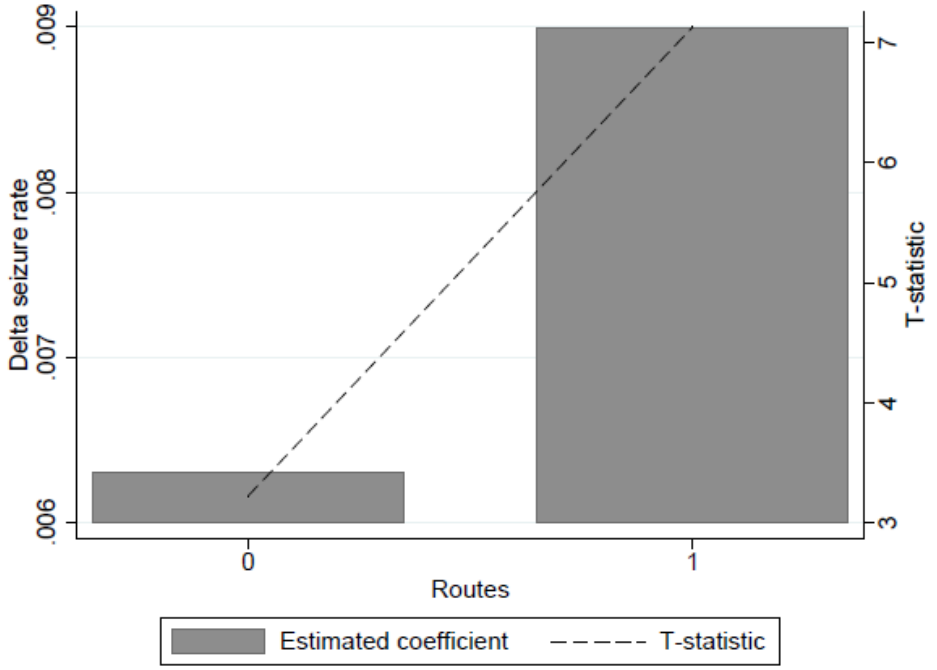
Figure 5: First stage by court size and trafficking routes

Panel A: First stage by court size



This figure plots the first stage coefficients on seizure rate (bars) and the relative *T*-statistics (dashed line) for each quartile of court size.

Panel B: First stage by trafficking routes



This figure plots the first stage coefficients on seizure rate (bars) and the relative *T*-statistics (dashed line) for courts along and not the trafficking routes.

Table 11: Instrumental variable estimator

Dependent variable in the second stage: Ball and Shivakumar *TLR* coefficient	(1) OLS	(2) OLS	(3) First Stage	(4) Reduced Form	(5) Reduced Form	(6) IV	(7) IV
Post×log(Court Efficiency)	**0.060****	**0.061****				**0.052****	**0.056***
	[0.024]	**[0.024]**				**[0.027]**	**[0.030]**
Post×delta(seizure rate)			-0.006***	-0.001**	-0.001**		
			[0.001]	[0.000]	[0.000]		
Post×log(Population)		-0.004			0.000		-0.003
		[0.004]			[0.002]		[0.004]
Post×log(Population density)		0.006			0.001		0.005
		[0.005]			[0.009]		[0.006]
Post×log(Household income)		-0.004			0.000		0.002
		[0.005]			[0.007]		[0.005]
Post×log(Unemployment)		-0.007			-0.005		-0.002
		[0.005]			[0.007]		[0.005]
Post×log(Agricultural share GDP)		0.000			0.001		0.001
		[0.003]			[0.005]		[0.005]
Post×log(Number branches)		0.001			-0.002		0.001
		[0.002]			[0.004]		[0.002]
Sample	Overall	Overall	Overall	Overall	Overall	Overall	Overall
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court Fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	134	134	134	134	134	134	134
Observations	938	938	938	938	938	938	938
R-squared	0.217	0.221	0.943	0.209	0.211	0.208	0.212

This table reports the results from OLS, reduced form, and IV regressions of the *TLR* coefficient on *Post × log(Court Efficiency)* and control variables. The dependent variable is the *TLR* coefficient from the Ball and Shivakumar model estimated for each court-year in columns (1), (2), (4), (5), (6), and (7). The dependent variable is *log(Court Efficiency)* interacted with the *Post* dummy in column (3). See Appendix A for variable definitions. Reported below the coefficients are standard errors clustered at court-level.

*, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.