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September 2022

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Abstract

We study the effects of a court decision granting creditors the power to force into bankruptcy corporate debtors whose liabilities exceed their assets even if they are current on their payments. We find that bond (stock) prices responded positively (negatively) to the court ruling and that firms affected by it did not reduce their risk, but increased their net worth through equity injections and aggressive accounting. As a result, the informativeness of these firms' financial reports decreased. We conclude that the benefits from some measures of creditor empowerment may be mitigated by borrowers' incentives to present overly-optimistic financial reports.

Keywords: Creditor Rights, Financial Distress, Bankruptcy, Aggressive Accounting, Informativeness

JEL Classifications: G30, G33, K22, M41

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We study the effects of a court decision granting creditors the power to force into bankruptcy corporate debtors whose liabilities exceed their assets even if they are current on their payments. We find that bond (stock) prices responded positively (negatively) to the court ruling and that firms affected by it did not reduce their risk, but increased their net worth through equity injections and aggressive accounting. As a result, the informativeness of these firms' financial reports decreased. We conclude that the benefits from some measures of creditor empowerment may be mitigated by borrowers' incentives to present overly-optimistic financial reports.

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Introduction

Creditor rights are essential for the development of private credit markets (e.g., Djankov, McLiesh and Shleifer, 2007), but may produce adverse effects as well. Much of the literature evaluating the effects of creditor rights focuses on states of the world in which creditors have direct control rights, typically in bankruptcy (e.g., Vig, 2013; Schoenherr and Starmass, 2021) or as a result of covenant violations. Some studies focus on directors' fiduciary duties in the vicinity of insolvency, requiring them to take creditors' interests into account (Becker and Stromberg, 2012). The present study takes a different approach, focusing on another measure of creditor empowerment: allowing creditors to force borrowers into bankruptcy (involuntary bankruptcy). Specifically, we examine a 2013 court ruling in Israel that granted creditors the power to force into bankruptcy (liquidation or reorganization) firms whose liabilities exceeded their assets even if these firms were current on their payments. This surprising ruling provides us with an opportunity to evaluate the effects of expanding creditors' control rights on firm and investor behavior in a relatively clean empirical setting.

As a matter of theory, empowering creditors to force corporate debtors into bankruptcy can produce several effects. First, it may address management incentives to delay bankruptcy. Unlike the debtor-in-possession regime prevalent in the United States, the bankruptcy regime we study generally requires that the control of companies in bankruptcy be taken from management. This threat of losing control discourages management (and controlling shareholders) from initiating bankruptcy (Hynes and Walt, 2020; Schoenherr and Starmass, 2021). Moreover, most companies in our sample have controlling shareholders. Under the absolute priority rule, filing for bankruptcy could lead these controlling shareholders to lose the option value of their equity. Allowing creditors to force firms into bankruptcy could prevent the costs produced by management's incentives to delay the onset of the process. Empowering creditors, however, might lead creditors with a liquidation bias or opportunistic creditor to file too early, thereby

undermining the firm's ability to overcome its difficulties without undergoing a costly bankruptcy process.

Second, the threat of creditor control could improve the incentives of management (and controlling shareholders) of distressed borrowers and lead them to change firm conduct. Management, for example, might refrain from asset-striping or other forms of opportunistic behavior. Controlling shareholders may provide the firm with fresh capital or take other steps that would raise company value and keep the debtor away from financial distress. At the same time, and depending on the legal test for insolvency, the prospect of creditor control could induce firms to use aggressive accounting methods in order to appear solvent and avoid losing control to creditors.

Given the uncertainty over the likely effect of this change in creditor power, our first objective is to determine whether the new rule was overall beneficial to creditors (or shareholders). We attempt to answer this question by focusing on market reaction to the decision. Our findings suggest that the legal change was perceived as a surprise and regarded as beneficial to creditors, possibly at the expense of shareholders: On the date of the court ruling, bonds issued by firms close to default and affected by the court ruling (as defined below) exhibited positive excess returns (relative to a portfolio of similar bonds), whereas the excess returns of the affected firms' stocks was negative.

Our next objective is to identify specific channels through which creditors' new power affected creditors and borrowers. Following the literature, we start by focusing on risk levels: On the one hand, if creditors are given more control, one might expect companies to reduce risk both prior to, and during, bankruptcy (e.g., Acharya, Amihud, and Litov, 2011; Becker and Stromberg, 2012; Schoenherr and Starmass, 2021). On the other hand, we study a rule that provides creditors with the *right* to initiate bankruptcy. As long as creditors do not file for bankruptcy, borrowers continue to be controlled by management. Thus, distressed companies which face the threat of creditor control might take on more

risks in a gamble to avoid default. In practice, we do not observe any change in the available measures of corporate risk (profit volatility and the volatility of stock returns). We believe that this non-result is driven by two factors: First, the court decision was a surprise and took immediate effect. This left borrowers without the opportunity to change their operations, or adjust their riskiness, in the short run. Second, unlike the setting of Becker and Stromberg (2012) or Vig (2013), firms in our sample became subject to a *threat* of creditor control (if their liabilities exceeded their assets), but creditors could not impose any decisions on them prior to the (uncertain) realization of this threat. In other words, the court did not grant creditors the power to impose low risk policies on borrowing firms.

Providing creditors with more power to initiate bankruptcy should induce managers to take measures that would reduce the threat of creditor control. In the third set of empirical results we show that, in comparison with a control group, there was a pronounced increase in the (reported) net worth of firms affected by the court ruling following the 2013 decision. The firms most likely to be affected by the legal change were especially likely to experience an increase in their net worth around 2013. Similarly, the fraction of affected companies with negative net worth declined by about 50%.

What could explain this surge in net worth? We observe an increase in equity issuances among some distressed firms affected by the new rule, primarily in the form of capital injections (private placements and warrants) by existing shareholders. Firms in the lowest decile of the net worth distribution prior to the court decision were especially likely to raise new equity following the ruling. This finding is consistent with the positive expected effect of the new rule on creditors. It is also consistent with the fact that many firms in our sample have controlling shareholders, who might be willing to make equity investments to ensure their continued control.

Yet equity issuances were not the only reason for the dramatic increase in net worth among firms affected by the ruling in the post-court ruling period. Instead, in the next set of empirical tests, we document a consistent relation between the court decision and changes in firms' reporting practices: low net worth borrowers affected by the court ruling responded to the increased risk of creditor control by making changes to the way their net worth was reported, exploiting discretion in accounting practices to increase long-term discretionary accruals and reduce accounting conservatism. By changing their accounting practices, firms affected by the court decision could report above-zero net worth and thus reduce the risk of involuntary bankruptcy and creditor control. The effects of these accounting changes appear to have been transitory — the (presumably inflated) earnings of treated firms declined a few years after the court ruling (in the spirit of studies like Barton and Simko, 2002, or Hirshleifer et al, 2004).

To the extent that it induced distressed firms to inflate reported earnings, the legal change underlying our study might have affected the informativeness of financial statements. In line with this conjecture, in our final set of results, we find that, in the post court-ruling period, bondholders seem to have attributed less informational content to the treated firms' reported earnings. This might have had an adverse effect on the functioning of public debt markets.

The 2013 legal development that took place in Israel is a perfect setup for our empirical analysis: bondholders of one of the largest holding companies asked the court to force the company into bankruptcy even though it was current on its payments and did not breach any bond covenant. Nevertheless, the bondholders argued that the company was insolvent in the sense that its liabilities exceeded its assets and should therefore be forced into bankruptcy.[1] In a precedential decision, the court accepted this argument and held that bondholders could indeed force into bankruptcy companies in such cases of "balance

[1] The bondholders did not ask for liquidation. Rather, they asked the court to order a court-supervised reorganization and approve a plan under which they will exchange their debt for equity.

sheet insolvency", even in absence of default or a breach of covenant. An exogenous legal change to bankruptcy law, this decision did not change the nature of creditors' control within bankruptcy. Rather, it expanded the conditions under which creditors of distressed borrowers could force them into bankruptcy (or use this threat as a leverage to obtain concessions from borrowers).

In our empirical analysis, we take advantage of the fact that many corporate bonds that were traded on the Tel Aviv Stock Exchange in 2013 lacked customary financial covenants that provide bondholders with control rights in case of imminent distress ("no-covenant bonds"). Accordingly, prior to 2013, no-covenant bondholders lacked the power to take control of distressed companies unless they actually defaulted, that is, failed to pay some or all of their financial obligations. The ruling, therefore, enhanced the potential control rights of no-covenant bondholders by providing them with the power to force into bankruptcy debtors with a negative net worth. By contrast, this legal change did not meaningfully affect bondholders that were already protected by customary financial covenants: in these companies, whose covenants were already more stringent than the new court-granted power, bondholders could, regardless of the court ruling, accelerate payments on their bonds and initiate bankruptcy procedures if borrowers failed to satisfy their covenants. We rely on the difference between the two types of corporate bonds, with and without covenants, to measure the effect of the court ruling on affected companies, which we define as companies with no financial covenants and relatively low net worth (i.e., close to distress).[2]

We focus on distressed firms whose bonds included no covenants as of the end of 2012, which we regard as the population of firms most affected by the decision. Our measure of distress is based on (reported) net worth, scaled by total assets. We choose this measure for two reasons. First, net worth is the proxy for balance sheet insolvency, the

[2] The new rule did not materially affect banks and other private lenders. These lenders were likely to include covenants in their loan documents in any case. We return to this point below.

condition that could trigger creditor control rights under the court ruling. Second, the creditors most affected by the ruling were bondholders, who often rely on issuers' reporting and other publicly available information. Thus, we consider firms with no-covenant bonds and whose net worth is below the sample median (alternatively, no-covenant firms whose net worth is below the 33rd percentile) as "treated" by the court decision. Our control group consists of firms with covenants (regardless of their net worth) and no-covenant firms with high (above the median, or above the 33rd percentile) net worth. These firms were not affected by the court ruling, as they either already had covenants in place (in which case the court decision was unlikely to materially affect creditor power), or had high net worth and therefore were not sensitive to a court decision affecting firms in the vicinity of financial distress. We use the distinction between treated and control firms (which are similar in many respects) to estimate the effects of the court ruling in a "diff-in-diff" framework, comparing treated and control firms before and after the 2013 court decision.

The literature on creditor rights and bankruptcy tends to focus on creditor control within bankruptcy, or on cases in which creditors have direct or indirect control over distressed borrowers outside bankruptcy. This paper, in contrast, provides novel evidence on the consequences of a legal chance expanding creditors' power to initiate bankruptcy. In other words, the legal change we study does not affect the control of firms in bankruptcy, but allows creditors exercise control at an earlier stage of distress. The existing literature (described in the next section) suggests that, when creditors have some control over borrowers, they tend to effect changes in corporate behavior to cater to their interests (e.g., Becker and Stromberg, 2012; Vig, 2013). They can also restrain the use of aggressive accounting practices to make sure financial reports reflect the company's true state of affairs (Aier et al., 2014; Bens et al., 2020). Studies also show that the mere fear of creditor control in bankruptcy may also change corporate behavior, as corporate managers would hesitate to adopt risky policies that might increase the likelihood of creditor control (e.g., Acharya et al, 2011; Agarwal et al., 2021). But what is the effect of expanding creditors'

power to initiate bankruptcy? On the one hand, our study is consistent with the view that expanding creditors' power to initiate involuntary bankruptcy can benefit creditors, especially in distressed firms, for example by motivating such firms to raise new equity. On the other hand, this study sheds light on a relatively unexplored adverse effect of expanding creditor control rights in this way: providing distressed firms with incentives to inflate reported earnings, thereby undermining the quality of information that bond issuers provide to the market.

The rest of the paper is organized as follows. The next section describes some of the related literature and presents more details about the legal and institutional background for the reform we study. The data set and the empirical approach are described in Section 3. Section 4 presents our main empirical findings and Section 5 concludes. Extensions are provided in several appendices.

2. Related Literature and Institutional background

2.1 Related Literature

The Law and Finance literature has focused on the relation between investor protection, corporate ownership and financial market development. Within this line of research, the literature on creditor rights (e.g., Djankov et al., 2007, and Djankov et al., 2008) has generally regarded strong creditor protection as essential for the functioning of debt markets.[3] Nevertheless, giving creditors strong control rights over distressed companies (e.g., the power to remove management, or to seize the borrower's assets) might have other, indirect, effects. Becker and Stromberg (2012) show that subjecting managers of distressed firms to fiduciary duties toward creditors can affect corporate risk-taking. Vig (2013) illustrates how enhancing creditor rights in certain states of the world can lead to a "liquidation bias" and reduced leverage. Acharya et al. (2011) and Agarwal et al. (2022) focus on the effect of strong creditor rights in bankruptcy on the behavior of non-

[3] A related line of research emphasizes the efficiency of bankruptcy codes around the world (e.g., Davydenko and Franks, 2008).

distressed firms (outside bankruptcy). Acharya et al. (2011) emphasize the negative relation between creditor power in bankruptcy and corporate risk-taking, as borrowing companies adjust their behavior so as to lower the probability of becoming subject to creditor control. In the same spirit, the findings reported by Agarwal et al. (2022) imply that empowering creditors of distressed companies in Denmark (to choose between liquidation and reorganization) affects both the financial management and the real behavior of non-distressed firms.

We depart from the existing literature in corporate finance in several ways. First, we study a mechanism for enhancing creditor rights that has not been examined before – giving creditors the right to initiate bankruptcy procedures on the basis of balance sheet insolvency even before any actual default. This change affects pre-bankruptcy control rights while keeping unchanged creditor control rights once the debtor is in bankruptcy. This focus on technical (or accounting) insolvency as a trigger for involuntary bankruptcy is novel and is presumably of relevance to many jurisdictions.[4] Second, we study a change that was exogenous, surprising, and had an immediate effect. Unlike debt covenants negotiated and agreed upon in advance, for example, the rule we study was adopted by a court and immediately applied to all outstanding debts. These features preclude, or significantly limit, radical changes in corporate risk-taking behavior, at least in the short run, shedding light instead on the possible link between creditor rights and other dimensions of corporate conduct.

The accounting literature has long recognized that firms adjust their reporting practices so as to meet various regulatory and legal thresholds.[5] In particular, DeFond and

[4] In a somewhat similar case in the UK, a hedge fund holding bonds in the Colt Telecom Group argued in 2002 that the company's liabilities exceeded its assets and that it was therefore insolvent. However, the UK judge ruled that it was not the case. In the US, Hynes and Watt (2020) lament the near disappearance of involuntary bankruptcy petitions in the twentieth century and propose ways to encourage more such petitions so as to prevent incumbent management from delaying the onset of bankruptcy.

[5] For example, Burgstahler and Dichev (1997) discuss earnings management in the context of firms trying to avoid reporting losses, Matsumoto (2002) suggests that firms manage earnings to meet analysts'

Jiamboalvo (1994) show that firms use abnormal accruals to manipulate earnings and avoid debt covenant violations. Sweeney (1994) provides evidence in the same spirit on what is sometimes referred to in the accounting literature as the "Debt Covenant Hypothesis" (e.g., Dichev and Skinner, 2001). Franz et al. (2014) find that firms close to covenant violations use earnings management techniques more than other firms. More recent studies in this line of research include, among many others, Dyreng et al. (2022), who argue that covenant violations are costly and "justify" the use of some forms of earnings management to avoid them (although Graham et al., 2005, present a somewhat more nuanced view).

Especially relevant to the present study are several studies focusing on the impact of changes in creditor rights on borrowing firms' accounting practices. Using the same legal change as in Becker and Stromberg (2012), both Aier et al. (2014) and Bens et al. (2020) argue that creditor influence can have a restraining effect on borrowing firms' inclination to manipulate their financial reports by inducing accounting conservatism.[6] Whereas these studies examine the accounting practices of distressed firms under creditor influence, a recent working paper by Gopalan et al. (2016) focuses, much like the present study, on the link between changes in creditor rights and the accounting practices of non-distressed firms. Gopalan et al. (2016) examine a pro-debtor bankruptcy code reform in India (weakening creditor rights in bankruptcy) and its impact on earnings management, showing that the reform induced borrowers to manipulate downwards their accounting figures in order to qualify for protection from creditors. In contrast with the pro-debtor bankruptcy reform in India, our study looks at a pro-creditor legal change of a different type – an expansion of the states of nature in which creditors can initiate bankruptcy

expectations, whereas Bergstresser and Philippon (2006) focus on accounting manipulations in the context of executive compensation targets. These and other motivations to manage earnings (e.g., regulatory reasons, lending contracts etc.) are discussed in detail in a survey by Healy and Wahlen (1999).

[6] In the same spirit and using Indian data, Aghamolla and Li (2018) argue that stronger debt contract enforcement induces more accounting conservatism. Tan (2013) makes a similar point with respect to debt covenant violations. All these studies, however, focus on circumstances where creditors already have control rights and can influence firm behavior.

procedures – and documents how borrowing firms adjust their reporting figures upwards, so as to avoid forced bankruptcy.

The present study adds to the growing literature in corporate finance on the (sometimes unintended) consequences of policies empowering creditors. In line with previous studies, we show that, in some respects, creditors benefit from stronger creditor rights but, at the same time, the impact of such policies on firm incentives may have adverse effects. In line with Aier et al. (2014) and Bens et al. (2020), we also find that changes to creditor rights can impact not only firm behavior and capital structure, but also financial reporting. However, unlike these studies, the present paper, together with Gopalan et al. (2016), highlight a possible adverse effect of reforms in creditor rights on the accounting practices of firms in which creditors do not (yet) have effective control. Our paper illustrates how some changes in creditor rights, even if desirable (we do not take a stand on that), may push (non-distressed) firms fearing borrower control toward accounting aggressiveness.

2.2 Institutional and Legal Background

In 2013, a group of *IDB Pituach Ltd*. (IDB) bondholders believed that the company's financial condition was hopeless unless it negotiated with bondholders and other creditors to restructure its financial obligations. IDB, however, refused to commence negotiations on restructuring its debt, and continued to pay its short-term creditors. The bondholders had no contractual remedies against IDB: the company was current on all its payments (the first payment on some of the bonds was due only in five years, in 2018); IDB did not breach any covenant it owed to these bondholders (as the bonds in question had no meaningful covenants).[7] The bondholders, therefore, turned to bankruptcy procedures: they filed an involuntary bankruptcy petition in which they asked the court to appoint a trustee to pursue a debt-for-equity reorganization plan. Their claim was that IDB, although current on its payments, was insolvent (its liabilities outweighed its assets).

[7] The company, however, did get its bank creditors to waive some of their covenants.

IDB, in contrast, argued that it was current on all its payments, that its assets exceeded its liabilities, and that the bondholders were opportunistically trying to take control of the company. Moreover, IDB argued that, from a purely legal standpoint, creditors of companies that are current on their payments and do not violate any covenant or other contractual provision cannot force companies into bankruptcy simply by showing that the value of their assets is smaller than their liabilities.

In a precedential decision, the court held that creditors had the power to force a company into bankruptcy by demonstrating that it was insolvent (i.e., that its liabilities exceeded its assets). The court found that IDB was probably insolvent and issued an order appointing trustees to inspect the company's records and issue an independent opinion concerning IDB's insolvency.[8] This decision offers a unique opportunity to study the effect of an exogenously-imposed change in the scope of creditor rights.[9]

At a practical level, the new legal rule made reported measures of net worth crucial for companies in our sample for two related reasons. First, holders of publicly traded corporate bonds normally rely on the issuers' reported financial statements (and market

[8] The decision was surprising and controversial: First, many bankruptcy judges held the view that creditors' only remedy was liquidation, that is, creditors lacked the standing to initiate involuntary bankruptcy aimed at reorganization. In fact, even after the ruling, the senior bankruptcy judge in the Tel Aviv court held that the IDB decision lacked legal basis. Second, many believed that it provided creditors with excessive power against firms that did not breach any financial or other contractual obligation. Accordingly, two attempts were made in 2014 to challenge the court decision in the Supreme Court, but both failed.

[9] Three additional pieces of background information are worth noting. First, as noted above, in 2013 Israel did not have a debtor-in-possession (DIP) regime for corporate bankruptcy. When companies enter bankruptcy (whether liquidation or reorganization), courts appoint trustees to take over the company. Second, during our sample period, nearly all companies that issued bonds had controlling shareholders. Controllers, in turn, knew that entering bankruptcy could make them lose control over the company. Third, the Israeli market for corporate bonds grew at a rapid pace in the early 2000s. Until 2011, many companies (including IDB) issued bonds without meaningful covenants to protect creditors. The 2008-2009 financial crisis forced many companies that had issued bonds to restructure their debt. This, in turn, led to regulatory reforms aimed at improving the protection of bondholders, including a 2011 reform that pressured companies into issuing bonds with more covenants. In 2013, however, many companies still had outstanding bonds with no meaningful covenants. This implies that the companies affected by the court decision in our sample, those without corporate bond covenants and low net worth, had their debt issued earlier, on average, than companies with covenants, included in the control group. We discuss this in the Data Section below.

prices) to assess the bonds' risk of default. Similarly, the courts, in evaluating whether a company is solvent or not, are also likely to rely on the reported net worth. Second, the companies in our sample report under the IFRS regime. This means that reported measures of net worth are supposed to reflect (to a large extent) economic reality rather than historical book values. Recall that the new rule empowered creditors to force companies into bankruptcy when their debts exceeded their assets (balance sheet insolvency). From a purely legal standpoint, accounting measures of asset values are not determinative. Yet, a company that, under the IFRS regime, reports that its liabilities exceed the *fair value* of its assets, would most likely be unable to convince a bankruptcy court that it meets the test for balance sheet solvency.[10]

3. Data and Empirical Approach

Our sample includes all listed Israeli firms with traded corporate debt between 2012 and 2015. We exclude financial companies as well as companies cross-listed on other exchanges, typically high-tech companies listed on NASDAQ. To be included in the sample, firms must have had traded corporate bonds in 2012 (before the court ruling, which took place in April 2013), and in at least one year between 2013 and 2015 (the post-court ruling period). In the main part of our empirical analysis, we exclude firms if they enter/complete debt restructurings during the sample period to ensure that our results are driven by the effect of the court ruling on companies not involved in formal bankruptcy procedures.

The classification of firms into the treatment and control groups is determined by two criteria: the existence and nature of the financial covenants included in each firm's bonds; and the firm's net worth at the end of 2012. Companies with no financial covenants and

[10] The precedential court decision was relied upon in 2016, when bondholders of *Alon* (a private retail company with traded bonds but no traded equity) tried to seize control over it arguing that, just like *IDB Pituah* a few years earlier, *Alon*'s liabilities exceeded its assets.

close to default (i.e., with low net worth) were presumably affected by the court ruling and are therefore considered "treated."

We determine whether a bond includes financial covenants through a manual inspection of the annual financial statements of firms. We consider only covenants stipulating that the issuer must maintain financial ratios above certain thresholds, or that the issuer must not be subject to a credit downgrade.[11] These covenants, described in more detail in Appendix A, resemble in nature many of the covenants commonly used in (private) debt contracts of US firms (Chava and Roberts, 2008, Table 1, p. 2091). Given that the breach of such covenants enables bondholders to demand an immediate repayment of the bonds, companies with covenants of this type were not directly affected by the court ruling.[12]

In most of the analysis, we measure *Net Worth* as the (IFRS, fair value-based) reported total assets net of total liabilities, scaled by total assets (alternative measures are described below). We use this variable to identify companies close to default which are most likely to have been affected by the court decision.

Specifically, we classify a firm as "treated" if its bonds did not include financial covenants at the end of 2012 (the last financial statement published prior to the court decision) and

[11] We treat rating-based covenants similarly to financial covenants, as the bond rating itself is very sensitive to a deterioration in financial conditions. Our main results are robust to the exclusion of firms with rating-based covenants.

[12] (i) For firms with covenants, we examine their conditions and find that the threshold triggering immediate payment is typically set at a level where net worth (or related measures) are positive, i.e., they are more binding than the insolvency criterion used by the court (zero or negative net worth). This corroborates our maintained assumption that companies with covenants were not directly affected by the court ruling and can be used as a control group. (ii) Throughout our analysis, we focus on financial covenants that pertain to outstanding corporate bonds, ignoring covenants associated with bank loans. Bank loan covenants are different than bond covenants; they allow for more flexibility, renegotiation, and confidentiality than covenants provided to dispersed bondholders. Additionally, the court ruling was supposed to promote the rights of "public" creditors who, prior to the ruling, had limited means to protect their claims. Nevertheless, we examine the sensitivity of our results to the presence of bank loans and find that they are unchanged.

it was close to default, that is, its net worth, also measured at the end of 2012, was below the sample median (or, alternatively, below the 33rd percentile).[13]

This process yields 190 firm-year observations (52 distinct firms) with no financial covenants and below-median net worth (treated firms), and 461 firm-year observations (122 distinct firms) either with financial covenants or without covenants but with above-median net worth. In some parts of the empirical analysis we use only a subset of the control group consisting of firms with covenants and low net worth.[14] The majority of firm-year observations in our data are concentrated in the following four industries: real-estate (47%); services (18%); manufacturing (13%) and holding and investment companies (11%); this distribution is not representative of the Israeli economy, but is consistent with evidence that Israel's corporate bond market tends to over-represent companies in the real estate and service sectors (Brodeski, 2021; Kliger, Mugerman and Rooz, 2022). Financial statements and trading data are drawn from the Tel Aviv Stock Exchange (TASE) website. We also collect financial statements' filing dates from the Israel Securities Authority's (ISA) website (www.magna.isa.gov.il), using a web scraping tool. These filing dates are used for calculating informativeness measures described below.

[13] We do not measure proximity to default using Merton's (1974) "distance-to-default". The reasons are that some of the companies in our data set do not have traded equity (only bonds) and share prices are needed for this calculation. In addition, there are companies in the sample whose shares are not very liquid, a feature which may affect the calculation of the standard deviation of equity prices which is used in the distance-to-default formula.

[14] If a firm has issued multiple bond series, we classify the firm as having a covenant if at least one of the issued bonds has a financial covenant attached to it. As noted, we utilize December 31, 2012, which is the year-end immediately preceding the April 2013 IDB court ruling, as our cutoff point to distinguish between companies with and without financial covenants associated with their outstanding bonds. Yet, as of August 11, 2012, two regulatory reforms were promulgated that independently enhanced creditor rights in newly issued bonds. Pursuant to the new reforms, bondholders of every newly-issued bond became entitled to require an immediate repayment of their bond if (1) there has been a deterioration of the issuer's business in relation to its financial situation at the time of the issuance of the bond, and (2) there is a substantive concern that either (i) the issuer will not be able to repay the bonds on time or (ii) the issuer will not meet its material obligations to the bondholders. These triggering events of repayment are applicable even if the contractual terms of the bond do not implicitly include these terms. Therefore, two firms that issued new bonds between August 11, 2012 and the end of 2012 were classified as part of the control group.

Panel A and Panel B of Table 1 present the treated and control sub-samples in the pre- (2012) and the post- (2013-2015) court ruling periods (Appendix B provides detailed variable definitions). The statistics refer either to the entire control group (middle columns), or to the subset of firms in the control group with low (below median) net worth and no covenants; in our empirical analysis below, we sometimes use the entire control group and sometimes focus on comparisons between low (high) net worth companies without covenants with low (high) net worth firms whose bonds include covenants.

Table 1 indicates that the average reported net worth scaled by assets – our proxy for firm proximity to creditor control – while roughly constant for firms in the full control group (at about 33%), as well as in the subset of control firms with low-net worth and no covenants, doubles for treated firms from about 7% prior to the 2013 court decision to about 14% in the post-court ruling period.[15] This dramatic increase is reflected also in the frequency of firms with negative net worth, which goes down by 50%, from 15% of the treated firms in 2012 to 7% in 2013-2015, while remaining constant at about 2% for the control group firms (6% in the subset of the control group with low net worth). These stylized facts will be examined in more detail below.

Treated companies tend to be somewhat smaller than firms in the control group, although there is considerable heterogeneity in size within the sub-samples of treated and control firms. Naturally, treated companies tend to be less profitable in the pre-court ruling period (2012), especially in comparison with the full sub-sample of firms in the control group; this difference disappears in the post-court ruling period. As bond covenants became more common in Israel after 2010 (following a government-appointed committee which recommended that covenants be used), the sub-sample of treated firms includes more "old vintage" bonds issued before 2010. However, included in the control

[15] The change is more apparent in the means, rather than the medians, because it is driven primarily by companies with very low net worth.

group are also firms with pre-2010 bonds with covenants, as well as firms with pre-2010 no-covenant bonds but high net worth. In addition, the distribution of firms across industries is quite similar in the treated and control samples, as are their levels of bank debt (not shown). Cross-sectional LPM regressions for 2012 predicting which firms are treated on the basis of size, profitability and industry (not shown) have very low predictive power.[16] Furthermore, net worth exhibits similar (parallel) downward-sloping trends prior to 2013 among both treated and control firms (Appendix C).

Our empirical approach is based on a standard difference-in-differences methodology, comparing changes in the various outcome variables in the treated group in 2013-2015 relative to 2012 with changes in the same variables in the control group over the same time period. The first outcome variable we focus on is changes in net worth. After documenting an increase in net worth among treated firms (reflecting a reduction in the likelihood that creditors could impose bankruptcy procedures following the court ruling), we turn to possible factors explaining this change: equity issuances and changes in proxies for accounting aggressiveness (discretionary long-term accruals and measures of accounting conservatism). Finally, we study the impact of increased accounting aggressiveness among treated firms on the informativeness of their financial statements.[17] For all outcome variables, we present regression results where the main variable of interest is an interaction term representing treated firms in the post-court ruling period (*treated*post*). In all regression specifications we include firm-level control variables, as well as year and firm fixed effects, and cluster the standard errors at the firm level.[18]

[16] Size is the only significant variable in these regressions (indicating that treated firms tend to be smaller than firms in the control group). This difference is less pronounced when comparing low-net worth treated and control firms.

[17] The precise definitions and derivations of these variables appear below.

[18] We use standard errors clustered by firm, combined with time (year) fixed effects, as is common in the literature. According to Petersen (2008), when the time dimension is short, two-way clustering (by firm and year) is not recommended: "When there are only a few clusters in one dimension, clustering by the more frequent cluster yields results that are almost identical to clustering by both firm and time" (p. 460). In our

4. Main Results

4.1 The Market Reaction to the Court Ruling

We begin by examining the perception of the court ruling by bondholders and shareholders. The objective of this test is to establish that the court ruling was surprising and expected to benefit creditors. It is important to note that corporate bonds in Israel are traded on a centralized exchange (not over the counter, as in most countries), offering high liquidity, low spreads and low trading costs relative to the corporate bond markets in other parts of the world (Abudy and Wohl, 2018), thus rendering credibility to a bond price-based event study approach.

Table 2 presents changes in bond prices on the date of the court ruling, April 30, 2013: We find that bonds issued by treated companies (with no covenants and net worth below the sample median) experienced, on average, positive (and statistically significant) excess returns of about 0.2% relative to portfolios of bonds of similar rating, indexation and maturity issued by control group firms.[19] The excess returns are larger when treated firms are defined as having no covenants and net worth below the 33rd percentile (i.e., closer to default): bond excess returns in this treated group are, on average, close to 0.3%.[20]

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data set, the time dimension includes only four years. Notwithstanding this reservation, the results reported below continue to hold (i.e., the coefficients of interest remain statistically significant) when the standard errors are clustered at both the firm and the time (year) levels. In the same spirit, the results continue to hold also when we follow the advice of Bertrand et al. (2004) and collapse the time series information for each firm in the post-2013 period into a single (average) observation (the pre-court ruling period consists of one year only, 2012, so there is no need to collapse the pre-court ruling observations).

[19] At the time, the vast majority of corporate bonds in Israel were inflation-index. About half of the treated companies have positive bond excess returns on the event date. The returns are winsorized at the 1% and 99% percent; the results are qualitatively similar without winsorizing. Since the court decision was made public on 4/30/2013 in the afternoon, we also examine abnormal bond and stock returns on the court decision date and the following day (4/30/2013-5/1/2013), finding qualitatively similar results. The reported *t*-statistic is based on clustering at the firm level, as some firms have multiple bond series (88 bond-level observations for 45 treated firms; seven treated firms have missing data on bond rating or duration, or no bond price data on the relevant dates).

[20] One possible interpretation of the positive bond price response to the court ruling could be the expectation that creditors might be able to initiate bankruptcy procedures earlier than in the pre-2013 years. In an appendix to a 2014 report issued by a government-appointed committee to examine the deficiencies of bankruptcy procedures in Israel (the "Andorn Committee"), written by one of the authors of the present paper (Steinberg), it is reported that many companies on the verge of financial distress in the

Moving from bond to stock prices, we use the "market model" and find negative excess returns in the treated group of about minus 0.8% in response to the court ruling (whereas the average excess returns of control group firms on that day is +0.15%, a statistically significant – at the 10% level – difference). The stock excess returns are even more negative (minus 1.2%) when treated firms are defined as having (no covenants and) net worth below the 33rd percentile. Changes in total firm value (excess bond returns, times the value of outstanding bonds, plus excess stock returns times the value of equity) for treated firms are marginally positive (0.14-0.15%), although the number of observations which can be used in this calculation is too small to draw firm conclusions. Overall, these findings may be interpreted as indicating that market participants considered the court ruling as beneficial to creditors, mostly at the expense of shareholders.[21]

4.2 Real Changes in Corporate Behavior in response to the Court Ruling

Creditor control, or a fiduciary duty to cater to creditors' interests, could lead borrowers to change their corporate policies so as to reduce risk (e.g., Becker and Stromberg, 2012). We therefore examine if treated firms reduced their risk after 2013, in comparison with the control group. The measures of risk we use are the change in the volatility of ROA and stock return volatility (in the spirit of Merton, 1974, and in line with Rajgopal and Shevlin, 2002), measured over 12 quarters before the court ruling (2010-2012) and 12 quarters

period 2008-2013 entered formal bankruptcy procedures only after their financial condition had deteriorated considerably, resulting in substantial "haircuts" to creditors. The small number of bankruptcies in our sample in the post-2013 period precludes a formal analysis of this question. In addition, we detect no meaningful cross-sectional variation in excess bond returns, that is, the excess bond returns are not correlated in the cross-section with net worth or the presence of bank debt (which could affect the relevance of the new creditor rights granted by the court). This is also likely to be due to the small sample size. Appendix D presents additional information on bond yield-spreads: Prior to the court decision, the spreads (yield differences) between bonds issued by treated firms and government bonds of similar maturity and indexation were higher than the spreads on bonds issued by firms in the control group. These differences narrowed in the post-court ruling period.

[21] (i) This reported excess returns are based on the calculation of Scholes-Williams *beta* using daily data for eleven months (month -12 to month -1) prior to the court ruling. There are 38 firms for which data are available (out of the 52 treated firms); six firms have no traded equity (only traded bonds) and for another eight no price information is available on the date of the event (presumably, because of no trading). 23 of these firms exhibit negative excess returns. The correlation between bond and stock excess returns is low; the small number of observations precludes a more detailed analysis of this issue.

after it (2013-2015).[22] However, we find no evidence of any change in these measures of corporate risk taking. One interpretation of this non-result is that, in contrast with studies like Becker and Stromberg (2012), that examines firm behavior after creditors already have some control rights (or influence on corporate decisions through a change in management's fiduciary duties), treated firms in our sample are not directly influenced by their creditors' preferences. Acharya et al. (2011), however, argue that risk taking might be affected even before creditors actually seize control by the prospects of creditor-induced liquidation if firms become distressed. In our setting, by contrast, the court's decision took effect immediately, subjecting low net worth firms to the risk of imminent creditor-induced involuntary bankruptcy. Companies in our sample, often operating in real estate, could not adjust their risk profile (or on-going projects) immediately in response to the court decision, and therefore chose to respond in other ways.

4.3 Changes in Net Worth

We examine differences in (the reported value of) net worth between the treated and control groups. Focusing on the most distressed firms, Figure 1 presents the cumulative distribution of net worth around zero (in the range between -10% to +10%) for the treatment and control groups before and after the court ruling. While the net worth of firms in the control group remains roughly similar before and after the court ruling, the net worth of treated firms, with no financial covenants and low net worth, at risk of involuntary bankruptcy, increases substantially. More formally, a Kolmogorov-Smirnov test for equality of distributions rejects the null hypothesis that the distribution of net worth is identical before and after the court ruling for treated firms (*P-value* of 0.02) but this null cannot be rejected for the firms in the control group. The significant shift in the distribution of net worth, especially in low levels, suggests that treated firms might have

[22] As the vast majority of firms in our sample are in non-manufacturing sectors, we do not use R&D expenditures, which have been used as a proxy for risk in the literature, as a measure of risk taking.

found ways to increase their net worth in order to prevent their creditors (bondholders) from activating their conditional control rights.

We now proceed to a difference-in-differences regression model with *Net Worth* as the dependent variable. Table 3A shows that the net worth of treated firms goes up significantly after the court ruling. Columns 1 and 4, the full sample regressions, indicate that net worth increases for treated firms by 3-4 percentage points relative to the control group in the years 2013-2015. In Columns 2 and 3 we split the sample into low net worth and high net worth firms; the dramatic increase in net worth (of five percentage points) is especially pronounced when comparing treated low net worth firms (with no covenants) with low net worth control firms (with covenants). Arguably, this sub-sample is especially relevant for the issue at hand as it includes only firms that are relatively close to default, distinguishing between those affected by the court ruling (no-covenant firms) and other low net worth firms. Similarly, in Columns 5 and 6 we split the sample into firms with very low net worth (below the 33rd percentile) and firms with net worth above the 33rd percentile. The increase in net worth is especially pronounced for the sub-sample of very low net worth firms: treated firms, with covenants experience, a net worth increases by about eight percentage points relative to the very low net worth control group (with no covenants). The results remain unchanged when non-controlling interests are excluded from the calculation of net worth, as well as when non-tangible assets are excluded (or both). Appendix E presents the estimated effects graphically, by year, showing a significant increase in net worth in 2014-2015 (the post court-ruling years) relative to 2012 (the base year).

We obtain qualitatively similar results also when running LPM (or Probit) regressions, where the dependent variable is a dummy variable that takes the value one if the company has negative net worth (Table 3B). In line with the univariate sample statistics presented in Table 1, the proportion of no-covenant, low net worth firms with negative net worth declines dramatically in the post-court ruling years, even in regressions with

controls for other firm characteristics.[23] As in Table 3A, the results are driven by the no-covenant, low net worth firms (Columns 2 and 5, where only low net worth treated and control firms are included).

Next, we turn to two possible explanations for the "miraculous" increase in net worth among treated firms facing the risk of impending creditor control.

4.4 New (Seasoned) Equity Issues

Almost half of all firms affected by the court decision (24 no-covenant, low net worth firms) raised new equity capital in the years following the court decision, as illustrated in Figure 2. This was done primarily (in 19 of the 24 cases) through private placements of equity in the hands of existing (or specific) shareholders, as well as through warrant issues.[24] The tendency of no-covenant, low net worth firms to issue new equity after the court ruling is evident also in difference-in-differences regressions: Table 4, which is similar in structure to Table 3A, shows that the value of seasoned equity offerings (SEOs) by treated firms increased significantly after the court ruling: Columns 1 and 4, the full sample regressions, indicate that the stock issuances of treated firms (scaled by total assets) increased by almost 2 percentage points relative to the control group in the years 2013-2015. When we split the sample by net worth, we find, in line with our findings for the change in net worth, that the increase in stock issuances is driven by treated firms with low net worth. This is evident also in Figure 2, where the increase in stock issuances following the court decision is especially prevalent among distressed firms in the bottom decile of the net worth distribution as of 2012.

[23] The frequency of negative net worth among treated firms in the pre-court ruling period (2012) is about 15% (Table 1). The coefficients in Table 3B imply a decline by about 40% in this frequency in the full sample and a decline by about 60% in the sub-sample of no-covenant and very low (below the 33rd percentile) net worth.

[24] The five firms offering equity to the public had relatively high net worth, even though they are part of the treated sub-sample of firms because their net worth is below the sample median.

To the extent that distressed firms can raise new equity capital, this increases their cash reserves and thus offers protection from creditors. This effect is, presumably, in line with what the court ruling was intended to achieve. Nevertheless, the evidence on some treated firms raising additional capital does not preclude the possibility that these firms also used other means, perhaps less beneficial to creditors, to increase their net worth. One such possibility is aggressive accounting practices to which we turn next.

4.5 Accounting Changes

We conjecture that the observed changes in the net worth of treated firms may be driven not only by real changes (equity injections), but also by changes in accounting policies and practices affecting how reality is presented to investors. Recall that the court ruling made zero (or negative) net worth the threshold at which bondholders had the right to impose bankruptcy. In this section, we examine if, in response, treated firms have adopted accounting practices to inflate their net worth and forestall creditor control (given their inability to change their operations in the short run). By contrast, for firms in the control group, whose bonds had already included financial covenants in 2012, or whose net worth was high, the incentives to try and increase their reported net worth should have remained constant before and after the court ruling.

4.5.1 Long-term Discretionary Accruals

What accounting mechanisms could firms have used to increase their net worth? Gopalan et al. (2016) suggest that earnings management is the primary accounting technique used by Indian companies to adjust the value of their net worth, in their case downwards, when facing a pro-debtor bankruptcy regime. However, Israeli firms follow the IFRS, whose impact on earnings management (and on the empirical proxy used to measure it, discretionary accruals) is unclear.[25] We argue that the enhanced discretion of

[25] In general, IFRS-based accounting systems allow for considerable discretion by reporting firms, as it is based on general principles, as opposed to very precise rules: Ball (2016, Section 6.3.1, p. 553) describes a wide range of choices firms have in implementing IFRS rules. Jeanjean and Stolowy (2008) write that "the

management with regard to accounting choices under the IFRS (in comparison with US GAAP) is likely to be especially pronounced with respect to long-term, non-current assets (Jeanjean and Stolowy, 2008). For example, under IFRS, companies can designate certain real-estate assets as "investment property" and measure them at fair value; under US GAAP this would be prohibited. Such differences may make it difficult to detect earnings management in current assets or working capital, but it is possible that earnings management practices would manifest in non-current assets/long-term discretionary accruals.[26]

Table 5 presents regression results where the dependent variable is discretionary long-term accruals (the calculation is based on the modified Jones model of Dechow et al., 1995, and the distinction between short and long-term accruals is as in Teoh et al., 1998). In line with our conjecture, we find that low net worth, no-covenant firms use more long-term discretionary accruals in the post-court ruling period than in the pre-court ruling period (relative to the control group). When defining treated firms as those with no covenants and very low net worth (below the 33rd percentile), the estimated magnitude of the effect (the difference in differences) is even larger. These findings are illustrated graphically in Figure 2: firms with no covenants in low net worth deciles exhibit a pronounced increase in the use of discretionary long-term accruals.[27]

application of accounting standards involves considerable judgment and the use of private information, and as a result, IFRS… provide(s) managers with substantial discretion" (p. 481). They also report that the mandatory implementation of IFRS was associated with an increase in earnings management in France, but not in the UK and Australia (see also Barth, 2008, who argues that IFRS reduces earnings management). Ahmed et al. (2013) also argue that the IFRS allows for "aggressive" reporting of accruals. In the context of Israel, Chen et al. (2019) describe the use of optimistic appraisals under IFRS to justify dividend payouts.

[26] Callao and Jarne (2010) find that the transition to IFRS in Europe has opened the way for a variety of earnings management methods, including those involving long-term discretionary accruals, which we use as well. Specifically, it is possible that discretion might affect such accruals under IFRS even though they are not commonly the focus of the discussion in US GAAP-based studies (e.g., Teoh et al., 1998).

[27] In contrast with the results for discretionary long-term accruals, we do not observe similar effects when using short-term accruals (not tabulated). We interpret this as evidence of the increased discretion under IFRS with respect to long-term assets, whereas its effect on shorter-term balance sheet items appears to be ambiguous.

4.5.2 Decline in Accounting Conservatism in Treated Firms

Another accounting policy change which could explain the increase in net worth of treated firms following the court ruling is their avoidance of timely recognition of losses, i.e., a departure from the accounting principle of conservatism (e.g., Basu, 1997). According to this principle, losses (bad news) should be recognized more quickly and with a lower degree of certainty than increased profits (good news), that is, reported earnings should reflect bad news more readily than good news.[28]

In our setting, treated firms are affected by a judicial decision that empowers creditors in the event of negative net worth without actually giving them immediate control rights over borrowing firms. We hypothesize that, in such a setting, borrowers are likely to deviate from the principle of conservativism, i.e., postpone loss recognition and perhaps precipitate the recognition of income (in comparison with the control group), so as to avoid reaching the threshold that would trigger creditor control if their net worth becomes negative.

In order to measure the degree of conservatism of treated and control firms, before and after the court ruling, we adopt a specification proposed by Ball and Shivakumar (2005), which is based on Basu's (1997) interpretation of conservatism using the tendency-to-reverse of net income:[29]

[28] Ball and Shivakumar (2005) point out that timely loss recognition is related to debt contracting efficiency, since it enables creditors to react immediately (as economic losses are reported promptly) to a deterioration in a firm's condition that triggers a debt covenant violation.

[29] Our tendency-to-reverse measure uses income statement data, as in Ball and Shivakumar (2005), rather than the earnings-returns measure of Basu (1997). This is primarily because of the shortcomings of the earnings-returns measure discussed in Dechow et al. (2010, starting on p. 363). In addition, this approach allows us to test the extent of timely loss recognition in firms with traded bonds but non-traded equity (14 treated firms and 22 control firms in our dataset did not have traded equity at the time of the court ruling, so we lose more than third of our firm-year observations when estimating equity-based measures). Ball and Shivakumar (2005) propose also an accruals-based test of conservatism which we do not use here, given that the effects we document tend not to be evident in standard (rather than long-term) accruals-based measures. In passing, although Ball and Shivakumar (2005) use data from the UK, their sample period predates the adoption of IFRS there, which may explain why they choose to use a standard accruals-based measure in some of their empirical tests.

$$\Delta Net\_Income_t = \alpha_0 + \alpha_1 Neg\_\Delta Net\_Income_{t-1} + \alpha_2 \Delta Net\_Income_{t-1} + \alpha_3 Neg\_\Delta Net\_Income_{t-1} * \Delta Net\_Income_{t-1} + \varepsilon_t \quad (1)$$

Where ΔNet_Income_t is the change in total income from fiscal year *t-1* to fiscal year *t*, scaled by the book value of total assets at *t-1*, and $Neg\_\Delta Net\_Income_{t-1}$ is a dummy variable that takes the value one if the income change is negative. In accordance with the principle of conservatism, α_2 is expected to be equal to zero, since the recognition of economic gains (unlike losses) takes place at a later stage – corresponding to the actual realization of the gains by a parallel increase in cash flows – making these gains persistent and unlikely to be reversed. α_3 is expected to be negative, since losses are recognized to their full extent in a timely fashion (i.e., when they are known). This implies that losses are recognized as a transitory income decreases and hence tend to reverse in the next period.[30]

We modify Equation (1) to allow for differences between treated and control firms before and after the court ruling:

$$\Delta Net\_Income_t = \alpha_0 + \alpha_1 Neg\_\Delta Net\_Income_{t-1} + \alpha_2 \Delta Net\_Income_{t-1} + \alpha_3 Neg\_\Delta Net\_Income_{t-1} * \Delta Net\_Income_{t-1} + \alpha_4 Post * Treated + \alpha_5 Neg\_\Delta Net\_Income_{t-1} * Post * Treated + \alpha_6 \Delta Net\_Income_{t-1} * Post * Treated + \alpha_7 Neg\_\Delta Net\_Income_{t-1} * \Delta Net\_Income_{t-1} * Post * Treated + Size + Fixed\ Effects + \varepsilon_t \quad (2)$$

[30] To illustrate this, Basu (1997) describes "a firm receiving news that changes its estimate of the productive life of a fixed asset. If the new estimated life is longer, the firm is economically better off, but under historical cost accounting no gain is recorded currently. Instead, the depreciation charges that would have been taken in the current and future periods are spread out over the new remaining life, resulting in lower depreciation charges. If the expected life decreases… the accountant records an asset impairment which results in… reduced current income, but no effect on future income" (pp. 4-5). As a result, "since accountants typically report the capitalized value of bad news as losses, bad news earnings is more timely but less persistent. In contrast, good news is reflected in earnings on a less timely basis, but good news earnings tends to be more persistent. Good news earnings is less timely because accountants require more verifiable information before they recognize good news. But good news earnings is more persistent than bad news earnings because the capitalized value of the good news is only partially reflected in current earnings, and after verification, is also reflected in subsequent earnings" (p. 6).

As before, treated firms are defined as firms with financial covenants and net worth in 2012 below the sample median (33rd percentile); *Post* takes the value one for the years 2013-2015. We control for firm size and include year and firm fixed effects.[31] If, after the court ruling, treated firms became less conservative, we would observe either the recognition of non-persistent transitory gains, in which case we would expect α_6 to be negative, and/or a delay in loss recognition (if, after the court ruling, treated firms do not report losses in a timely manner), in which case we would expect α_7 to be positive.

Table 6 presents the results. In Column 1, α_6 is negative (though not statistically significant) and α_7 is significantly positive, indicating that, after the court ruling, treated firms delayed the recognition of losses, thus increasing their net worth and avoiding foreclosure by creditors. As in the previous tables, we split the sample into low (below the sample median) and high net worth firms (Columns 2 and 3, respectively), finding that the decline in conservatism is driven by no-covenant, low net worth firms. Columns 4 – 6 present similar regressions when low net worth firms are defined using the 33rd percentile. In line with the results in Table 3A regarding changes in net worth, we find that reduced conservatism is especially pronounced in the sub-sample of very low net worth firms with no covenants. Another approach to measuring accounting conservatism is the *C-Score*, used in Khan and Watts (2009) and in Tan (2013). The results of the *C-Score* estimation, presented in Appendix F, are consistent with reduced conservatism among treated firms. Appendix G provides further illustrations and concrete examples of the use of accounting practices by firms in our sample to inflate their net worth.

4.6 Informativeness

Bondholders are concerned with the ability of accounting information to adequately and promptly convey downside risks and unfavorable information (Givoly, Hayn and Katz, 2017). If treated firms' financial statements are subject to aggressive, or overly-optimistic, accounting practices, their informational content to bondholders may be reduced. We

[31] Thus, absorbing the effects of the *post* and *treated* dummy variables, respectively.

test this conjecture by examining abnormal bond returns, where the calculation is relative to portfolios of bonds matched by maturity, rating and indexation, within three-days of the financial statements' filing date before and after the court ruling, for treated and control firms.[32]

Table 7 reports the results. If earnings reported in accounting statements were registered (recognized) using aggressive accounting techniques, the price response to their publications should be limited. In line with this conjecture, the interaction term denoting treated firms after the court ruling (times earnings) is negative (Columns 1 and 4), with the results being driven primarily by low net worth/no covenant firms (below the sample median in Column 2 and below the 33rd percentile in Column 5). We conclude that the increased incentive of firms to make sure their accounting statements do not make them vulnerable to creditor control may result in reduced informational content that accounting reports convey to creditors.[33]

4.7 Further Robustness

4.7.1 Calculations using Propensity-Score-Matching (PSM)

As an alternative to the linear regression models used in our empirical analyses so far, we use PSM to match treated companies with similar firms in the control group on the basis of their 2012 size, profitability and industry (using the nearest-neighbor, one-to-one

[32] In Israel firms do not report an early earnings announcement. Therefore, the filing date of the financial statements is the relevant date for estimating the market response to earnings.

[33] We also examine changes in the informativeness of share prices and do not find significant results. One possible explanation for this is that, in companies close to default, like the treated companies in our sample, the relevant security prices are bond rather than equity prices. Another, technical reason, could be that the sample of firms with available equity prices is smaller than that for which we have bond price information, partly because of companies with no traded equity (only corporate bonds) and partly because of companies whose equity trading is infrequent and not very liquid. See also footnotes 19 and 29. Finally, it would be of potential interest to examine trends in informativeness over long periods of time. We do not perform such tests here as the level of informativeness in the long-run is likely to be influenced by regulatory and other changes taking place after the court ruling. Note, however, that the results presented here are based on differences in informativeness between treated and control firms, and are therefore unlikely to be driven by general trends.

matching without replacement). Appendix H presents the results, corresponding to the results presented in Tables 3A, 3B and 5. The coefficients are very similar (in signs, magnitudes and statistical significance) to those reported in the original tables.[34]

4.7.2 Falsification/Placebo Test

Although it is impossible to rule out completely other changes which may have affected the treated and control firms during our sample period, Appendix J presents a figure similar to Figure 1 comparing changes in the net worth of treated and control firms which are close to distress. In contrast with Figure 1, where we focus on 2013, the year of the court ruling, in Appendix J we focus on 2012 instead. There are no observable differences between the treatment and control groups.

5. Concluding Remarks

We study the effects of an exogenous positive shock to creditor rights, emanating from a high-profile 2013 court case in Israel, where unsecured creditors were granted the ability to force borrowing companies into bankruptcy if the value of their liabilities exceeded the value of their assets. We find that investors interpreted the court ruling as beneficial to bondholders (and detrimental to shareholders), perhaps because of the expectation for an early onset of future bankruptcy procedures or because some firms raised new equity capital. At the same time, the attempt to empower creditors had other, unintended, consequences, primarily in the form of inducing borrowing companies to use (legal) accounting "tricks" to inflate their net worth. As a result, we observe a significant decrease in a commonly-used measure of informativeness to bondholders of financial reports issued by treated firms.

These findings emphasize one possible downside associated with policies that expand the set of contingencies in which creditors can impose involuntary bankruptcy on borrowing

[34] We also re-estimate the regressions presented in Tables 6 and 7 using PSM, with similar results to the main results presented in these tables. The full results are available upon request.

firms without giving them control or influence on current firm decisions. Such conditional creditor rights may induce firms to try and make sure the states of the world in which creditors can seize control will never materialize, and doing so through aggressive accounting practices may lead to reduced transparency and lower value of mandatory disclosure policies. Previous studies have shown how firms adjust their capital structure and risk-taking to accommodate a change in creditor rights (e.g., Becker and Stromberg, 2012; Vig, 2013; Agarwal et al., 2021). Other studies have shown that firms may change their accounting reports when creditors can influence corporate decisions (Aier et al., 2014; Bens et al., 2020). Our study complements this literature and demonstrates how firms not controlled by creditors, whose ability to make real changes is limited, respond to creditor empowerment by adjusting the way in which reality is reflected in their accounting reports.

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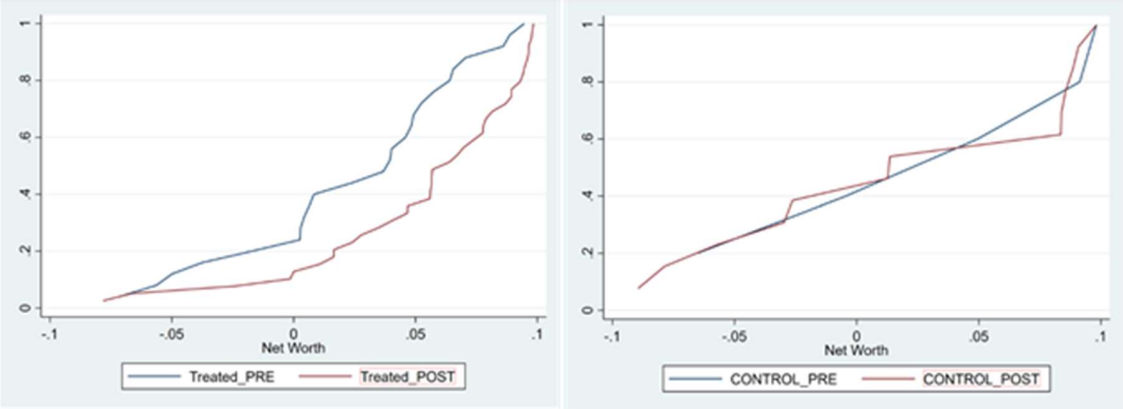
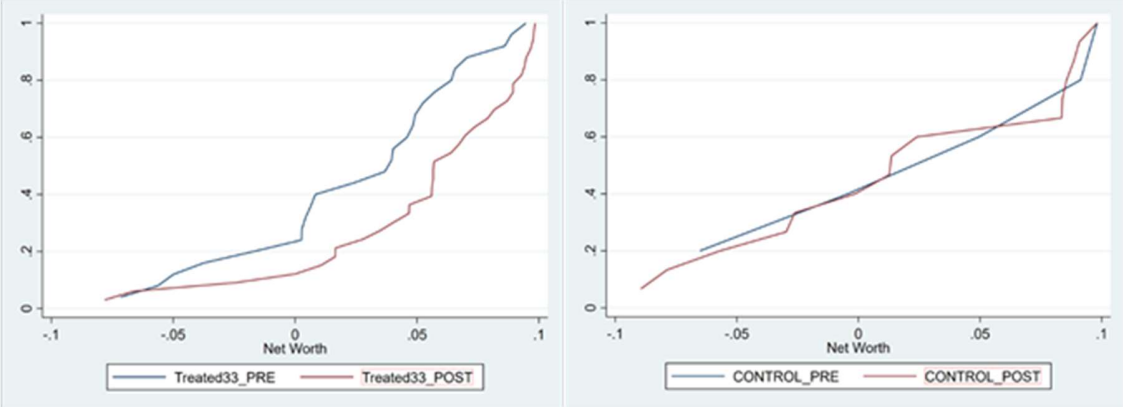
Figure 1: The Distribution of Net Worth (scaled by total assets) around Zero

Panel A: Treated firms (with no covenants and net worth below the sample median) vs. control firms before and after the court ruling (2012 vs. 2013-2015)

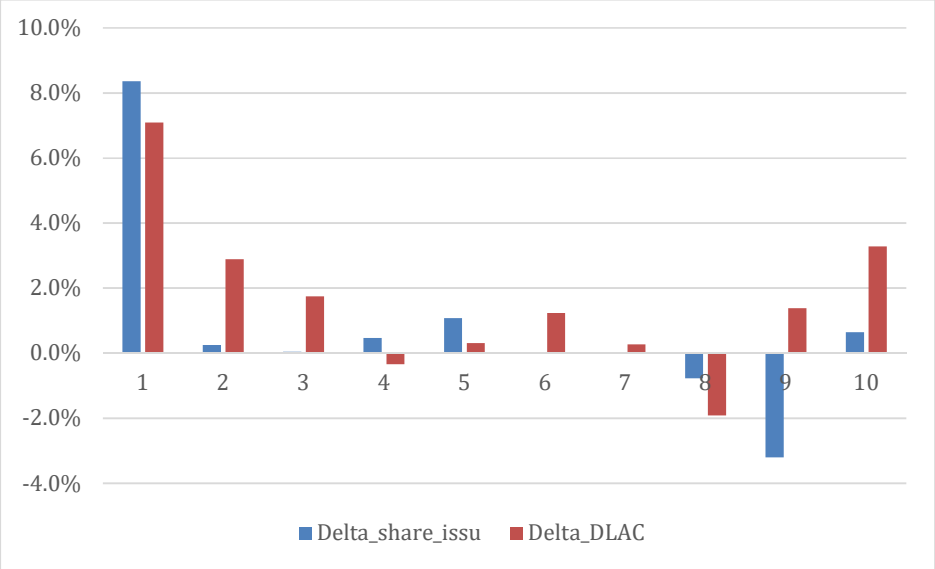


Panel B: Treated firms (with no covenants and net worth below the 33rd percentile) vs. control firms before and after the court ruling (2012 vs. 2013-2015)



The figure is based on the cumulative distribution functions for treated and control firms before and after the court ruling. We separately compute, and show in the figure, eight cumulative net worth distribution functions in the scaled net worth range (-0.1, 0.1) for the treatment and control groups before (PRE, in blue) and after (POST, in red) the court ruling (2012 vs. 2013-2015). Panel A presents the distributions of treated firms (with no covenants and net worth below the sample median) vs. control (other) firms; while in Panel B treated firms are defined as firms with no covenants and net worth below the 33rd percentile; the control (other) firms are defined accordingly.

Figure 2: Share Issuances and Changes in Long-term Discretionary Accruals by Net Worth Deciles



The figure presents, for all firms without covenants, new share issuances (in blue) and changes in discretionary long-term accruals (in red) by net worth deciles in 2012. New share issuances are defined as equity capital raised (in seasoned public offerings on the Tel Aviv Stock Exchange), scaled by total assets. The change in long-term discretionary accruals is the percent change in the firm's mean value in the post-court ruling period (2013-2015) relative to its 2012 value. Discretionary long-term accruals are calculated using the modified Jones model as in Dechow et al. (1995) and the distinction between short and long-term accruals is based on Teoh et al. (1998). See also Table 5 below. The bars represent the average change within each 2012 net worth deciles (omitting observations where net worth scaled by total assets is larger than +1 or smaller than -1). Treated firms are firms that do not have covenants and whose scaled net worth in 2012 is below the sample median, hence they are included in deciles 1 through 5.

Table 1: Descriptive Statistics

Panel A: Pre-court Ruling Period (2012):

	Treated=1			Treated=0			Treated=0 & Below-median Net Worth		
Variable	N	Mean		N	mean	Difference	N	mean	Difference
Net_Worth	52	0.0750		122	0.3247	-0.250***	34	0.1526	-0.0776**
Negative_Net_Worth	52	0.1538		122	0.0164	0.137***	34	0.0588	0.095
SIZE	52	13.7186		122	13.9741	-0.256	34	14.5002	-0.782**
ROA	52	0.0153		122	0.0443	-0.0289**	34	0.0287	-0.0134

Panel B: Post-court Ruling Period (2013-2015)

	Treated=1			Treated=0			Treated=0 & Below-median Net Worth		
Variable	N	Mean		N	mean	Difference	N	mean	Difference
Net_Worth	138	0.1424		339	0.3378	-0.195***	94	0.1561	-0.0136
Negative_Net_Worth	138	0.0725		339	0.0206	0.0518***	94	0.0638	0.0086
SIZE	138	13.8930		339	14.0825	-0.189	94	14.5892	-0.696***
ROA	138	0.0312		339	0.0381	-0.00699	94	0.0301	0.0011

Note: The table provides descriptive statistics for the treated and control (non-treated) firms. Panel A presents the pre-court ruling year (2012) and Panel B the post-court ruling period (2013-2015). Treated firms are firms that do not have covenants and whose net worth over assets in 2012 is below the sample median. Control firms are firms that do not have financial covenants but whose net worth over assets in 2012 is above the sample median, or firms that have financial covenants, regardless of their net worth. Net Worth is measured as the total assets net of total liabilities, scaled by total assets (observations above one in absolute value are omitted); Negative Net Worth is an indicator variable that takes the value one if net worth is negative, and zero otherwise; *SIZE* is natural log of total assets; *ROA* is operating profit over total assets. Appendix B provides a detailed description of the variable construction. *SIZE* and *ROA* are winsorized at the 1st and 99th percentiles of their distribution. The last two columns show the statistical difference between the means of the treated and control firms. Standard errors are reported in parentheses. ***, **, and *, indicate that a t-test for the difference between the means is significant at the 0.01, 0.05, and 0.10 levels, respectively.

Table 2: Excess Returns in response to the Court Decision (April 30, 2013)

		Median Net Worth				p33 Net Worth			
		N	All Firms (1)	Low Net Worth (2)	High Net Worth (3)	N	All Firms (4)	Low Net Worth (5)	High Net Worth (6)
Bonds Excess Returns	Treated Firms	88	0.212**	0.234***	0.0271	61	0.243**	0.275**	0.0869**
	T-stat		(2.55)	(2.75)	(0.82)		(2.12)	(2.27)	(2.56)
Stocks Excess Returns	Treated Firms	38	-0.810%	-0.810%	0.209%	25	-1.217%	-1.217%	0.007%
	Control Firms	99	0.151%	0.345%	0.017%	112	0.130%	0.543%	0.162%
	Difference		-0.961%*	-1.155%	0.1924%		-1.347%**	-1.76%	-0.155%
	T-stat		(-1.86)	(-1.56)	(0.3)		(-2.27)	(-1.67)	(-0.3)
Change in Firm Value	Treated Firms	30	0.142**	0.142**	0.013	22	0.146*	0.151*	0.014
	T-stat		(2.27)	(2.28)	(0.46)		(1.76)	(1.79)	(0.53)

Note: The table reports the results of an event study examining the market reaction to the court ruling in the IDB case on April 30, 2013. Excess returns (ER) refer to bonds issued by treated firms only and are estimated relative to a portfolio of similar bonds (in terms of rating, indexation and maturity, as in Bessembinder et al. 2009) issued by firms in the control sample. The standard errors are clustered at the firm level, as some firms have multiple bonds.

ER for stocks are estimated based on the calculation of Scholes-Williams beta relative to the Tel Aviv Stock Exchange's main index (Tel Aviv 100 at the time, currently Tel Aviv 125) using daily data for eleven months (month -12 to month -1) prior to the court ruling. Because ER are calculated relative to a general index, this measure is available for firms in both the treated and control groups.

The change in firm value is available for treated firms only because bond ER are not available for control group firms. The change in firm value is defined as the sum of the bond ER multiplied by the firm's previous day market value of debt (for firms with multiple bonds we sum up the product of excess bond return and bond capitalization of all bonds), plus stock ER multiplied by the firm's previous day market capitalization. The result is scaled by the previous day's combined market value of the firm's stocks and bonds.

Treated firms are firms that do not have covenants and whose net worth over assets in 2012 is below the sample median (33rd percentile). Columns (1) and (4) include the full sample. Columns (2)-(3) and (5)-(6) include sub-samples of firms that are below/above the sample median (33rd percentile) of net worth over assets. The treated firms are the same in columns (1) and (2), as well as in (4) and (5), which differ only in the control group. ***, **, and *, indicate significance levels of 0.01, 0.05, and 0.10, respectively for the difference between the treated firms' ER and the control firms' ER and the T-tests are reported in parentheses.

Table 3: Difference in Differences in Net Worth

Panel A: Scaled Net Worth

	Dependent Variable: Net Worth					
	Median Net Worth			p33 Net Worth		
	All Firms	Low Net Worth	High Net Worth	All Firms	Low Net Worth	High Net Worth
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treated	0.0334*	0.0505*	-0.000101	0.0451*	0.0792**	-0.00591
	(0.0193)	(0.0266)	(0.0179)	(0.0251)	(0.0387)	(0.0139)
SIZE	0.0450	0.134	-0.0724*	0.0445	0.206*	-0.0707**
	(0.0566)	(0.0838)	(0.0421)	(0.0563)	(0.104)	(0.0303)
ROA	0.202	0.435***	0.204	0.198	0.377**	0.227
	(0.177)	(0.164)	(0.140)	(0.175)	(0.186)	(0.140)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
N	651	318	333	651	203	448
adj. R^2	0.060	0.182	0.080	0.063	0.274	0.106

Panel B: Negative Net Worth

	Dependent Variable: Negative Net Worth					
	Median Net Worth			p33 Net Worth		
	All Firms	Low Net Worth	High Net Worth	All Firms	Low Net Worth	High Net Worth
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treated	-0.0665**	-0.0926**	0.0122	-0.0868*	-0.145**	0.00929
	(0.0312)	(0.0437)	(0.0123)	(0.0445)	(0.0707)	(0.00934)
SIZE	-0.0269	-0.0385	-0.0103	-0.0260	-0.0450	-0.00746
	(0.0668)	(0.115)	(0.0107)	(0.0660)	(0.157)	(0.00772)
ROA	-0.0235	-0.166	0.0155	-0.0160	-0.166	0.0110
	(0.0630)	(0.107)	(0.0179)	(0.0583)	(0.138)	(0.0128)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
N	651	318	333	651	203	448
adj. R^2	0.024	0.030	0.000	0.031	0.041	0.000

Note: The table reports the results of difference-in-differences regressions explaining the changes in firms' net worth due to the court ruling. In Panel A, the dependent variable is Net Worth calculated as total assets net of total liabilities, scaled by total assets (values below -1 or above +1 are omitted). In Panel B, the dependent variable is Negative Net Worth, a dummy variable equal to one if net worth is negative, and zero otherwise. Treated firms are firms that do not have covenants and whose net worth over assets in 2012 is below the sample median (33rd percentile). *Post* is an Indicator variable that takes the value one for the three years after the court ruling (2013-2015) and zero for the prior year (2012). *Size* (log of assets) and *ROA* (operating profits over assets) are both winsorized at the 1st and 99th percentiles. Columns (1) and (4) include the full sample. Columns (2)-(3) and ((5)-(6) include sub-samples of firms that are below/above the sample median (33rd percentile) of net worth over assets in 2012. The treated firms are the same in columns (1) and (2), as well as (4) and (5), which differ only in the control group. Firm and year fixed effects are included throughout. Robust standard errors clustered by firm are reported in parentheses. ***, **, and *, indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Table 4: Shares Issuances

	Dependent Variable: New Shares Issued Scaled by Total Assets					
	Median Net Worth			p33 Net Worth		
	All Firms	Low Net Worth	High Net Worth	All Firms	Low Net Worth	High Net Worth
	(1)	(2)	(3)	(4)	(5)	(6)
Post*treated	0.0178***	0.0163***	-0.00321	0.0203**	0.0178**	-0.000551
	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.005)
SIZE	-0.00884	0.00351	-0.0252	-0.00910	0.00853	-0.0210*
	(0.011)	(0.015)	(0.015)	(0.011)	(0.019)	(0.012)
ROA	-0.0467	-0.0512	-0.0358*	-0.0477	-0.0413	-0.0409*
	(0.030)	(0.076)	(0.020)	(0.030)	(0.091)	(0.021)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
N	651	318	333	651	203	448
adj. R^2	0.046	0.035	0.076	0.046	0.045	0.058

Note: The table reports the results of a difference-in-differences regressions where the dependent variable is the equity capital raised (in seasoned public offerings), scaled by total assets. Treated firms are firms that do not have covenants and whose net worth over assets in 2012 is below the sample median (33rd percentile). *Post* is an indicator variable that takes the value one for the three years after the court ruling (2013-2015) and zero for the prior year (2012); *Size* (log of assets) and *ROA* (operating profits over assets) are both winsorized at the 1st and 99th percentiles. Columns (1) and (4) include the full sample. Columns (2)-(3) and ((5)-(6) include sub-samples of firms that are below/above the sample median (33rd percentile) of net worth over assets in 2012. The treated firms are the same in columns (1) and (2), as well as (4) and (5), which differ only in the control group. Firm and year fixed effects are included throughout. Robust standard errors clustered by firms are reported in parentheses. ***, **, and *, indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Table 5: Discretionary Long-Term Accruals

	Dependent Variable: Discretionary Long-Term Accruals (DLA)					
	Median Net Worth			P33 Net Worth		
	All Firms	Low Net Worth	High Net Worth	All Firms	Low Net Worth	High Net Worth
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treated	0.0192*	0.0277**	0.0157	0.0316**	0.0438**	0.0120
	(0.0103)	(0.0109)	(0.0122)	(0.0152)	(0.0166)	(0.00850)
SIZE	0.00812	0.0129	-0.00296	0.00671	0.0391**	-0.00752
	(0.01000)	(0.0142)	(0.0148)	(0.00955)	(0.0173)	(0.00859)
ROA	0.0585	0.0537	0.0590	0.0513	0.0217	0.0559
	(0.0485)	(0.142)	(0.0472)	(0.0468)	(0.230)	(0.0401)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
N	521	263	258	521	148	373
adj. R-sq	0.130	0.138	0.134	0.147	0.180	0.150

Note: The table reports the results of difference-in-differences regressions explaining changes in firms' Discretionary Long-Term Accruals (DLA) due to the court ruling. The dependent variable, DLA, is the discretionary accruals in long term assets, where discretionary and non-discretionary total accruals and short-term accruals are calculated using the cross-sectional modified Jones model Dechow et al. (1995) and are used to extrapolate the long-term discretionary accruals following Teoh et al. (1998); Appendix B provides further details. Treated firms are firms that do not have covenants and whose net worth over assets in 2012 is below the sample median (33rd percentile). *Post* is an Indicator variable that takes the value one for the three years after the court ruling (2013-2015) and zero for the prior year (2012). *Size* (log of assets) and *ROA* (operating profits over assets) are both winsorized at the 1st and 99th percentiles. Columns (1) and (4) include the full sample. Columns (2)-(3) and ((5)-(6) include sub-samples of firms that are below/above the sample median (33rd percentile) net worth in 2012. The treated firms are the same in columns (1) and (2), as well as (4) and (5), which differ only in the control group. Firm and year fixed effects are included throughout. Robust standard errors clustered by firms are reported in parentheses. ***, **, and *, indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Table 6: Timely Loss Recognition
Conservatism Test following Ball and Shivakumar (2005)

	Dependent Variable: ΔNet_Income_t					
	Median Net Worth			p33 Net Worth		
	All Firms	Low Net Worth	High Net Worth	All Firms	Low Net Worth	High Net Worth
	(1)	(2)	(3)	(4)	(5)	(6)
$Neg\_\Delta Net\_Income_{t-1}$	-0.00590	-0.0145	0.00552	-0.00713	-0.00937	0.00578
	(0.00981)	(0.0193)	(0.0118)	(0.00956)	(0.0263)	(0.0104)
ΔNet_Income_{t-1}	0.200	0.486	0.167	0.190	0.634	0.128
	(0.210)	(0.530)	(0.248)	(0.204)	(0.634)	(0.216)
$Neg\_\Delta Net\_Income_{t-1}$*$\Delta Net\_Income_{t-1}$	-1.288***	-2.646***	-0.237	-1.291***	-3.025***	-0.538
	(0.454)	(0.940)	(0.603)	(0.430)	(1.109)	(0.499)
Post*Treated	0.0115	0.00114	-0.0292	0.00384	0.00110	-0.0173
	(0.0287)	(0.0326)	(0.0268)	(0.0413)	(0.0478)	(0.0221)
$Neg\_\Delta Net\_Income_{t-1}$*Post*Treated	0.0114	0.0193	-0.0090	0.0229	0.0248	-0.0154
	(0.0261)	(0.0353)	(0.0331)	(0.0338)	(0.0514)	(0.0287)
ΔNet_Income_{t-1}*Post*Treated	-0.433	-0.448	-0.809	-0.402	-0.539	-0.847
	(0.316)	(0.559)	(0.793)	(0.326)	(0.656)	(0.752)
ΔNet_Income_{t-1}*$Neg\_\Delta Net\_Income_{t-1}$	0.802*	1.776**	0.475	0.799**	2.065**	0.757
*Post*Treated	(0.409)	(0.715)	(0.955)	(0.398)	(0.843)	(0.854)
SIZE	0.0857**	0.132**	0.0734	0.0853**	0.188***	0.0330
	(0.0393)	(0.0523)	(0.0460)	(0.0390)	(0.0700)	(0.0318)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
N	617	314	303	617	199	418
adj. R^2	0.249	0.370	0.114	0.250	0.392	0.116

Note: The table reports results of a regression model that estimates changes in timely loss recognition after the court ruling, following Ball and Shivakumar (2005). The dependent variable ΔNet_Income_t is the change in total income from fiscal year *t-1* to *t*, scaled by year *t-1* book value of total assets. Treated firms are firms that do not have covenants and whose net worth over assets in 2012 is below the sample median (33rd percentile). *Post* is an Indicator variable that takes the value one for the three years after the court ruling (2013-2015) and zero for the prior year (2012). $Neg\_\Delta Net\_Income_{t-1}$ is a dummy variable that takes the value one if ΔNet_Income_{t-1} is negative, and zero otherwise; ΔNet_Income_{t-1} is the lag of ΔNet_Income_t; *Size* is the natural log of assets. Columns (1) and (4) include the full sample. Columns (2)-(3) and ((5)-(6) include sub-samples of firms that are below/above the sample median (33rd percentile) of net worth over assets in 2012. The treated firms are the same in columns (1) and (2), as well as (4) and (5), which differ only in the control group. Firm and year fixed effects are included throughout. Continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by firms are reported in parentheses. ***, **, and *, indicate significance levels of 0.01, 0.05, and 0.10, respectively.

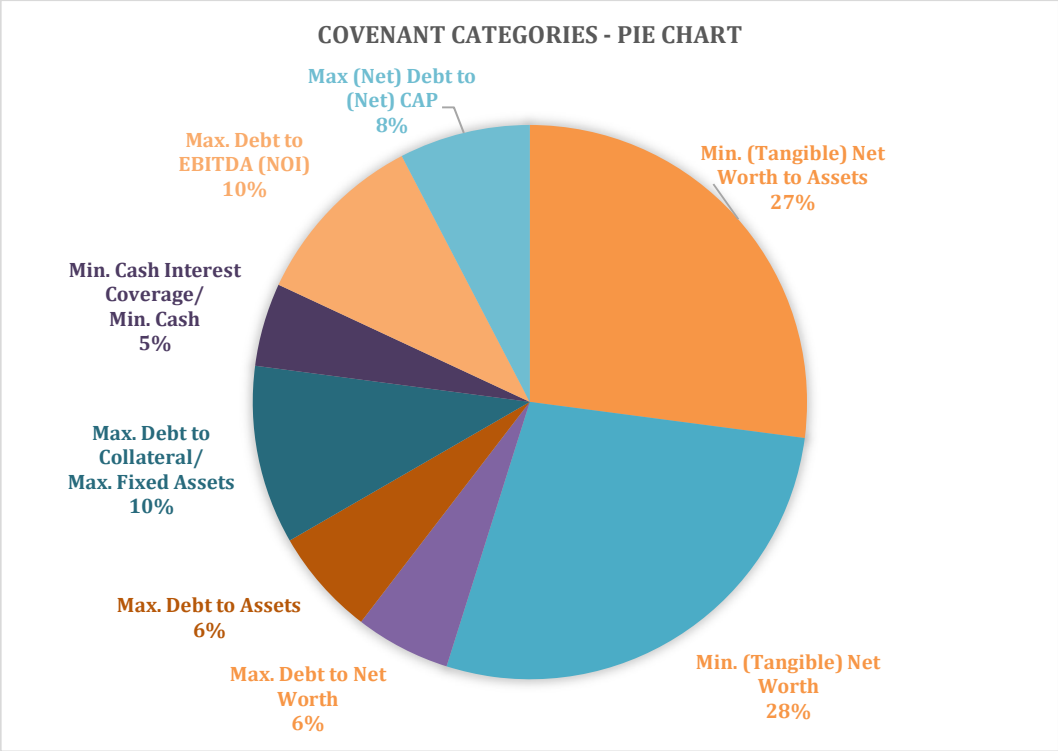
Table 7: Informativeness using Bond Abnormal Returns

	Dependent Variable: Abnormal Bond Returns (-1,+1)					
	Median Net Worth			p33 Net Worth		
	All Firms	Low Net Worth	High Net Worth	All Firms	Low Net Worth	High Net Worth
	(1)	(2)	(3)	(4)	(5)	(6)
ΔNet_Income_t	-0.00209	-0.0217*	0.00372	-0.00183	-0.0354***	0.00184
	(0.00167)	(0.0115)	(0.0135)	(0.00158)	(0.00920)	(0.0101)
ΔNet_Income_t*Treat	0.00638	0.0259*	-0.00402	0.00426	0.0370***	-0.00184
	(0.00821)	(0.0140)	(0.0135)	(0.00905)	(0.0130)	(0.0101)
Post*Treat	-0.000385	-0.000418	0.000119	-0.000809*	-0.000664	0.000150
	(0.000342)	(0.000401)	(0.000447)	(0.000443)	(0.000506)	(0.000301)
ΔNet_Income_t* Post	0.00155	0.0211*	-0.00384	0.00105	0.0354***	-0.00246
	(0.00188)	(0.0117)	(0.0132)	(0.00177)	(0.00953)	(0.00975)
ΔNet_Income_t*Post*Treat	-0.00631	-0.0256*	0.00453	-0.00391	-0.0377***	0.00240
	(0.00812)	(0.0142)	(0.0132)	(0.00889)	(0.0130)	(0.00986)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
N	456	244	212	456	157	299
adj. R^2	0.007	0.021	0.015	0.022	0.076	0.006

Note: The table reports the results of a regression model that estimates changes to informativeness by measuring the bond response to earnings announcements. The dependent variable, Abnormal Bond Return, is the value-weighted firm average of cumulative abnormal bond returns in a three-day window around the earnings announcement date (to account for the possibility of multiple bond series issued by a single firm). Abnormal Bond Return is calculated using the matching portfolio model of Bessembinder et al. (2009); Appendix B provides further details. ΔNet_Income_t is the change in total income from fiscal year *t-1* to *t*, scaled by year *t-1* book value of total assets; Treated firms are firms that do not have covenants and whose net worth over assets in 2012 is below the sample median (33rd percentile); *Post* is an Indicator variable that takes the value one for the three years after the court ruling (2013-2015) and zero for the prior year (2012). Columns (1) and (4) include the full sample. Columns (2)-(3) and ((5)-(6) include sub-samples of firms that are below/above the sample median (33rd percentile) of net worth over assets in 2012. The treated firms are the same in columns (1) and (2), as well as (4) and (5), which differ only in the control group. Firm and year fixed effects are included throughout. Continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by firms are reported in parentheses. ***, **, and *, indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Appendix A: Financial Covenants in Debt Contracts of Firms in the Control Group

The chart below describes the types of financial covenants and their prevalence in debt contracts of the control group in 2012, prior to the court ruling. The table below provides additional information on each covenant type. Credit downgrade covenants (not included in the chart) account for 26% of covenants in the control group (our results are robust to the exclusion of control firms that had only credit downgrade covenants).



Covenant	Explanation
Min. (Tangible) Net Worth to Assets	Min. ratio of book net worth, or tangible net worth, to book assets.
Min. (Tangible) Net Worth	Min. monetary value of net worth, tangible net worth or net worth excluding capital reserves.
Max. Debt to EBITDA (NOI)	Max. ratio of debt to EBITDA, or debt to net operating income (NOI).
Max. Debt to Assets	Max. ratio of debt to assets.
Max. Debt to Collateral/ Max. Fixed Assets	Max. ratio of debt to collateral including limitations on debt to non-collateral assets.
Min. Cash Interest Coverage/ Min. Cash	Min. ratio of cash interest coverage or minimum monetary amount of cash.
Max. Debt to Net Worth	Max. ratio of debt to net worth, or debt to the equity stake (net worth) of controlling shareholders (alternatively, min. ratio of equity to debt).

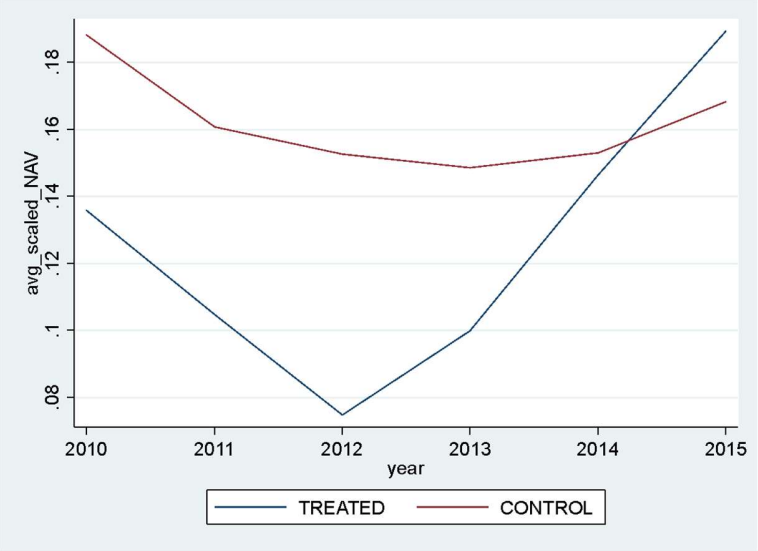
Appendix B: Variable Definitions

Variable	
$Treated_i$	Indicator variable that takes the value one for companies whose bonds did not include financial covenants at the time of the court decision and their net worth was below the sample median (or 33rd percentile) as of 2012.
$Post_t$	Indicator variable that takes the value one for the three years after the court ruling (2013-2015) and zero for the prior year (2012).
Net_Worth_{it}	Total assets net of total liabilities, of firm *i* in year *t,* scaled by total assets.
$Negative\_Net\_Worth_{it}$	Indicator variable that takes the value one if Net_Worth_{it} is negative, and zero otherwise
$SIZE_{it}$	Natural logarithm of total assets (measured in thousands of NIS, about 4 NIS equal 1 USD).
ROA_{it}	Operating profit deflated by total assets
ΔNet_Income_t	The change in the total income from fiscal year t-1 to t, scaled by year t-1 book value of total assets.
$Neg\_\Delta Net\_Income_{t-1}$	A dummy variable that takes the value one if ΔNet_Income_t is negative, and zero otherwise.
$Pos\_\_\Delta Net\_Income_t$	A dummy variable that takes the value one if ΔNet_Income_t is positive and zero otherwise.
DLA	Discretionary Long-term Accruals (DLA) are calculated in a similar manner to Teoh et al. (1998), where discretionary and non-discretionary total accruals are calculated using the cross sectional modified Jones model (Dechow et al., 1995) and are decomposed into short-term and long-term discretionary and non-discretionary components. Total accrual is the difference between net income and cash flow from operation: $AC = Net\ Income - Cash\ Flows\ from\ Operation$ Current accruals are defined as the change in noncash current assets minus the change in operating current liabilities: $CA = \Delta Current\ Assets - \Delta Cash\ and\ Cash\ Equivalent$ $- (\Delta Current\ Liabilities - \Delta Short\ Term\ Debt)$ Non-discretionary accruals are expected accruals from a cross-sectional modified Jones model and the discretionary variables are the residuals. Expected current accruals for a firm in a given year are estimated from a cross-sectional regression in that year of current accruals on the change in sales using an estimation sample of all companies within the same industry. Thus, for the expected current accruals of firm *i* in year *t*, we run the following cross-sectional OLS regression: $\frac{CA_{j,t}}{TA_{j,t-1}} = \alpha_0 \left(\frac{1}{TA_{j,t-1}}\right) + \alpha_1 \left(\frac{\Delta Sales_{j,t} - \Delta AR_{j,t}}{TA_{j,t-1}}\right) + E_{j,t}$

	where ΔSales is the change in sales, ΔAR is the change in accounts receivables and TA is total assets. Non-discretionary current accruals are calculated as: $$NDCA_{i,t} = \hat{\alpha}_0\left(\frac{1}{TA_{i,t-1}}\right) + \hat{\alpha}_1\left(\frac{\Delta Sales_{j,t} - \Delta AR_{j,t}}{TA_{j,t-1}}\right)$$ where $\widehat{\alpha_0}$ is the estimated intercept and $\widehat{\alpha_1}$ is the slope coefficient for firm *i* in year *t.* Discretionary current accruals, DCA for firm *i* in year *t* are represented by the residual: $$DCA_{i,t} = \frac{CA_{j,t}}{TA_{j,t-1}} - NDCA_{i,t}$$ To obtain discretionary and non-discretionary long-term accruals, discretionary and non-discretionary total accruals are first estimated. The discretionary total accrual, DAC for firm *i* for year t is calculated in a similar manner as for current accruals except that now total accruals AC is used as the dependent variable and the regression includes gross property, plant, and equipment (PPE) as an additional explanatory variable: $$\frac{AC_{j,t}}{TA_{j,t-1}} = b_0\left(\frac{1}{TA_{j,t-1}}\right) + b_1\left(\frac{\Delta Sales_{j,t} - \Delta AR_{j,t}}{TA_{j,t-1}}\right) + b_2\left(\frac{\Delta PPE_{j,t}}{TA_{j,t-1}}\right) + E_{j,t}$$ Non-discretionary total accruals or NDTAC are calculated as: $$NDTAC_{i,t} = \hat{b}_0\left(\frac{1}{TA_{i,t-1}}\right) + \hat{b}_1\left(\frac{\Delta Sales_{j,t} - \Delta AR_{j,t}}{TA_{j,t-1}}\right) + \hat{b}_2\left(\frac{\Delta PPE_{i,t}}{TA_{i,t-1}}\right)$$ $\hat{b}_0$ is the estimated intercept, and $\hat{b}_1$ and $\hat{b}_2$ are the estimated slope coefficients from previews regression. Thus, non-discretionary long-term accrual is the difference between nondiscretionary total accrual and non-discretionary current accrual. $$NDLA = NDTAC - NDCA$$ Finally, discretionary long-term accrual is the difference between assets-scaled long-term accruals and non-discretionary long-term accruals. $$DLA = \frac{AC_{i,t}}{TA_{i,t-1}} - NDLA$$
$Abnormal\ Bond\ Return\ (-1+1)_{it}$	Value-weighted average of cumulative abnormal bond returns in a three-day window around the earnings announcement date (day zero). Abnormal bond returns are calculated separately for each bond series using the matching portfolio model of Bessembinder et al. (2009). As in Bessembinder et al. (2009), twelve matching portfolios are created by classifying bonds into six major rating categories (AA- or above, A+, A, A-, between BBB+ to BB, below BB), and then segmenting each of these categories into intermediate and long-term indices based on time to maturity (below three years, or equal and above three years).

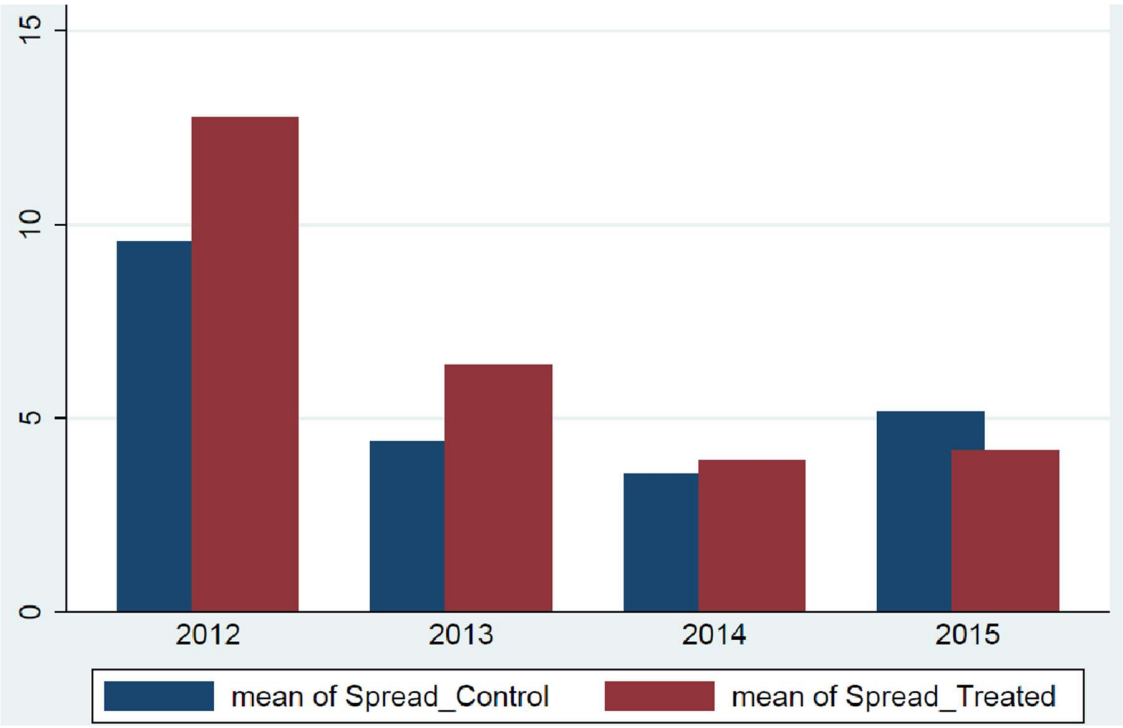
	In addition to matching by rating and maturity we also match on indexation since some of the bonds in Israel are linked to the CPI index. Thus, we split each of the 12 portfolios into index linked and non-index linked, resulting in 24 portfolios. The 24 indices' daily returns are value-weighted and used as the expected bond return (EBR) for a matched bond in our sample. The abnormal bond return (ABR) for bond *b* of firm *i* at day *d* is calculated as the difference between the bond return (BR) and the expected return of the matched portfolio, excluding bond b: $$ABR_{bid} = BR_{bid} - EBR_{pd}$$ We than add up the three-day abnormal return of bond *b* of firm *i*, around the earnings announcement day *t*: $$CABR(-1+1)_{bit} = \sum_{d=-1}^{1} ABR_{bid}$$ We calculate the firm-level Abnormal Bond Return as the weighted average of all bonds issued by firm *i* that were traded around the earnings announcement date *t*: $$Abnormal\ Bond\ Return\ (-1+1)_{it} = \sum_{b=1}^{n} W_b * CABR(-1+1)_{bit}$$

Appendix C: Net Worth in Treated and Control Firms before and after 2013
(Low Net Worth Firms only)



The figure describes the average net worth for low net worth (below the sample median) treated and control firms. Net worth is calculated as total assets net of total liabilities, scaled by total assets (values below -1 or above +1 are omitted). Treated firms do not have covenants and their net worth over assets in 2012 is below the sample median. The subset of control firms included in this figure are firms with financial covenants whose net worth over assets in 2012 is also below the sample median.

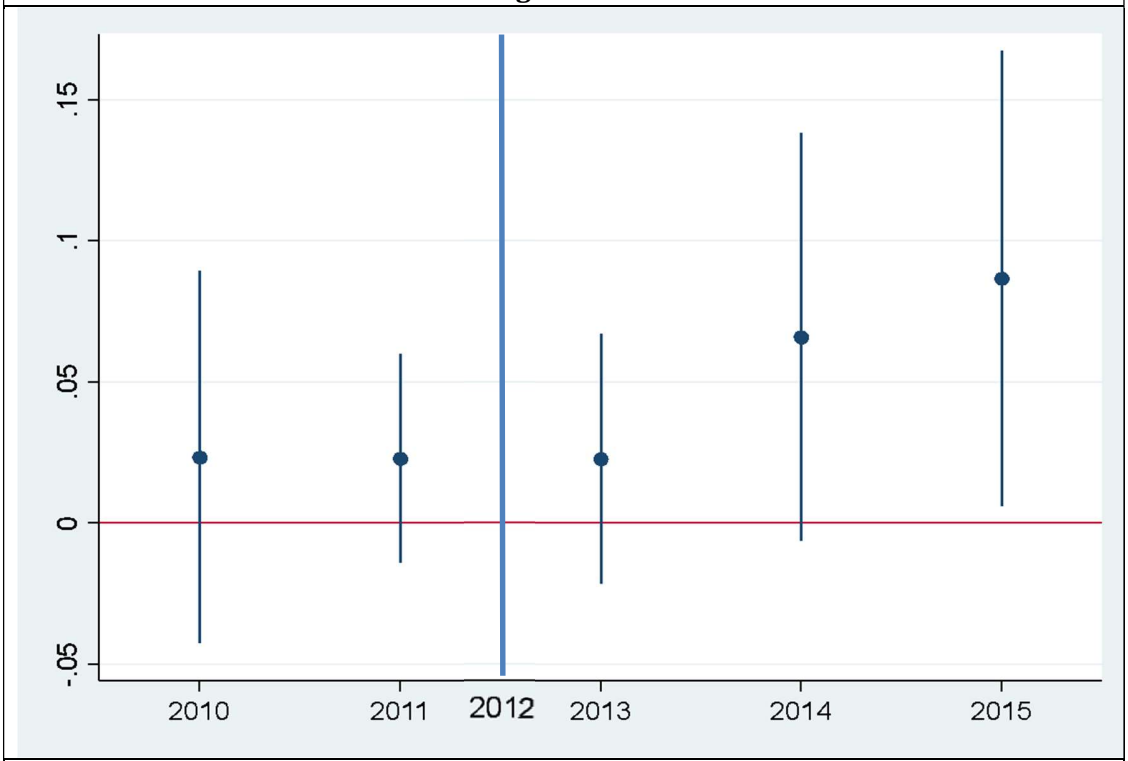
Appendix D: Average Bond Yield Spreads for Treated and Control Firms
(above government bonds of similar maturity and indexation)



The figure describes the annual (simple) average bond yield spread in treated firms (red bars) vs. control firms (blue bars). For firms with multiple bond series, we use the value-weighted average of the daily bond spreads of all the bond series. Spreads are measured relative to government bonds with similar maturity and indexation (drawn from the Bank of Israel). The daily spreads are winsorized at the 1st and 99th percentiles. Treated firms are firms that do not have covenants and whose net worth over assets in 2012 is below the sample median. Control firms are firms that do not have financial covenants but whose net worth over assets in 2012 is above the sample median, or firms that have financial covenants, regardless of their net worth.

Appendix E: Graphical Presentation of Regression Coefficients

Net Worth Regression Coefficients



Note: this figure presents a different version of the regression in Table 3A, Col. 1. As in Table 3A, the dependent variable is Net Worth (scaled by total assets), but, instead of the Treated*Post interaction term, we use year-specific treatment indicators (e.g., TREATED*2014). Treated firms do not have covenants and their net worth over assets in 2012 is below the sample median. As in Table 3A, the regression includes controls for *Size* (log of assets) and *ROA* (operating profits over assets), as well as firm and year fixed effects. 2012 constitutes the base year (omitted). The figure displays the coefficients and 95% confidence intervals for the interaction terms between a dummy denoting treated firms and each year. Standard errors are clustered by firm.

Appendix F: The C-Score Measure of Conservatism (Khan and Watts, 2009; Tan, 2013)

	Dependent Variable: C_SCORE					
	Median Net Worth			p33 Net Worth		
	All Firms	Low Net Worth	High Net Worth	All Firms	Low Net Worth	High Net Worth
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treat	-0.00067*	-0.00022	0.00012	-0.00091	-0.00035	0.00011
	(0.0004)	(0.0004)	(0.0002)	(0.0006)	(0.0008)	(0.0002)
SIZE	0.00083*	0.00099	0.0007	0.00085*	0.00103	0.00070*
	(0.0005)	(0.0009)	(0.0005)	(0.0005)	(0.0014)	(0.0004)
ROA	-0.00306**	-0.00648	-0.00206*	-0.00297**	-0.00686	-0.00230**
	(0.0013)	(0.0047)	(0.0011)	(0.0013)	(0.0077)	(0.0010)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.0109*	-0.0129	-0.00937	-0.0112*	-0.0133	-0.00927*
	(0.0065)	(0.0124)	(0.0064)	(0.0067)	(0.0188)	(0.0053)
N	529	242	287	529	151	378
R^2	0.281	0.273	0.368	0.282	0.277	0.396

Note: The table reports the results of difference-in-differences regression estimates of conservatism after the court ruling. The dependent variable, *C_SCORE*, is a firm-year conservatism measure, based on Khan and Watts (2009) and Tan (2013). It is estimated by first running the following cross-sectional model:

$$\begin{aligned} ScaledEarnings_i &= \beta_0 + \beta_1 NegReturn_i + Return_i(\mu_1 + \mu_2 SIZEMV_i + \mu_3 MB_i + \mu_4 Leverage_i) \\ &+ NegReturn_i * Return_i(\lambda_1 + \lambda_2 SIZEMV_i + \lambda_3 MB_i + \lambda_4 Leverage) \\ &+ (\delta_1 SIZEMV_i + \delta_2 MB_i + \delta_3 Leverage_i + \delta_4 NegReturn_i SIZEMV_i \\ &+ \delta_5 NegReturn_i MB_i + \delta_6 NegReturn_i Leverage_i) \end{aligned}$$

$ScaledEarnings_i$ is earnings scaled by the lagged market value of equity; $Return_i$ is the annual buy and hold stock return of firm i; $NegReturn_i$ is a dummy variable that takes the value one if $Return_i$ is negative, and zero otherwise; $SIZEMV_i$ is the natural log of market value of equity of firm i; MB_i is market value of equity divided by book value of equity; and $Leverage_i$ is the leverage ratio of firm i (short term and long term debt/market value of equity). The estimated coefficients are then used to construct a firm-year specific *C_SCORE*, which is calculated as: $C\_SCORE_i = \hat{\lambda}_1 + \hat{\lambda}_2 SIZEMV_i + \hat{\lambda}_3 MB_i + \hat{\lambda}_4 Leverage_i$

Treated firms are firms that do not have covenants and whose net worth over assets in 2012 is below the sample median (33rd percentile). *Post* is an Indicator variable that takes the value one for the three years after the court ruling (2013-2015) and zero for the prior year (2012). *Size* is the natural log of assets. Columns (1) and (4) include the full sample. Columns (2)-(3) and ((5)-(6) include sub-samples of firms that are below/above the sample median (33rd percentile) of net worth over assets in 2012. The treated firms are the same in columns (1) and (2), as well as (4) and (5), which differ only in the control group. Firm and year fixed effects are included throughout. All Continuous variables that are used to construct the C_SCORE, and the control variables in the final regressions are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by firms are reported in parentheses. ***, **, and *, indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Appendix G: Illustrations of Practices Increasing Net Worth

Increased long-term discretionary accruals and reduced conservatism may manifest in various ways. In our sample, over 70% of the treated firms have registered revenues from at least one of the following four accounting practices in the post-court ruling period, possibly to inflate their net worth: Registering revenues from fair value asset appraisals/revaluations (27 treated firms, over half of all treated firms); registering revenues from business combinations by revaluating the assets and liabilities of a subsidiary/associate company (13 treated firms, a quarter of all treated firms); registering revenues from the early adoption of new accounting standards (six treated firms); and registering revenues from the cancelation of impairment provisions (four treated firms). A detailed illustration of the use of these methods appears below. This is not designed to prove that treated firms have used these methods more frequently or more aggressively than firms in the control group, but rather to show some real-world examples of practices which might affect discretionary long-term accruals or accounting conservatism.

G.1 Fair Value Appraisals of Investment Property under IFRS (used by 27 treated firms)

As noted above, IFRS enables the use of managerial discretion in accounting reports. For example, the use of fair value accounting, whereby companies regularly (in each reporting period) adjust the value of certain items in their financial statements (e.g., investment property; financial assets and liabilities), opens the door to aggressive revaluations that increase the company's net income and net worth, as changes in the fair value estimates are recognized as a profit/loss when they occur. Dietrich, Harris and Muller (2000) find that UK firms exploit the fair value of investment property to report higher earnings and smooth (reported) net asset changes. Chen, Gavious and Steinberg (2019) find that Israeli firms exploit fair value accounting to increase dividends to the detriment of bondholders. Moreover, an audit report published by ISA with regard to the practice of revaluation of investment property by public companies sheds light on the ways in which firms manipulate their fair value appraisals: Most investment properties are evaluated using the Discounted Cash Flow (DCF) method; ISA points out the possibility of manipulations

both in estimating higher projected income (e.g., rent) from the asset (the numerator in the DCF model), as well as in using a lower discount rate (the denominator in the DCF model). To illustrate, a real estate company in our sample changed the method used to value its property from the DCF approach, combined with a comparison to similar assets before 2013, to the (easier to manipulate?) DCF method only after the court ruling. We also observe firms inflating the cash flow projections (the numerator) associated with their property in comparison with prior years, as well as firms lowering the discount rates (the denominator) used in estimations prior to the court ruling. Although these changes in the estimation methods and assumptions could conceivably reflect the company's actual expectations regarding the cash flows and risk of the asset, they highlight the relative ease of changing the underlying assumptions and of recording optimistic appraisals of investment assets.

G.2 Revenues from Business Combinations (used by 13 treated firms)

When control of a business is obtained, its value is recorded according to the acquisition method, whereby the assets acquired and the liabilities assumed are measured at fair value. Importantly, the fair value measurement is applied when control is achieved, or when it ceases to exist, regardless of whether non-controlling shares in the acquired company were held by the acquirer prior to the date of achieving control, or whether non-controlling shares in the acquired company remain with the acquirer after the date of formally losing control.[35] Therefore, a firm can potentially buy/sell a relatively small equity stake in an existing associate/subsidiary, leading to a revaluation of the assets and liabilities of the investee so as to record any resultant gain in the profit and loss statement, increasing the acquiring company's net worth. To illustrate the use of this mechanism, one of the treated companies in our sample applied this method to record a transitory profit as follows: at the beginning of 2013, it held 36.7% of the outstanding shares of an

[35] See International Financial Reporting Standard No. 3 (IFRS 3) regarding the accounting for business acquisition, including business combination, achieved in stages, as well as International Accounting Standard No. 28 (IAS 28) regarding the accounting for investment in associates.

affiliated company, giving it "effective control"[36] and therefore the financial statements were consolidated. During the second quarter of 2013 (following the court ruling), the company sold 3.6% of its holdings in the affiliate, remaining with an equity stake of 33.1% and claiming that its effective control had ceased to exist. This change led to the revaluation of the investment in the affiliate at fair value and a profit of 686 million NIS (about $200 million) was recorded as a result.

G.3 Additional Mechanisms

In addition to the practices described above, two treated real estate companies in our sample chose to adopt IFRS 15 before it became mandatory. This enabled them to include future profits from current contracts with customers earlier than these sources of income would have been recorded otherwise, resulting in a significant increase in the profitability and net worth of the two companies. Another company registered an income of NIS 14 million from the cancelation of past impairments of land and inventory. The firm explained the cancelation of impairment by a reduction in the uncertainty surrounding one project and by a revised external appraisal of another real estate project.

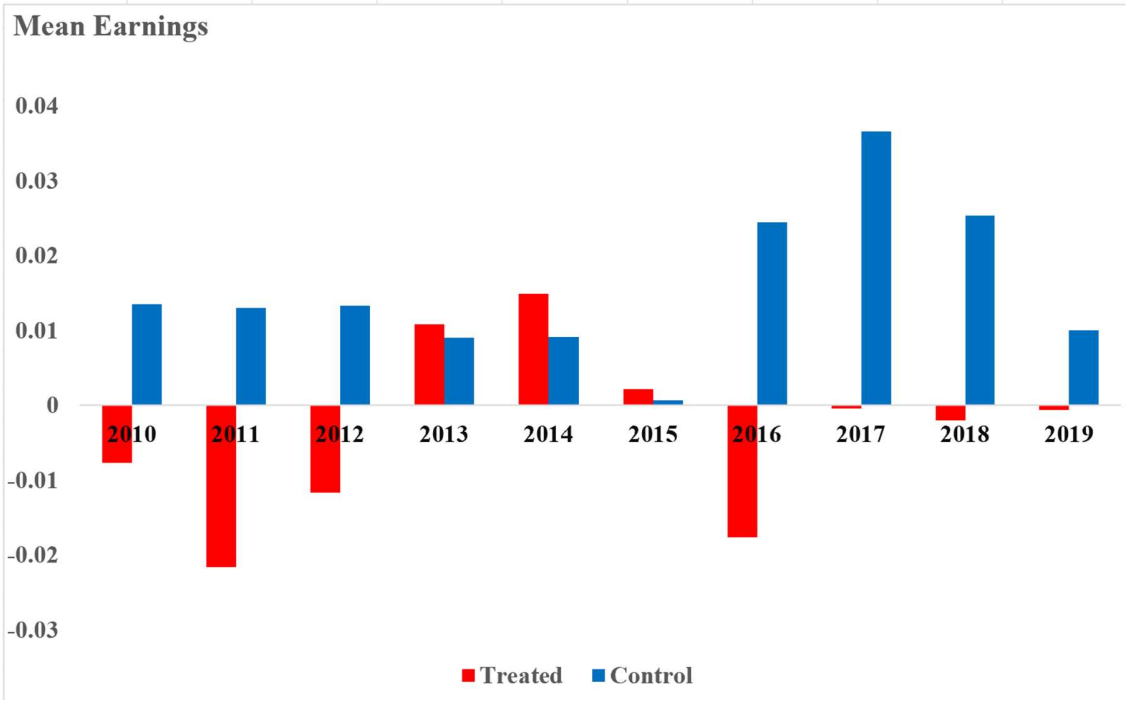
How long-lasting are the effects of these accounting practices? Appendix Figure G3 suggests that the post-2013 rise in earnings of treated firms took place immediately following the court ruling, with earnings rising sharply (relative to the control group) in 2013 and 2014, but reversing their trend and declining afterwards. This is consistent with the view that the ability to overstate earnings (in various ways) is limited to a certain time period, as companies are subject to some form of an inter-temporal balance sheet constraint (Barton and Simko, 2002).[37] Similar arguments have been made in the context

[36] Note, that IFRS and US GAAP define control differently. Under IFRS there is more discretion in the determination of control (which incorporates the concepts of effective control and substantive potential voting rights) that may lead to divergent accounting results relative to US GAAP. See pp. 1-2 in PWC's guide for business combinations https://www.pwc.nl/nl/audit-assurance/assets/documents/pwc-guide-business-combinations-noncontrolling-interests.pdf.

[37] Barton and Simko's (2002) analysis is carried out in the context of US GAAP rather than IFRS. Somewhat related is Hirshleifer et al. (2004) who discuss the impact of "bloated" balance sheets on stock prices. They also show (in Panel A of their Figure 1, p. 313) that earnings cannot remain "bloated" forever.

of the well-documented decline in firm profitability following an IPO (e.g., Jain and Kini, 1994), which some studies attribute to “window dressing” practices (although other explanations are, of course, also possible).

Figure G3: Time Trends in Mean Earnings for Treated and Control Firms



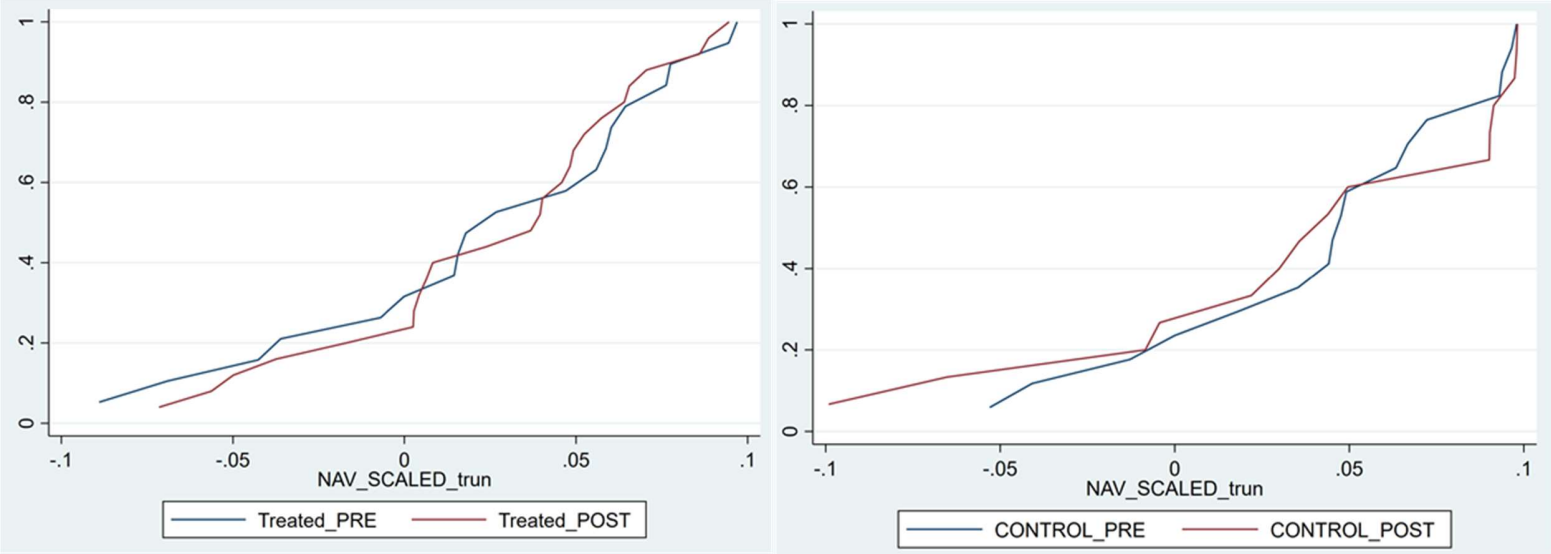
The figure presents the time series of average earnings for the treated (red bars) and control (blue bars) firms. Mean earnings are the simple average of net income, normalized by total assets within each group (treated /control firms). Treated firms are firms with no covenants and whose net worth is below the sample median. The earnings are winsorized at the 1st and 99th percentiles.

Appendix H: Propensity Score Matching Results corresponding to the Results in Tables 3A, 3B and 5, Columns (1) – (3)

	Dependent Variable: Net Worth			Dependent Variable: Neg. Net Worth			Dependent Variable: DLA		
		Median Net Worth			Median Net Worth			Median Net Worth	
	All Firms	Low Net Worth	High Net Worth	All Firms	Low Net Worth	High Net Worth	All Firms	Low Net Worth	High Net Worth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treat	0.0335	0.0508*	-0.0214	-0.0657*	-0.0781*	0.0220	0.0150	0.0246**	0.0177
	(0.0235)	(0.0268)	(0.0234)	(0.0343)	(0.0454)	(0.0221)	(0.0104)	(0.0104)	(0.0117)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
N	381	254	238	381	254	238	330	240	213
Adj R^2	0.035	0.011	-0.001	0.028	0.020	0.010	0.148	0.179	0.053

Note: The table reports the results of difference in differences regressions explaining changes in net worth, negative net worth, and discretionary long-term accruals (DLA) following the court ruling, corresponding to the results of Columns (1)-(3) in Tables 2A, 2B and 3, respectively. Unlike the original tables, the estimation here is based on matching treated firms to control firms on the basis of *Size* (log of assets) and *ROA* (operating profits over assets), both winsorized at the 1st and 99th percentiles, as well as industry. Matching is based on the "nearest neighbor propensity score matching" without replacement, hence the number of observations is smaller than in the original tables. Columns (1) through (3) correspond to the same columns in Table 2A; Columns (4) through (6) correspond to Columns (1) through (3) in Table 2B; and Columns (7) through (9) correspond to Columns (1) through (3) in Table 3. Treated firms are firms that do not have covenants and whose net worth over assets in 2012 is below the sample median. *Post* is an indicator variable that takes the value one for the three years after the court ruling (2013-2015) and zero for the prior year (2012). In Columns (1)-(3), the dependent variable is Net Worth, calculated as total assets net of total liabilities, scaled by total assets (values below -1 or above +1 are omitted). In columns (4)-(6), the dependent variable is Negative Net Worth, a dummy variable which takes the value one if Net Worth is negative, and zero otherwise. In columns (7)-(9) the dependent variable discretionary long-term accruals, DLA, calculated as in Teoh et al. (1998), where discretionary and non-discretionary total accruals are calculated using the cross-sectional Jones (1991) model and are decomposed into short-term and long-term discretionary and non-discretionary components; Appendix B provides further details. Columns (1), (4), (7) include the full sample. Columns (2)-(3), ((5)-(6) and (8)-(9) include sub-samples of firms that are below/above the sample median of net worth over assets in 2012. The treated firms are the same in columns (1) and (2), as well as (4) and (5), which differ only in the control group. Firm and year fixed effects are included throughout. Robust standard errors clustered by firms are reported in parentheses. ***, **, and *, indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Appendix J: Falsification/Placebo Test (using 2012 as the change year instead of 2013)



The figure is similar to Figure 1 except that, instead of examining changes around the court decision year, 2013, it presents changes in net worth around an arbitrary ("placebo") year, 2012. The calculation is based on the cumulative distribution functions for treated and control firms in 2011 and 2012. We separately compute, and show in the figure, four cumulative net worth distribution functions in the scaled net worth range (-0.1, 0.1) for the treatment and control groups for 2011 (in blue) and 2012 (in red). Treated firms are firms with no covenants and net worth below the sample median.

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