Reporting Regulation and Private Firms' Bank Credit*

Antonio Moreta†

October 1, 2023

Click here for the latest version

Abstract

This paper studies the effect of reporting regulation on private firms' bank credit, an overlooked aspect in the literature. I exploit the unique features of the Spanish institutional setting to extract quasi-exogenous variation in reporting regulation while setting fixed the effects of the auditing regulation. Using a fuzzy regression discontinuity design, as well as a difference-in-difference approach, I find that private firms subject to incremental reporting regulation have more bank credit. Empirical evidence is consistent with this result being driven by an increase in banking competition. Additional analyses suggest that reporting regulation results in a substitution between bank credit and alternative financing sources.

Keywords: private firms; disclosure; regulation; bank credit

JEL Codes: K22, L51, M41, M48, G32, G38

*I greatly appreciate the guidance and support of my advisors Juan Manuel García Lara, Beatriz García Osma, and David Martinez-Miera. I wish to thank Edoardo Maria Acabbi, Alejandro Casado, Ilia Dichev, Marco Giommetti, Paulo Maduro, Gonzalo Maturana, Pablo Ruiz, Jesper Rudiger, and Anna Toldrá for their valuable comments and suggestions. I thank the financial support from the Spanish Ministry of Science, Innovation, and Universities (PRE2020-093224) and from the Spanish Agencia Estatal de Investigación (PID2019-111143GB-C32/AEI/10.13039/501100011033). Usual disclaimer applies.

†Department of Business Administration, UC3M, Spain. Contact: amoreta@emp.uc3m.es

1 Introduction

Despite the imposition of financial statement requirements (hereafter reporting regulation) on millions of firms worldwide, their desirability remains a subject of debate.[1] The low rates of voluntary disclosures suggest that the individual costs of expanded reporting perceived by firms outweigh the corresponding benefits (Bernard, 2016; Lisowsky and Minnis, 2020). However, the positive externalities from collective public disclosure may provide a justification for the regulation of financial statements (Minnis and Shroff, 2017). The limited causal evidence regarding the economic consequences of reporting regulation constrains the ability to inform this debate (Leuz and Wysocki, 2016). This is particularly the case when evaluating the impact on credit markets, given that prior research focus mostly on equity markets.[2] The main contribution of this paper is to examine how plausible exogenous variation in reporting regulation affects private firms' bank credit.

There are varying ex-ante predictions regarding the impact of reporting regulation on firms' bank credit. Empirical evidence limits to documenting that reporting regulation reduces information asymmetries between incumbent and competing banks, promoting banking competition and shifting a relational for a transactional type of bank lending (Breuer, Hombach and Müller, 2018). This shift can result in either a positive or negative effect on firms' bank credit. On the one hand, economic theory predicts that an increase in banking competition leads to greater loan volume in equilibrium by shifting the loan supply curve to the right.[3] On the other hand, a transactional type of bank lending may result in the loss of mutual relationship benefits, restricting the availability of bank credit (Petersen and Rajan, 1995). Thus, the effect of reporting regulation on firms' bank credit is ultimately an empirical question.

[1] For example, in the US and Canada, only a minority of firms (i.e., public firms) are required to publicly disclose their financial statements. By contrast, in the European Union (EU) most private firms are mandated to disclose at least some form of abbreviated financial statements.

[2] See for instance Bushee and Leuz (2005); Greenstone, Oyer and Vissing-Jorgensen (2006); La Porta, Lopez-de Silanes and Shleifer (2006); Christensen, Hail and Leuz (2016a); Aghamolla and Thakor (2022).

[3] This result is a central part of theoretical research on the effects of banking competition on financial stability (e.g., Allen and Gale (2004); Boyd and De Nicolo (2005); Martinez-Miera and Repullo (2010)).

Evaluating the impact of reporting regulation on private firms' bank credit is challenging for several reasons. First, the firms' disclosure decision is endogenous. Firms' disclosures are not randomly assigned and are likely correlated with unobserved determinants of bank credit. For example, the disclosure decision may be correlated with an unobserved aspect of firm quality that also affects the firms' bank credit, potentially introducing a bias. Second, reporting regulation is typically bundled together with auditing regulation (Bernard, Burgstahler and Kaya, 2018). This implies that the regulation requires not only the public disclosure of financial statements but also the auditing of these financial statements, preventing the separate identification of their individual effects. Third, firm-level data on private firms is frequently unavailable because some countries impose essentially no public disclosure requirements on privately held firms (e.g., the United States).

I address these challenges by exploiting the unique features of the Spanish institutional framework. In Spain, all private limited liability firms are required by law to publicly disclose their financial statements, enabling me to collect firm-level data. Further, I enrich this data with confidential information from the Central Credit Registry of Spain, which mitigates measurement error issues present in firms' balance sheet bank debt (Duro, López-Espinosa, Mayordomo, Ormazabal and Rodríguez-Moreno, 2022). Importantly, the legislation establishes differential reporting requirements on a subset of firms subject to mandatory financial statement audits. The requirements are conditional on whether these firms exceed two out of three size-based thresholds related to total assets, sales, and employees. Firms just below the thresholds, labeled as "small" firms, must only publicly disclose an abbreviated balance sheet, brief notes, and an abbreviated income statement. In contrast, firms just above the thresholds, labeled as "medium" firms, are required to publicly disclose a detailed balance sheet, extended notes, and an abbreviated income statement. By focusing on a comparable sample of firms in the vicinity of the thresholds, I can extract quasi-exogenous variation in reporting regulation while keeping constant the effect of the audit mandates. The regulation causes medium firms to be 14 percentage points more likely to publicly disclose more detailed financial statements, allowing me to isolate the causal impact of reporting regulation using a fuzzy regression discontinuity design.

I find that private firms with incremental reporting regulation have more bank credit, measured as bank credit over total assets. This result is robust to multiple empirical specifications, including the use of alternative bandwidths, kernels, functional forms, clusters, and the inclusion of controls for firm characteristics, and industry-year and region-year fixed effects. My estimates reveal an economically large impact of reporting regulation on bank credit. In particular, medium firms affected by the regulation have bank credit to assets ratios 0.29 larger than otherwise similar small firms. Since the average total assets in my sample are around 6 million euros, this result implies 1.74 million euros more in bank credit.

The main identification assumption of my RDD is local continuity, that is, the conditional expectation function of bank credit would be continuous around the thresholds in the absence of differential reporting requirements. I perform multiple tests to verify the internal validity of my methodology. First, Bernard et al. (2018) raise the concern that firms can manipulate their size to fall below the thresholds in order to avoid the expanded reports and mandated audits. My setting complements the work by Bernard et al. (2018) as I can disentangle if manipulation is driven by the reporting or the auditing regulation. I find strong support for size manipulation around the auditing thresholds, while I do not observe similar patterns around the reporting thresholds. Second, I show that small and medium firms are similar around the thresholds across multiple firm characteristics. Third, I consider different placebo tests to falsify any spurious link between reporting regulation and bank credit (e.g., other policies sharing the same thresholds). Importantly, I conduct the RDD specification during a placebo period capitalizing on a regulatory threshold change. Using the thresholds from the main analysis during this placebo period yields insignificant results. Similarly, this setting allows me to implement a difference-in-differences design as an alternative to RDD to demonstrate the robustness of my results. These findings are consistent with the differential reporting requirements having a causal link on bank credit.

I present additional evidence on the potential mechanism behind the main result. Based on recent empirical research, I argue that firms' bank credit can increase due to the greater banking competition that results from reporting regulation reducing information asymmetries with prospective banks, rather than incumbent banks (e.g., Breuer et al. (2018)). Alternatively, reporting regulation could reduce information asymmetries between the firm and *all* banks, with no impact on banking competition. This could also result in greater firms' bank credit due to lower credit rationing (Stiglitz and Weiss, 1981). I find that firms affected by the regulation contract with more banks, similar to Breuer et al. (2018). I also document that the results are stronger in ex-ante more contestable banking markets. Both results are consistent with the findings from recent empirical evidence (Breuer et al., 2018) and suggest that increased banking competition is a primary mechanism driving the main results.

The documented positive impact of reporting regulation on firms' bank credit questions why firms do not voluntarily disclose their financial statements to the public.[4] To shed light on this question, I examine if the effect on firms' bank credit translates into firms' real outcomes. My results suggest that firms affected by the regulation substitute alternative balance sheet liabilities for bank credit, while I do not find significant effects on other funding sources such as equity capital or cash holdings. This substitution effect is not driven by trade credit.[5] Overall, I do not find that reporting regulation leads to significant effects on the *net* financing capital of the firm, which consequently has no impact on the firm's total assets.

In short, I document a positive effect of reporting regulation on private firms' bank credit plausibly explained by an increase in banking competition. However, this positive effect on bank credit does not lead to a significant impact on the net financing capital of the firm, as my evidence points to a substitution between bank credit and alternative balance sheet liabilities. Consequently, I do not observe discernible differences in the total assets of firms

[4]Existing literature provides evidence of firm-level costs that could outweigh the positive bank credit effect (e.g., Dedman and Lennox (2009); Bernard (2016); Breuer, Leuz and Vanhaverbeke (2019)), yielding a potential explanation of why firms do not provide voluntary reports.

[5]Due to the limited granularity of my database, I am unable to conduct further analyses to identify which specific liabilities contribute to this substitution effect.

subject to incremental reporting regulation.

This paper relates to several strands of the literature. First, I contribute to the fundamental debate surrounding the regulation of financial statements. Although the effects of mandatory financial reports on public firms have been extensively examined by previous research (see Leuz and Wysocki (2016) and Roychowdhury, Shroff and Verdi (2019) for reviews),[6] our understanding of the effects of mandatory financial statement requirements on smaller private firms remains limited (Minnis and Sutherland, 2017).[7] In particular, research on credit markets limits to documenting a transition from a relational to a transactional type of lending (Breuer et al., 2018), without providing further insights into its specific impact on firms' bank credit. I address this gap by showing that reporting regulation has a positive effect on private firms' bank credit and by exploring the subsequent firm-level implications stemming from this positive effect on bank credit.

Second, I contribute to the accounting literature by exploiting a novel institutional framework that is well-suited to establish causal relationships. My paper responds to recent calls emphasizing both the scarcity and importance of causal inferences when informing policymakers about reporting regulations (Leuz and Wysocki, 2016; Leuz, 2018). The increasing use of quasi-experimental methods in accounting research highlights not only the significance of natural experiments but also the need for appropriate institutional settings to draw causal inferences (Armstrong, Kepler, Samuels and Taylor, 2022). In this sense, my research design offers a combination of the methods with the institutional framework, bringing forth several appealing features. First, since I observe actual reporting outcomes (i.e., the level of detail of financial statements), I provide evidence throughout the entire causal path, that is, from

[6]For instance, these effects encompass positive stock returns and sustained liquidity increases (Bushee and Leuz, 2005; Greenstone et al., 2006), better firm and peer firms investment efficiency (Badertscher, Shroff and White, 2013; Cheng, Dhaliwal and Zhang, 2013; Shroff, Verdi and Yu, 2014; Durnev and Mangen, 2020), lower firm and peer firms cost of capital (Lambert, Leuz and Verrecchia, 2007; Baginski and Hinson, 2016; Shroff, Verdi and Yost, 2017), increased import competition (Yang, 2019; Glaeser and Omartian, 2022), or distributional effects on corporate innovation (Kim and Valentine, 2021).

[7]A new and emerging literature studies the effects of financial reporting regulation on private firms in EU countries (e.g., Bernard (2016); Breuer et al. (2018); Breuer (2021)). These countries mandate all limited liability private firms to publicly disclose at least some form of abbreviated financial statements, thus enabling researchers to collect firm-level financial data.

reporting regulation to reporting outcomes to economic effects, increasing the reliability of my findings (Gow, Larcker and Reiss, 2016; Leuz and Wysocki, 2016). Second, the institutional setting provides a high enforcement rate, low voluntary disclosures, a bank-dependent economy, and separate auditing thresholds, offering an ideal context to examine the research question. Finally, my setting allows me to triangulate my inferences using different methods (e.g., RDD or DiD), specifications (e.g., placebo tests or regression specifications), and replicating the results from prior evidence (e.g., from Breuer et al. (2018)) (Hail, Lang and Leuz, 2020; Leuz, 2022; Armstrong et al., 2022).

Finally, my study relates to the literature on the role of accounting information in the debt contracting process (e.g., Bharath, Sunder and Sunder (2008); Armstrong, Guay and Weber (2010); Christensen, Nikolaev and Wittenberg-Moerman (2016b)). More specifically, I contribute to the understanding of how financial statements influence the access of small and medium-sized enterprises (SMEs) to credit markets (Allee and Yohn, 2009; Minnis, 2011; Berger, Minnis and Sutherland, 2017; Donelson, Jennings and Mcinnis, 2017; Minnis and Sutherland, 2017; Breuer et al., 2018; Lisowsky and Minnis, 2020; Duro et al., 2022; Minnis, Sutherland and Vetter, 2023). My evidence suggests that imposing greater transparency regulation on these informational opaque firms can yield important consequences in the banking industry, subsequently influencing bank credit. Future research could explore the impact of reporting regulation on the financial system to offer a more complete framework of the consequences of such regulations.

The remainder of the paper proceeds as follows. Section 2 presents the theoretical background. Section 3 and 4 describe the Spanish institutional framework and the data. Section 5 presents the empirical strategy and discusses its validity. Section 6 and 7 present the main result of the paper and multiple robustness tests. Section 8 provides evidence about the potential mechanism and real effects. Section 9 concludes.

2 Literature Review and Hypothesis Development

2.1 Literature Review

The optimal regulation of financial statements remains an open question. Early work by Coase (1960) and Stigler (1964) considers the hypothesis that the optimal government policy is to leave capital markets unregulated. Under this policy, firms have market-based incentives to voluntarily disclose their financial statements because they ultimately bear the agency costs with capital providers (e.g., Jensen and Meckling (1976)). In contrast, an alternative view holds that capital markets do not prosper if left to market forces alone. Previous evidence links larger capital markets to extensive disclosure requirements and liability standards that facilitate the recovery of losses by investors (La Porta, Lopez-de Silanes, Shleifer and Vishny, 1997; La Porta et al., 2006). Private firms' revealed preferences suggest that the costs of disclosure are higher than the associated benefits since very few opt to voluntarily disclose their financial statements in the absence of a reporting mandate (Bernard, 2016; Lisowsky and Minnis, 2020). However, firms are unlikely to incorporate in their disclosure decisions the externalities their financial statements have on other market agents. If these externalities outweigh the net unilateral disclosure costs, reporting regulation can potentially improve the welfare of the economy (Minnis and Shroff, 2017).

The growing literature on reporting regulation provides evidence of individual costs and benefits but it fails to ascertain the net aggregate effect. On the one hand, a firm's mandatory reporting has significant economic benefits for related firms. Several studies link reporting regulation to better information environments. For example, US studies document that a greater industry concentration of public firms is associated with better investment decisions of firms operating within the industry (Badertscher et al., 2013), and a lower cost of capital of private firms in that industry (Shroff et al., 2017).[8] Bushee and Leuz (2005) suggest that

[8] In the US, the SEC requires public firms to file annual reports on form 10-K, quarterly reports on form 10-Q and certain ongoing events on form 8-K. By contrast, private firms are subject to essentially no reporting regulation.

more stringent reporting mandates generate liquidity spillovers. Similarly, Bernard, Kaya and Wertz (2021) find a significant increase in capital structure mimicking in concentrated markets when incumbent firms publicly disclose their financial statements. On the other hand, the literature highlights the adverse effects of reporting regulation, particularly the proprietary costs resulting from revealing sensitive information to competitors (e.g., Dedman and Lennox (2009); Bernard (2016); Bernard et al. (2018); Breuer et al. (2019)). Other negative effects include the loss of information conveyed in the firms' revealed preferences for disclosure (Kausar, Shroff and White, 2016), the crowding out of unregulated peer firms' voluntary disclosures (Admati and Pfleiderer, 2000; Baginski and Hinson, 2016; Breuer, Hombach and Müller, 2022) and the direct costs related to the production and dissemination of the financial statements.[9]

Banks are one of the primary users of financial statements. Therefore, it is plausible that reporting regulation has an impact on banks' capital allocation decisions. Diamond (1984) suggests that banks have a competitive advantage as delegated monitors of borrowers due to their superior ability to obtain information. Financial statements play a crucial role in gathering information for both the screening (Demiroglu, Ozbas, Silva and Ulu, 2021) and monitoring (Carrizosa and Ryan, 2017; Minnis and Sutherland, 2017) of borrowers. Survey evidence from Minnis and Shroff (2017) further supports that lenders are the primary users of financial statements.[10] This is not surprising given that the main accounting regulatory bodies gear financial statements to be relevant in the decision-making process of creditors and investors. Yet, prior research mainly focuses on the effects of reporting regulation on equity markets, thereby neglecting the impact on debt markets (e.g., Bushee and Leuz (2005); Greenstone et al. (2006); La Porta et al. (2006); Christensen et al. (2016a); Baik, Berfeld and Verdi (2021); Aghamolla and Thakor (2022)).

[9]See Leuz and Wysocki (2016) and Minnis and Shroff (2017) for a further discussion of the costs and benefits of financial reporting regulation.

[10]Among two samples of 1477 surveyed firms and 25 surveyed standard setters, 69.4%, and 84.0%, respectively, state that lenders and creditors definitely download and view their public financial statements.

2.2 Hypothesis Development

The impact of reporting regulation on firms' access to bank credit is not obvious. On the one hand, firms' public reporting can reduce information asymmetries between incumbent and prospective banks, encouraging banking competition (Dell'Ariccia, Friedman and Marquez, 1999; Breuer et al., 2018). As a result, banks operating in more competitive markets may extend more credit (e.g., Carlson, Correia and Luck (2022)). Similarly, the informational loss of incumbent banks may prevent them from holding up the borrower, resulting in more favorable financing terms (e.g., Sharpe (1990); Hale and Santos (2009)). On the other hand, reporting mandates lowers banks' incentives to engage in a relationship type of financing because the public dissemination of the financial statements reveals (part of) their borrowers' private information. Thus, reporting regulation can undermine the mutual benefits that stem from relationship lending, potentially constraining access to bank credit (e.g., Petersen and Rajan (1995); Bharath, Dahiya, Saunders and Srinivasan (2011)).

There exist alternative predictions regarding the effect of reporting regulation on bank credit. First, reporting regulation may simply have no effect on firms' bank credit. Banks may have access to alternative private channels to satisfy their information demands. These private channels may provide "soft" private information such as the quality of the management team and future investment plans; or other "hard" information which may be of greater detail and substitute the information contained in the financial statements (e.g., Minnis et al. (2023)). For example, banks may collect alternative information from transaction accounts (Mester, Nakamura and Renault, 2007), credit registries (Sutherland, 2018), aging reports (Frankel, Kim, Ma and Martin, 2020), or site visits and third-party appraisals (Gustafson, Ivanov and Meisenzahl, 2021). However, these alternative private information channels may complement (rather than substitute) the public information from financial statements by providing a more comprehensive information mosaic of the firm (Cheynel and Levine, 2020). Likewise, reporting regulation may not matter if all regulated firms already privately share their financial statements with all competing banks.

Another alternative prediction holds that reporting regulation mitigates information asymmetries between borrowers and all banks (both incumbent and competing banks). Under this hypothesis, the regulation primarily mitigates information frictions in credit markets (Stiglitz and Weiss, 1981), rather than increasing competition within the banking industry. Thus, the regulation would alleviate credit rationing.

Collectively, the combined findings from both theoretical and empirical research provide varying ex-ante predictions regarding the impact of reporting regulation on the bank credit of firms. Therefore, this question is left to empirical evidence.

Importantly, firms' changes in bank credit can lead to significant downstream implications regarding the firms' capital structure and real economic outcomes. On the one hand, firms could offset the change in bank credit by either increasing or decreasing the usage of alternative external and internal financing sources (Myers, 1984; Acharya, Almeida and Campello, 2007; Frank and Goyal, 2008; Almeida and Campello, 2010), with no further economic impact on the firms' economic activity. On the other hand, ample research points to the real economic consequences of changes in firms' bank credit (Banerjee and Duflo, 2014; Chodorow-Reich, 2014; Alfaro, García-Santana and Moral-Benito, 2021).

3 Institutional Background

Following EU accounting directives, Spanish law imposes sized-based regulatory reporting and auditing requirements for all private limited liability firms. The regulation includes small- and medium-sized private firms that typically rely on banks as their primary source of capital given their limited access to public capital markets. For these otherwise publicly opaque firms, mandated financial statements serve as the primary source of firm-specific public information. While the requirements apply to all private limited liability firms, medium-sized firms are obliged to disclose a greater amount of public information relative to small firms.

To simplify the accounting obligations of small firms, in May 2017 the Spanish government increased the reporting size-based regulatory thresholds to be classified as a medium-sized firm.[11] Importantly, the law specifies different size-based regulatory thresholds for reporting and auditing requirements, which were previously bundled. The effective date of implementation applies to fiscal years from 2016 (included). This occurs because the majority of firms deposit their financial statements in the Commercial Registry approximately seven months after the fiscal year-end. For fiscal years initiated in January 2016 or later, financial statements are typically filed in the corresponding local Commercial Registry no earlier than July 2017, that is, after the passage of the law.[12]

Table 1 describes the reporting and auditing sized-based regulatory thresholds and the corresponding requirements for fiscal years starting in 2016 and later. Panel A shows that a firm is classified as small if it falls below at least two of the following thresholds for two consecutive years: (i) total assets equal to 4 million euros, (ii) sales equal to 8 million euros, or (iii) employees equal to 50. Similarly, firms are classified as medium if they are not classified as small and they fall below at least two of the following thresholds for two consecutive years: (i) total assets equal to 11.4 million euros, (ii) sales equal to 22.8 million euros, or (iii) employees equal to 250. Firms are classified as big otherwise. Panel B shows the requirements to undergo mandated financial statement audits. Note that the auditing regulatory thresholds are lower along the assets-based and sales-based thresholds compared to the reporting regulatory thresholds of medium firms, while they both share the same employee-based threshold.

[**Table 1 here**]

[11] The specific passage of the law can be found at: https://www.boe.es/buscar/doc.php?id=BOE-A-2017-5775.

[12] After the fiscal year-end, firms have a three-month deadline to produce their annual financial statements, a four-month deadline to legalize the accounting books and a six-month deadline to obtain the approval at the General Shareholders' Meeting. The deposit of the financial statements must be done the month immediately after the General Shareholders' Meeting. As a result, most firms end up depositing their financial statements in July (or seven months after the fiscal year-end). Although I am aware that some firms whose natural year coincides with the fiscal year may have deposited their financial statements earlier (before the passage of the law), evidence shows that most firms deposit their financial reports in July. In any case, this would lead to attenuation bias in the main specification, preventing the identification of any effect of the regulation.

Table 1 further summarizes the public information requirements of small, medium, and big firms. Small firms must publicly disclose at least an "abbreviated" model, which includes an abbreviated balance sheet, an abbreviated income statement, and brief notes. Medium firms must provide at least a "mixed" model, which contains a detailed balance sheet and detailed notes, and an abbreviated income statement. An abbreviated balance sheet presents major asset and liability categories (e.g., inventories), while a detailed balance sheet additionally disaggregates these categories into specific individual accounts (e.g., raw materials, work in process, finished goods).[13] For a comparison of an abbreviated and mixed model refer to Figure A1 of the Appendix. Big firms must publish a "detailed" model, which incorporates a detailed balance sheet, income statement, notes, cash-flow statement, and statement of changes in equity. Note that compliance costs associated with stricter regulations are minimal within this framework because European private firms must typically prepare complete accounts for shareholders and tax authorities (Bernard et al., 2018).

Reporting regulation must meet two necessary conditions in order to have any significant impact: (i) the regulation is effectively enforced, and (ii) firms prefer lower rates of voluntary public reporting in the absence of regulatory requirements. Failure to satisfy either of these two necessary conditions would imply that the regulation has minimal (if any) impact since it would not result in a significant change in the public reporting practices of regulated firms. In the former case, firms would simply forego the reporting requirements and opt to withhold firm-specific information. In the later case, the disclosed information would remain unaltered because firms are willing to make it public regardless of the reporting requirements.

Figure 1 presents the distribution of firms publicly disclosing detailed, mixed, and abbreviated models across different size categories, including big, medium, small firms with mandated audits, and small firms without mandated audits. Both necessary conditions are supported. First, the reporting requirements of medium firms are enforced as 90.7% of medium firms in the sample publicly disclose mixed or detailed models. This result com-

[13] Previous research claims that the level of disaggregation of financial statements is a relevant aspect of disclosure quality (Chen, Miao and Shevlin, 2015).

plements the evidence in Duro et al. (2022), who document that 80-85% of Spanish limited liability firms deposit their financial statements (both correct and incorrect models) in the local Commercial Registry since 2009. Note that this number plausibly represents a conservative estimate of "compliers" for the present study because small firms without mandated audits (excluded from the main sample) are the most likely candidates for not depositing their financial statements in the respective registry. Taken together, both results suggest a lower bound of 75% of limited liability firms complying with the reporting regulation during the specified sample period. The high rate of compliance is consistent with the stringent penalties imposed on firms that fail to comply with the regulation.[14]

[**Figure 1 here**]

Second, only 23.6% of the subsample of small firms with mandated audits voluntarily disclose mixed or detailed models. Voluntary reports drop dramatically in the sample of small firms without mandated audits, where only 0.4% report mixed or detailed models. This result indicates that the majority of these firms do not voluntarily disclose additional public information absent a reporting mandate. This evidence is consistent with the low voluntary disclosure rates in the absence of a mandate documented in other studies (e.g., Bernard (2016); Minnis and Shroff (2017); Lisowsky and Minnis (2020)).

Collectively, both findings suggest that the regulation likely shifts the reporting behavior of medium firms. Additionally, they provide descriptive evidence of the discontinuous reporting practices of small and medium audited firms, consistent with the size-based regulatory thresholds inducing quasi-exogenous variation in public reporting.

Finally, Figure 1 reveals another interesting aspect of the reporting behavior of the sample firms. The marginal cost of disclosing the detailed model relative to the mixed model is low.

[14] According to the *Ley de Sociedades de Capital*, failure to comply with the legal obligation to deposit the accounts in the Registry entails fines ranging between 1,200 and 60,000 euros in the case of SMEs, and can reach 300,000 euros for firms with sales greater than six million euros. In addition, the lack of deposit of the annual accounts in the Commercial Registry causes the provisional closure of the registration sheet, and the loss of the benefit of the limitation of liability in relation to the debts contracted after the end of that term.

For example, only 26.5% of medium firms (which are required to disclose at least the mixed model) disclose the mixed model, while 64.2% of medium firms disclose the detailed model. Similarly, small firms with mandated audits tend to skip the mixed model when disclosing voluntary information. Only 5.9% of these firms disclose the mixed model, while 17.7% disclose the detailed model. Accordingly, the reporting requirements imposed on big firms are less likely to have significant effects because a substantial fraction of firms below the thresholds (medium firms) already disclose detailed models (see Section 7.3).[15] Consequently, I focus on the regulatory thresholds that separate medium and small firms.

Overall, this institutional framework is suitable to test the impact of reporting regulation. On the one hand, the evidence suggests that the regulation is enforced and that firms prefer lower rates of voluntary reporting absent the mandates, allowing to identify any bank credit effect of the regulation if it exists. On the other hand, I can compare the outcomes of medium audited firms, with a subsample of small firms with mandated audits, thus keeping constant the effect of the audit mandates. This subsample of small audited firms is also more comparable in terms of size to medium firms because these firms constitute the largest firms within the pool of all small firms.

4 Data

I obtain firm-level administrative data from *Central de Balances Integrada* (CBI), a database provided by the Bank of Spain. Spanish limited liability firms are obliged by law to submit their financial statements to the Commercial Registry on an annual basis. The Bank of Spain collects this information and performs a thorough data digitalization process and multiple cross-checks to ensure the completeness and accuracy of the information disclosed.[16] This

[15]Note that although a 64.2% of medium firms disclose the detailed model, this percentage is even greater for medium firms at the top of the size distribution (i.e., below the big regulatory thresholds) because voluntary disclosures are likely an increasing function of firm size (Breuer et al., 2022).

[16]Specifically, the most valuable assignment is the evaluation of the internal consistency of each individual financial statement. Financial statements that exhibit divergences exceeding 5% in balance sheet items or inconsistencies between total assets, liabilities, and equity are categorized as "inadequate" for statistical

database provides a superior coverage of Spanish firms than other relevant databases of European private firms (e.g., Amadeus, SABI) (Almunia, Lopez Rodriguez and Moral-Benito, 2018). Importantly, the database includes information regarding the financial statement model disclosed by each firm (i.e., abbreviated, mixed, and detailed models). This allows me to provide evidence throughout the entire causal path, that is, from reporting regulation to disclosure outcomes to economic effects, increasing the reliability of my findings. By contrast, most studies estimate a reduced-form approach and directly gauge the economic impact of regulatory changes (Leuz and Wysocki, 2016).

The evidence in Duro et al. (2022) reveals that a substantial number of Spanish private firms underreport their balance sheet debt, leading to measurement error in the potential dependent variable from CBI.[17] To address this issue, I collect confidential loan-level data from the Central Credit Registry of Spain (*Central de Información de Riesgos* or CIR), maintained by the Bank of Spain in its capacity as the primary banking supervisory agency. It contains comprehensive monthly information on the universe of outstanding loans issued to nonfinancial firms by all banks operating in Spain. The CIR includes an extensive number of variables such as a unique identifier for each party involved in a loan (allowing for merging with firm-level administrative data), outstanding loan amount, maturity, and spread, among others. I compute firm-year bank credit by aggregating the outstanding credit amount of all loans of the same firm in December.[18] [19]

I focus my analysis on audited small and medium limited liability firms, within the optimal bandwidth from the thresholds, operating in nonfinancial industries over 2016-2019. While my methodology already accounts for differences in size between treated and control

examination. Further evaluations are conducted to ensure the coherence between the number of employees and wages, and between monetary units within each individual report.

[17]This can result in inconsistent estimates if the measurement error in balance sheet bank debt is correlated with the degree of compliance with the regulation.

[18]In the main specification, I use the outstanding credit amount of asset-based and cash-flow-based loans, the two most common types of loans in Spain (see Ivashina, Laeven and Moral-Benito (2022)). The results remain similar if I use the outstanding bank credit amount of all loan types, suggesting that the main result is driven by asset-based and cash-flow-based loans.

[19]Less than 1% of firms have a fiscal year ending in a month other than December (Banco de España, 2021). This ensures that the data from CBI and CIR are measured at the same point in time.

firms, I further trim firm-years in the top and bottom 5% percentile of the distributions of assets, sales, and employees. This trimming process helps to ensure a more balanced and comparable sample.[20] I also drop from the sample firm-years with missing data of any of the variables included in the main specification.

The final sample includes 3,797 firm-year observations, with 2,140 corresponding to small firms and 1,657 corresponding to medium firms. Table 2 presents the summary statistics of the small and medium sample firms. The main dependent variable ($Credit/Assets$) and all the controls from Panel B are winsorized at the 1% level. Table A1 of the Appendix provides a description of the variables. Panel A shows descriptive evidence of a stark discontinuity in the reporting practices of both types of firms as only 25% of small firms publicly disclose mixed or detailed models, in contrast to the 59.9% of medium firms.[21] Medium firms also have higher bank credit to assets ratios. However, both findings could be the result of inherent size differences in assets, sales, and employees between small and medium firms, and thus, a formal parametric test is required to account for these differences. Consistent with the reporting regulation rule, on average, small firms exceed only one size-based threshold (i.e., the assets-based threshold), whereas medium firms exceed two size-based thresholds (i.e., the assets- and sales-based thresholds). The average small firm distance to the binding threshold ($LDT2$) is -5.34% of the threshold value, and the average medium firm distance to the binding threshold is 4.98%. In the next section, I define in detail the running variable, the least distance to the second threshold ($LDT2$).

[Table 2 here]

[20]To illustrate the intuition of this timing process consider the following two firms: (i) Firm S with assets equal to 3.9 million euros, sales equal to 8.1 million euros, and employees equal to 49, and (ii) Firm M with assets equal to 100 million euros, sales equal to 8.1 million euros and employees equal to 49. These two firms (small and medium, respectively) are questionably comparable because they broadly differ along the assets dimension. Firm size is correlated with numerous firm characteristics and this may result in an unbalanced sample.

[21]These results are different from those in Figure 1 because my final sample focuses on the observations just around the threshold, while Figure 1 displays the results for the whole population of firms in Spain.

5 Empirical Strategy and Internal Validity

5.1 Empirical Strategy

The Spanish institutional framework provides quasirandom variation in the reporting practices of comparable small and medium private firms while setting fixed the effects of the auditing mandate. As previously described, a firm is classified as small if it falls beneath any two of three size-based thresholds for two consecutive years ($Assets$<4M, $Sales$<8M, Emp<50). Small firms are required to disclose at least an abbreviated balance sheet, an abbreviated income statement, and brief notes (i.e., the abbreviated model). In contrast, medium firms are required to disclose at least a detailed balance sheet and extended notes, and an abbreviated income statement (i.e., the mixed model). Importantly, the largest subset of small firms is obliged to undergo financial statement audits. As long as the regulation is enforced to some extent and firms prefer lower voluntary disclosure rates absent any reporting requirements (see Section 3), the likelihood of treatment will discontinuously increase at the size-based thresholds. This allows me to implement a fuzzy regression discontinuity design to estimate the impact of reporting regulation on private firms' bank credit.

I reduce the dimensionality of the regulatory assignment rule by combining the relative distances to the regulatory thresholds of assets, sales, and employees into a single size dimension, labeled as the least distance to the second threshold ($LDT2$).[22] This combined size dimension simplifies the econometric approach by enabling the use of conventional methods from regression discontinuity designs based on a single running variable. For example, it facilitates the identification of an optimal bandwidth, enables graphical representation, and enhances the statistical power by avoiding restrictions on the sample to subsets where each size-based threshold perfectly defines treatment (e.g., Reardon and Robinson (2012)). Specifically, $LDT2$ is defined as the second-highest value among the three size dimensions (i.e., binding dimension) scaled by the respective regulatory thresholds, minus one:

[22]Similar to Breuer et al. (2018).

$$LDT2_{f,t} = 2^{nd}Max\{\frac{Assets_{f,t}}{4}, \frac{Sales_{f,t}}{8}, \frac{Emp_{f,t}}{50}\} - 1 \tag{1}$$

The main identification assumption is local continuity, that is, the conditional expectation function of the outcome variable around the cutoff is continuous in the absence of differential reporting requirements. Assuming local continuity, the fuzzy RD estimator recovers the local average treatment effect (LATE) of greater public disclosure requirements for a firm with a size equal to the binding regulatory threshold. I follow Imbens and Lemieux (2008) and Gelman and Imbens (2019) and employ a local linear regression within a narrow bandwidth of the regulatory thresholds. I also allow for different slopes of the running variable ($LDT2$) at each side of the thresholds. My primary specification is estimated using the following two-stage-least-squares (2SLS) procedure:[23] [24]

$$\begin{aligned} Mixed_{f,t} = \gamma_0 + \gamma_1 Medium_{f,t} + \gamma_2 LDT2_{f,t} + \gamma_3 LDT2_{f,t} * Medium_{f,t} \\ + \alpha f(Size_{f,t}) + \kappa X_{f,t} + \lambda_{i,t} + \eta_{d,t} + \varepsilon_{f,t} \end{aligned} \tag{2}$$

$$\begin{aligned} \frac{Credit_{f,t+1}}{Assets_{f,t}} = \beta_0 + \beta_1 \widehat{Mixed}_{f,t} + \beta_2 LDT2_{f,t} + \beta_3 LDT2_{f,t} * Medium_{f,t} \\ + \mu f(Size_{f,t}) + \tau X_{f,t} + \delta_{i,t} + \theta_{d,t} + \epsilon_{f,t+1} \end{aligned} \tag{3}$$

Where $\frac{Credit_{f,t+1}}{Assets_{f,t}}$ is the ratio of the bank credit of firm f in year $t+1$ to total assets;[25] $Mixed_{f,t}$ is an indicator variable equal to one if the firm publicly discloses the mixed or detailed model, and zero if it discloses the abbreviated model; $\widehat{Mixed}_{f,t}$ corresponds to the

[23] The strong correspondence between fuzzy RDD and instrumental variables (IV) enables to alternatively characterize the main identification assumption as the conventional *exclusion* restriction, that is, being classified as medium only affects bank credit through the disclosure of the mixed or detailed model.

[24] Even though the dependent variable in the first-stage regression ($Mixed$) is binary, I employ a linear probability model. This decision is based on the concern that employing a probit or logit model could potentially yield inconsistent estimates (Angrist and Pischke, 2009).

[25] The dependent variable is forwarded because financial statements of year t are publicly disclosed in the following year $t+1$.

fitted values of $Mixed_{f,t}$ from the first-stage regression; $Medium_{f,t}$ is an indicator variable equal to one if the firm is classified as medium, and zero if it is classified as small; $LDT2_{f,t}$ is the least distance to the second threshold previously defined. $f(Size_{f,t})$ is a control function that includes $LDT1_{f,t}$ and $LDT3_{f,t}$, which are the least distance to the first and third thresholds, respectively; $I^1_{f,t}$ and $I^3_{f,t}$, which are indicator variables equal to one if the firm exceeds at least one or three thresholds, respectively; and the interactions $LDT1_{f,t} * I^1_{f,t}$ and $LDT3_{f,t} * I^3_{f,t}$. These four variables and their respective interactions allow me to compare firms exceeding the same number of thresholds and further account for size differences along the non-binding dimensions. $X_{f,t}$ is a vector of firm-level controls that includes the current ratio (*Liquidity*), cash over total assets ($Cash/Assets$), equity over total assets (*Equity/Assets*), inventories over total assets ($Inventories/Assets$), EBITDA over sales ($EBITDA$), Ohlson score (*O-Score*), the logarithm of firm age (*Log*(*Age*)), the logarithm of the accounts receivables cycle (*Log*(*AR Cycle*)), and the logarithm of the accounts payables cycle (*Log*(*AP Cycle*)). $\delta_{i,t}$ and $\theta_{d,t}$ are industry-year and region-year fixed effects, respectively. Table A1 of the Appendix provides a description of the variables. Note that the inclusion of controls other than the forcing variable is not required to produce consistent estimates in regression discontinuity designs, although it may improve the precision of the estimates (Imbens and Lemieux, 2008) (see Table A2 of the Appendix and Table 6). The coefficient β_1 captures the local average treatment effect of disclosing at least the mixed model on the ratio of bank credit to total assets.

Equations (2) and (3) are estimated using a triangular kernel, which assigns greater weights to observations close to the threshold, as in Dell (2015). An optimal bandwidth of 10.4% is determined following Calonico, Cattaneo and Titiunik (2014). The selection of a larger bandwidth involves a trade-off where precision improves at the expense of introducing additional bias. Standard errors are clustered at the firm level to account for potential covariance among observations drawn from the same firm. The results are robust to different bandwidths, clusters, controls, or kernels.

5.2 Internal Validity

I present additional evidence to support the assumption of local continuity. First, thus far, I assume that whether a firm lies just below or above the thresholds is random. Bernard et al. (2018) raise the concern that firms with high proprietary costs of disclosure (e.g., high-innovation firms) can manage their size with precision to fall below the size-based regulatory thresholds to avoid expanded reports and mandated audits. If this holds true, my findings could be driven by the specific characteristics of these firms, rather than by their differential reporting requirements. While not entirely ruled out, size manipulation seems unlikely in this framework.

Bernard et al. (2018) examine size management by European (including Spanish) private firms over 2003-2012. The reporting and auditing thresholds were bundled in Spain over that period. Figure 2 presents the frequency distributions of Spanish firms along the continuum of total assets, sales, and employees over 2016-2019, when the reporting and auditing requirements were *not* bundled. This evidence complements the work by Bernard et al. (2018) because it allows me to disentangle if size management is driven by the reporting or the auditing requirements. I use the standardized difference statistic for the interval immediately left of the threshold to assess the significance of the discontinuities at the thresholds (Burgstahler and Chuk, 2015). I find that the standardized difference statistic is highly significant for all the auditing thresholds, but only for the employee-based reporting threshold. Given that both regulations share the same employee-based threshold, the discontinuity in the density of the number of employees seems to be driven by the auditing, rather than by the reporting regulation. Further, note that the reporting exemption requires to fall below two of the three thresholds, which implies that if size manipulation is present, it would be more plausible to result in a discontinuity in at least two of the distribution densities.

[**Figure 2 here**]

For completeness, Figure A2 of the Appendix shows the frequency distributions of assets, sales, and employees over 2012-2015, when the reporting and auditing requirements were bundled. I find evidence of size manipulation around all the bundled regulatory thresholds, in line with Bernard et al. (2018). I provide additional evidence to support the non-existence of systematic manipulation to fall below the 2016-2019 reporting thresholds. Figure A3 of the Appendix shows that the distribution of $LDT2$ is smooth around the cutoff and estimates a formal density test of $LDT2$ following Cattaneo, Jansson and Ma (2018) that fails to reject the hypothesis of no manipulation ($t\text{-}stat = -1.18$). In Figure A4 of the Appendix, a McCrary (2008) test does not identify any statistical discontinuity in the density of observations around the threshold (Panel (a); $t\text{-}stat = -0.1$). The result of no manipulation continues to hold when I collapse the data in size bins within the optimal bandwidth and test the existence of a discontinuity in the distribution of the number of observations (Manual McCrary test in Panel (b); $t\text{-}stat = -0.5$). Collectively, this evidence suggests that firms manipulate their size to avoid the auditing, but not the reporting thresholds. This result is consistent with Breuer (2021), who finds that auditing mandates impose a fixed cost on firms, whereas reporting mandates have mixed effects on the efficiency of industry-wide resource allocation.[26]

Second, I further evaluate random assignment into treatment by providing evidence of the balancing of the covariates around the size thresholds. Covariates not affected by the reporting regulation should exhibit a continuous distribution around the threshold. Panel B of Table 2 presents the results of the balancing of the baseline covariates. I find no evidence of significant differences in the mean values of the firm covariates at each side of the cutoff (column (4)), except for the logarithm of the accounts receivables cycle. In columns (5) and (6), I present the results of the RD specification using each covariate as the outcome variable (and excluding it from the set of firm-level controls). The coefficient of $\widehat{Mixed}$ is not statistically significant in each regression. Figure 3 provides a graphical representation

[26] Additionally, the final sample only includes small and medium *audited* firms. This constrains accounting-based size manipulations. Moreover, both real-operations-based and accounting-based manipulations increase the auditors' litigation risk and audit complexity. This results in higher audit fees (Choi, Lee, Park and Sohn, 2022), making size manipulation more costly.

of the RD balance test. It plots the average bin values of each firm-level covariate along the running variable ($LDT2$), residual of controls and fixed effects, similar to Asher and Novosad (2020). The plots also display linear estimations on each side of the cutoff and 90% percent confidence intervals.[27] The results are consistent with those of the main specification. In Figure A5 of the Appendix, I repeat the graphical analysis for the raw value of each covariate (rather than the residual value), and the results continue to hold.

[**Figure 3 here**]

I provide additional evidence on the balancing of small and medium firms by combining all the baseline covariates into a single variable ($\widehat{Credit/Assets}$), based on their ability to predict bank credit to assets, and showing likewise no evidence of a discontinuity at the size threshold in this variable. This test offers a more powered test of the balance of covariates. Specifically, I regress $Credit/Assets$ in $t+1$ on all the covariates from Panel B of Table 2 in t, and then use the coefficients from this regression to predict the bank credit to assets ratio for each observation. The last row of Panel B in Table 2 shows no significant differences in the predicted bank credit to assets ratios of small and medium firms, both in the difference of means and in the RD estimates. Similarly, Figure A6 of the Appendix shows no evidence of a discontinuity in the distribution of ($\widehat{Credit/Assets}$) at the size threshold.

6 Main Results

I begin by showcasing a graphical analysis and subsequently present the outcomes of the 2SLS procedure. Figure 4 plots the share of firms publicly disclosing the mixed or detailed models, and the average bank credit to assets ratio residual of controls and fixed effects in size bins around the cutoff (Figure A7 of the Appendix plots the raw bank credit to assets ratio). A linear fit is generated at each side of the cutoff, with the corresponding 90%

[27] 90% confidence intervals allow me to be more conservative in validating the balancing of the covariates at each side of the cutoff.

confidence intervals. Both graphical representations exhibit a discontinuity in the vicinity of the cutoff point. Under the identification assumption, the observed discontinuity in bank credit to assets is attributed to the causal impact of reporting regulation.

[**Figure 4 here**]

In a fuzzy RDD, crossing the threshold increases the probability of treatment, rather than perfectly defining treatment. In this specific context, it occurs because incentives to reveal public information change discontinuously at the cutoff (e.g., monetary penalties), but these incentives lack the strength to induce all firms to alter their disclosure practices (e.g., firms with high proprietary costs). Table 3 shows evidence of this fact by presenting the first-stage estimates from Equation (2), using different bandwidths, including the 10.4% optimal bandwidth. The coefficient on $Medium$ is about 0.14, that is, medium firms are 14 percentage points more likely to publicly disclose the mixed or detailed model than otherwise similar small firms. This estimate is very robust to alternative bandwidth choices. The effect reported in the first-stage regression is economically important considering that 25% of small firms just below the threshold publicly disclose the mixed or detailed model. This implies an increase of 56% in the probability of disclosing the mixed or detailed model. In addition, the instrument $Medium$ is statistically strong as the first-stage F-statistic is greater than 10, which adheres to the rule of thumb F-statistic from Staiger and Stock (1997) for models involving one endogenous regressor and one instrument.

[**Table 3 here**]

Next, I use the classification of firms as medium as an instrument to examine the effect of reporting regulation on private firms' bank credit. Table 4 presents the second-stage estimates from Equation (3), using the same bandwidths as those in Table 3. The coefficient on $\widehat{Mixed}$, β_1, is approximately 0.29, that is, medium firms affected by the reporting regulation increase their bank credit to assets ratio by 0.29. Again, this estimate is robust to alternative bandwidth choices. This finding reveals an economically large impact of reporting regulation on private firms' bank credit. With an average total assets of approximately 6 million euros in my sample, this result implies an additional 1.74 million euros in bank credit.

[**Table 4 here**]

It is important to note that while RDD has a high degree of internal validity, it has limited external validity because it provides an estimate of the average treatment effect for a specific sample of firms around the cutoff. Fuzzy RDD further limits the sample to *sensitive* firms (Imbens and Angrist, 1994), that is, firms that would shift their reporting practices if they are classified as medium, and hence are sensitive to variation in the instrument. In section B of the Appendix, a simple example is provided to illustrate this point. Note that this implies that the average causal effect is the difference in the limits of the conditional expectation function of bank credit to assets around the cutoff (reduced-form estimate), scaled by the proportion of firms affected by the treatment (1-stage estimate). Table A2 of the Appendix presents the reduced-form results and reveals that the coefficient on $Medium$ is around 0.04. Thus, drawing from the 1-stage and reduced-form estimates, β_1 is approximately $\frac{0.04}{0.14} = 0.29$, which coincides with the results from the second-stage regression.

7 Robustness

7.1 Placebo Tests and Difference-in-Differences

I further validate my fuzzy RDD by conducting different placebo tests. If the exclusion restriction is violated, then being above the threshold (i.e., medium firms) affects bank credit through other channels than through increased public disclosure. A natural candidate for such a channel may include other policies that share the same regulatory thresholds. To the best of my knowledge, there are no other regulations in Spain that employ the same assets- and sales-based thresholds as the reporting regulation. There are, however, a few regulations that share the 50-employee threshold (other than the auditing regulation), mostly related to labor protection and representation.[28] Still, there is limited theoretical justification to suggest that these policies are causally influencing my findings. An additional concern may originate if the combined size dimension $LDT2$ fails to adequately summarize all the relevant aspects related to the size-based assignment rule. In such a scenario, my findings might be driven by firm size rather than by differential reporting requirements. These alternative channels, and more generally any other potential channel, should become apparent in the following placebo tests.

First, I look at the distributions of $Mixed$ and $Credit/Assets$ around the same cutoff over fiscal years 2012-2015, that is, a period when the reporting thresholds described in Table 1 were not applicable.[29] If the main results are driven by other policies with the same thresholds, by an omitted size aspect, or by any other alternative channel, I would expect to find a discontinuity in bank credit to assets around the cutoff. Figure 5 shows the absence of a discontinuity in both $Mixed$ and residualized $Credit/Assets$ around the size cutoff (Figure A8 plots the raw bank credit to assets values). I formally confirm this in column (1) of Table 5, which presents the first-stage and reduced-form estimates of the RD specification, in Panel A and B respectively. The coefficient of $Medium$ in both specifications is not statistically significant. These findings provide strong support for the interpretation that my main results result from differential reporting requirements.

[**Figure 5 here**]

[**Table 5 here**]

If reporting outcomes and bank credit to assets ratios are balanced before the size-based thresholds reform, a difference-in-differences (DiD) approach produces similar estimates to those derived from the RDD. Table 5 documents that my results are robust to using a

[28]These regulations include quotas for individuals with disabilities, a gender equality plan (including wage audits), a protocol for the prevention of occupational hazards, the obligation to form a work council, and a protocol for the prevention of harassment of LGTBI individuals (only since 2023).

[29]I choose a four-year estimation period to mirror the length of the main analysis.

difference-in-differences specification and illustrates the correspondence between the RDD and the DiD estimates. Columns (1) and (2) report the estimates of the RD specification in the placebo period (2012-2015) and the main period (2016-2019), respectively; and column (3) presents the results from the DiD specification. Panel A presents the first-stage results, where $Mixed$ is the dependent variable, and Panel B presents the reduced-form estimates, where $Credit/Assets$ is the dependent variable. Column (3) shows that medium firms are more likely to disclose more detailed financial statements and have higher bank credit to assets ratios after the size-based thresholds reform, relative to small firms. Note that the magnitude of the DiD estimate is exactly the difference between the RDD estimates from the main period (2016-2019) and the placebo period (2012-2015). Thus, the DiD specification provides a formal statistical test for the difference in the RDD coefficients between the main period and the placebo period. Section C of the Appendix provides the details of the difference-in-differences specification used in column (3).

Finally, I repeat the main analysis around arbitrary placebo cutoffs where the effect is expected to be zero. Again, if the instrument $Medium$ is capturing any alternative channel other than the incremental reporting requirements, I would find a discontinuity in the distribution of $Mixed$ or $Credit/Assets$. Table A3 of the Appendix presents the results for different placebo cutoffs: $LDT2 = c$ for $c = -20\%, 20\%, 40\%, 60\%$, and 80%. Both the first and second rows, which correspond to the main estimates from the first-stage and reduced-form regressions respectively, show that the distribution of public reporting practices and bank credit to assets are continuous around these placebo cutoffs.

7.2 Alternative Regression and Sample Specifications

In this section, I assess the robustness of the results to alternative sample and regression specifications.

First, I examine the robustness of the results to different regression specifications such as the inclusion of controls and fixed effects, alternative kernels, clusters, a different winsorization of the dependent variable, and an alternative definition of the dependent variable that includes all types of loans. All the different specifications are similarly estimated for multiple bandwidths. The coefficients of $\widehat{Mixed}$ from the second-stage regression of the RD for these different specifications are displayed in Table 6. The results are robust to all the different specifications estimated for different bandwidths. The coefficients reported in the main analysis tend to lie in the middle of the distribution of the coefficients derived from these alternative specifications. In Table A4 of the Appendix, I present the results for the first-stage regression and I find similar results.

[**Table 6 here**]

In my main specification, I assume that the effect of size on bank credit to assets is captured by a linear function. Failing to specify the correct functional form of the relationship between bank credit to assets and size may result in a potential bias of the estimates. An alternative approach to local linear regression consists of fitting higher-order polynomials of the running variable using a larger sample size (e.g., Roberts and Whited (2013); Malenko and Shen (2016)).[30] Table A5 of the Appendix presents the first- and second-stage results using quadratic, cubic, and quartic polynomials estimated on a 20.8% bandwidth around the threshold. The main coefficients remain quantitatively similar to alternative functional forms.

Next, I estimate the first- and second-stage regressions excluding from the sample the observations where the employee-based threshold is binding. If the sorting of firms below the employee-based threshold occurs to avoid the reporting regulation, and not the auditing regulation as Figure 2 suggests, or if the instrument $Medium$ is capturing the effect of other policies using the 50-employees threshold, I would expect to find different results if I remove these observations from the sample. Table A6 of the Appendix presents the first- and second-stage estimates. The results remain similar, supporting the validity of the main specification,

[30] Note, however, that recent work by Gelman and Imbens (2019) recommends the use of linear or quadratic polynomials of the running variable.

although the coefficients from the second-stage regression are somewhat smaller.[31]

I repeat the main analysis excluding firms that exceed either none or the three size-based thresholds. This process accounts for the plausible inherent differences of these firms, potentially leading to more comparable observations between the treated and control groups. The results presented in Table A7 of the Appendix show the robustness of the estimates to this alternative specification.[32]

I also repeat the analysis allowing for a flexible optimal bandwidth, different at the two sides of the size cutoff (e.g., Calonico et al. (2014)). Opting for a flexible bandwidth is appealing in this specific setting as it accounts for how firms' density decreases with firm size. Table A8 of the Appendix presents the first- and second-stage results and shows that the results are robust to using a flexible bandwidth.

Finally, pooling multiple firm-year observations raises the concern that medium firms previously classified as medium are more likely to be classified as medium again in the following years. If receiving treatment affects not only firms' bank credit, but also firms' real growth, then being treated also increases the probability of being treated in the future. Thus, there may not be random assignment into treatment (e.g., Carlson et al. (2022)). I estimate the first- and second-stage regressions limiting medium firms to firms that are classified as medium for the first time in the sample and the following year (due to the two-year feature of the assignment rule). Table A9 of the Appendix shows that the first- and second-stage results remain robust to this alternative specification.

7.3 Specification Using Big and Medium Firms

As outlined in Section 3, my emphasis is placed on the regulation that classifies small and medium firms due to the anticipated significant impact on private firms' bank credit. In

[31]The second-stage results estimated on 8% and 9% bandwidths are not significant for conventional levels. I attribute the lack of significance to the lower power of this test given the smaller number of observations compared to the main analysis. Note that the results on wider bandwidths incorporate more observations and yield more precise (statistically significant) and stable coefficients.

[32]Note that the main specification already accounts for the effect of exceeding a certain number of thresholds and for the inherent size differences between firms exceeding a different number of thresholds.

contrast, I expect lower (if any) effects on the regulation that classifies medium and big firms because most medium firms already voluntarily adhere to the detailed model, resulting in the regulation having a limited impact on the public reporting practices of these firms.

Table 7 presents the first- and second-stage estimates of the sample of medium and big firms around the larger regulatory thresholds ($Assets = 11,400$, $Sales = 22,800$, $Emp = 250$), respectively (Figure A9 and A10 of the Appendix presents the corresponding plots). In these specifications, $Mixed$ is replaced by $Detailed$ which equals one if the firm publicly discloses the detailed model, and zero if it publicly discloses the mixed or abbreviated model. In line with my prediction, I find that the regulation has a lower effect on the public reporting of big firms compared to the results from the main specification, and no significant effect on bank credit to assets ratios. Specifically, big firms are only about 8% more likely to publicly disclose the detailed model than otherwise similar medium firms. Furthermore, I cannot reject the hypothesis that the instrument is weak because the F-statistic is below 10. The alignment of these findings with my anticipated effects lends additional support to the validity of the research design.

[**Table 7 here**]

8 Potential Mechanism and Real Outcomes

8.1 Potential Mechanism

Considering the expansive nature of reporting requirements and their known impact on various dimensions (e.g., Bernard (2016); Breuer et al. (2018, 2019); Bernard et al. (2021); Breuer (2021); Breuer et al. (2022)), it is likely that several potential mechanisms are playing a role in shaping my results. Moreover, it is worth noting that these mechanisms may not be fully observable or function through interconnected causal relationships. Therefore, it is difficult to attribute the results to a single mechanism in this particular setting.

Drawing from prior evidence, I argue that my main result could occur through changes in the firm's information environment which encourage banking competition (Breuer et al., 2018). Specifically, reporting regulation, by forcing firms to disclose more detailed financial statements, may reveal part of the private information acquired by incumbent banks about their borrowers and reduce the banks' incentives to invest in relationship lending. In turn, this may level information asymmetries in the banking industry encouraging banking competition. Consequently, banks operating in more competitive environments may extend more credit (e.g., Carlson et al. (2022)).

I provide evidence about the plausibility of this mechanism in two different ways. First, I document that the results from the German setting of Breuer et al. (2018) apply to my setting. Second, I study if the reporting regulation has a stronger effect on private firms' bank credit in ex-ante more contestable credit markets. Table 8 presents the results from these tests.

[**Table 8 here**]

In column (1), I estimate my RD specification using the number of banks a firm has a debt relationship with in $t+1$ as the dependent variable, similar to Breuer et al. (2018). I find that the coefficient of $\widehat{Mixed}$ is positive and significant, indicating that medium firms affected by the regulation contract with more banks relative to small firms. These findings are consistent with those in Breuer et al. (2018). For a comparison, Breuer et al. (2018) report that German medium firms have a 4% greater number of bank relationships relative to small firms (reduced-form effect), while my estimates for the Spanish setting imply an 11.8% increase (reduced-form estimate/mean Nbanks at the threshold $= 3.148 * 0.138/4 = 0.118$). This result supports that reporting regulation shifts a relational for a transactional type of bank lending, consistent with reporting regulation fostering banking competition.

In columns (2) and (3), I partition the main sample into two subsamples based on the median values of the Herfindahl-Hirschman index (HHI) measured in December of 2016, and then I estimate the RD specification separately for each subsample.[33] The HHI measures bank concentration at the province level (2-digit zip code) with high values indicating high bank market concentration and low values indicating low bank market concentration. I find that the coefficient of $\widehat{Mixed}$ is greater for the subsample with low HHI values relative to the subsample with high HHI values. This finding suggests that reporting regulation has stronger effects on private firms' bank credit in ex-ante more contestable markets, consistent with the main results being driven by an increase in banking competition.[34]

8.2 Real Outcomes

The increase in firms' bank credit raises a natural question regarding why firms do not choose to publicly disclose their financial statements voluntarily. While existing literature highlights some costs associated with the regulation that provide a potential explanation of why firms generally do not provide voluntary reports (e.g., Bernard (2016); Bernard et al. (2018); Breuer et al. (2019)), I further shed light on this question by examining whether firms substitute other financing sources with the new bank credit, and if the regulation impacts other firm-level dimensions.

In Table 9, I conduct my RD specification using different firm-level *balance sheet* dependent variables, all measured in $t+1$. Note that the sample in this analysis is smaller since I lose some observations in $t+1$ because they are missing in my CBI database.[35] First, in column (1), I examine the effect of reporting regulation on the logarithm of firms' total assets ($Log(Assets)$). The coefficient of $\widehat{Mixed}$ from the second-stage regression is not statistically significant, indicating that firms affected by the reporting regulation do not grow

[33] Note that the HHI is measured right before the financial statements of fiscal year 2016 (first fiscal year after affected by the regulation) are disclosed, mitigating endogenous changes in bank market structures.

[34] Given that bank market structures are endogenously determined, these cross-sectional results aim to provide suggestive, rather than causal, evidence.

[35] Recall that the dependent variable in the main specification, $Credit/Assets$, is collected from the Central Credit Registry (CIR) and thus, I do not use data in $t+1$ from CBI. Data in $t+1$ from my CBI database may be missing because observations do not meet the financial statements quality standards applied by the Bank of Spain, because the panel data is unbalanced, or because the observation is dropped when I apply some of the filters described in section 4.

more, despite their greater levels of bank credit. This finding suggests a substitution between bank credit and alternative financing sources. If the net financing capital increased as a consequence of the increase in bank credit, it would necessarily result in an increase in the firm's total assets, as a firm's total assets are equal to total equity and liabilities.

[**Table 9 here**]

Next, I examine if firms affected by the reporting regulation exhibit differences in the two sources of capital that comprise the balance sheet financing structure, equity capital and total liabilities. Columns (2) and (3) present the coefficients of $\widehat{Mixed}$ from the second-stage regression using equity over total assets ($Equity/Assets$) and total liabilities over total assets ($Liab/Assets$) as the dependent variables, respectively. Both coefficients are not statistically significant. Finding insignificant coefficients in both regressions necessarily implies a trade-off of firms affected by the regulation between bank credit and other liabilities, since bank credit is included in a firm's total liabilities.

Finally, I explore the effect of reporting regulation on internal financing sources. The theory of precautionary savings posits that holding cash enables firms to undertake valuable projects as they arise. Yet, the significance of these cash buffers decreases with easier access to external capital markets (Almeida, Campello and Weisbach, 2004). If the provision of public financial statements improves access to capital markets, then I would expect regulated firms to decrease their cash holdings. Alternatively, if the observed change in firms' capital structure is independent of capital markets access, I should not observe a significant change in firms' cash holdings. Column (4) presents the coefficient of $\widehat{Mixed}$ from the second-stage regression using cash plus short-term investments over total assets ($Cash/Assets$) as the dependent variable. The coefficient is not statistically significant, suggesting no substitution between bank credit and internal financing sources of firms affected by the regulation.

The previous results suggest that firms affected by the regulation substitute other liabilities with the new bank credit. Table 10 further explores this substitution effect by showing the coefficient of $\widehat{Mixed}$ from the second-stage regression of the RD specification using different balance sheet liabilities all measured in $t+1$. Columns (1) and (2) present the results using long-term and short-term balance sheet debt over total assets as the dependent variables, respectively. The coefficient of $\widehat{Mixed}$ is only positive and significant for column (1), indicating that firms affected by the reporting regulation increase their long-term balance sheet debt. This result is consistent with the central result of this paper.[36]

[**Table 10 here**]

In Column (3), I find that firms affected by the regulation decrease other (bank unrelated) short-term liabilities labeled as $Ot.Liab/Assets$, providing further evidence of this substitution between financing sources. Note that I cannot perform additional analyses to distinguish what specific liabilities drive the substitution effect as data from CBI is not sufficiently granular. The observed effect could be influenced by multiple liabilities such as creditors for services rendered, advances from customers, remunerations pending payment, or public treasury creditors, among others. Finally, I test if this substitution effect can be attributed to a lower reliance of regulated firms on trade credit, which is an important source of financing for small and medium-sized enterprises (SMEs) (e.g., Berger and Udell (2006)). Column (4) shows that this is not the case as the coefficient on $\widehat{Mixed}$ is not significant when using accounts payable over total assets as the dependent variable.

9 Conclusion

In this paper, I contribute to the debate on reporting regulation by investigating its impact on private firms' bank credit, an aspect that has received limited attention in the literature. My findings reveal a positive effect of reporting regulation on private firms' bank credit potentially driven by an increase in banking competition. Yet, this positive effect on bank

[36]The reported coefficient in this specification is smaller than the coefficient reported in the main specification. I attribute this finding to firms underreporting their balance sheet bank debt (Duro et al., 2022).

credit does not lead to a significant impact on the net financing capital of the firm, as my evidence points to a substitution between bank credit and alternative balance sheet liabilities. As a result, I do not observe significant differences in the total assets of firms subject to incremental reporting regulation.

While my research design offers unique opportunities for establishing a causal relationship by combining a natural experiment with an appropriate institutional framework, I acknowledge that my inferences are specific to a subset of Spanish firms and the results may not generalize to other economies. However, several features presented in this study are consistent with the results from other studies using different institutional frameworks (Hail et al., 2020). For instance, I reproduce the results from Breuer et al. (2018), who investigate small and medium German firms; or I document the sorting of firms below the bundled reporting auditing and thresholds, a behavior that is observed in most EU countries (Bernard et al., 2018). This supports that my results could apply to other settings.

The evidence in this paper adds to the ongoing discussion on reporting regulation. It brings to the debate a novel dimension of the effects of such regulation by highlighting its impact on bank credit for private SMEs, a pivotal funding source for these firms. Additionally, my contribution exploits a novel institutional framework that supports the existence of a causal relationship. This approach is instrumental in providing valuable insights to regulators.

References

Acharya, V.V., Almeida, H., Campello, M., 2007. Is cash negative debt? a hedging perspective on corporate financial policies. Journal of financial intermediation 16, 515–554.

Admati, A.R., Pfleiderer, P., 2000. Forcing firms to talk: Financial disclosure regulation and externalities. The Review of financial studies 13, 479–519.

Aghamolla, C., Thakor, R.T., 2022. Do mandatory disclosure requirements for private firms increase the propensity of going public? Journal of Accounting Research 60, 755–804.

Alfaro, L., García-Santana, M., Moral-Benito, E., 2021. On the direct and indirect real effects of credit supply shocks. Journal of Financial Economics 139, 895–921.

Allee, K.D., Yohn, T.L., 2009. The demand for financial statements in an unregulated environment: An examination of the production and use of financial statements by privately held small businesses. The accounting review 84, 1–25.

Allen, F., Gale, D., 2004. Competition and financial stability. Journal of money, credit and banking , 453–480.

Almeida, H., Campello, M., 2010. Financing frictions and the substitution between internal and external funds. Journal of financial and quantitative analysis 45, 589–622.

Almeida, H., Campello, M., Weisbach, M.S., 2004. The cash flow sensitivity of cash. The journal of finance 59, 1777–1804.

Almunia, M., Lopez Rodriguez, D., Moral-Benito, E., 2018. Evaluating the macro-representativeness of a firm-level database: an application for the spanish economy. Banco de Espana Ocassional Paper .

Angrist, J.D., Pischke, J.S., 2009. Mostly harmless econometrics: An empiricist's companion. Princeton university press.

Armstrong, C., Kepler, J.D., Samuels, D., Taylor, D., 2022. Causality redux: The evolution of empirical methods in accounting research and the growth of quasi-experiments. Journal of Accounting and Economics , 101521.

Armstrong, C.S., Guay, W.R., Weber, J.P., 2010. The role of information and financial reporting in corporate governance and debt contracting. Journal of accounting and economics 50, 179–234.

Asher, S., Novosad, P., 2020. Rural roads and local economic development. American economic review 110, 797–823.

Badertscher, B., Shroff, N., White, H.D., 2013. Externalities of public firm presence: Evidence from private firms' investment decisions. Journal of Financial Economics 109, 682–706.

Baginski, S.P., Hinson, L.A., 2016. Cost of capital free-riders. The Accounting Review 91, 1291–1313.

Baik, B.K., Berfeld, N., Verdi, R.S., 2021. Do public financial statements influence venture capital and private equity financing? Available at SSRN 3867958 .

Banco de España, 2021. Resultados anuales de las empresas no financieras. Suplemento Metodólogico. Banco de España.

Banerjee, A.V., Duflo, E., 2014. Do firms want to borrow more? testing credit constraints using a directed lending program. Review of Economic Studies 81, 572–607.

Berger, A.N., Udell, G.F., 2006. A more complete conceptual framework for sme finance. Journal of Banking & Finance 30, 2945–2966.

Berger, P.G., Minnis, M., Sutherland, A., 2017. Commercial lending concentration and bank expertise: Evidence from borrower financial statements. Journal of Accounting and Economics 64, 253–277.

Bernard, D., 2016. Is the risk of product market predation a cost of disclosure? Journal of Accounting and Economics 62, 305–325.

Bernard, D., Burgstahler, D., Kaya, D., 2018. Size management by european private firms to minimize proprietary costs of disclosure. Journal of Accounting and Economics 66, 94–122.

Bernard, D., Kaya, D., Wertz, J., 2021. Entry and capital structure mimicking in concentrated markets: The role of incumbents' financial disclosures. Journal of Accounting and Economics 71, 101379.

Bernstein, S., 2015. Does going public affect innovation? The Journal of finance 70, 1365–1403.

Bharath, S.T., Dahiya, S., Saunders, A., Srinivasan, A., 2011. Lending relationships and loan contract terms. The Review of Financial Studies 24, 1141–1203.

Bharath, S.T., Sunder, J., Sunder, S.V., 2008. Accounting quality and debt contracting. The accounting review 83, 1–28.

Boyd, J.H., De Nicolo, G., 2005. The theory of bank risk taking and competition revisited. The Journal of finance 60, 1329–1343.

Breuer, M., 2021. How does financial-reporting regulation affect industry-wide resource allocation? Journal of Accounting Research 59, 59–110.

Breuer, M., Hombach, K., Müller, M.A., 2018. How does financial reporting regulation affect firms' banking? The Review of Financial Studies 31, 1265–1297.

Breuer, M., Hombach, K., Müller, M.A., 2022. When you talk, i remain silent: Spillover effects of peers' mandatory disclosures on firms' voluntary disclosures. The Accounting Review 97, 155–186.

Breuer, M., Leuz, C., Vanhaverbeke, S., 2019. Reporting regulation and corporate innovation. Technical Report. National Bureau of Economic Research.

Burgstahler, D., Chuk, E., 2015. Do scaling and selection explain earnings discontinuities? Journal of Accounting and Economics 60, 168–186.

Bushee, B.J., Leuz, C., 2005. Economic consequences of sec disclosure regulation: evidence from the otc bulletin board. Journal of accounting and economics 39, 233–264.

Calonico, S., Cattaneo, M.D., Titiunik, R., 2014. Robust nonparametric confidence intervals for regression-discontinuity designs. Econometrica 82, 2295–2326.

Carlson, M., Correia, S., Luck, S., 2022. The effects of banking competition on growth and financial stability: Evidence from the national banking era. Journal of Political Economy 130, 462–520.

Carrizosa, R., Ryan, S.G., 2017. Borrower private information covenants and loan contract monitoring. Journal of Accounting and Economics 64, 313–339.

Cattaneo, M.D., Jansson, M., Ma, X., 2018. Manipulation testing based on density discontinuity. The Stata Journal 18, 234–261.

Chen, S., Miao, B., Shevlin, T., 2015. A new measure of disclosure quality: The level of disaggregation of accounting data in annual reports. Journal of Accounting Research 53, 1017–1054.

Cheng, M., Dhaliwal, D., Zhang, Y., 2013. Does investment efficiency improve after the disclosure of material weaknesses in internal control over financial reporting? Journal of accounting and economics 56, 1–18.

Cheynel, E., Levine, C.B., 2020. Public disclosures and information asymmetry: A theory of the mosaic. The Accounting Review 95, 79–99.

Chodorow-Reich, G., 2014. The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis. The Quarterly Journal of Economics 129, 1–59.

Choi, A., Lee, E.Y., Park, S., Sohn, B.C., 2022. The differential effect of accrual-based and real earnings management on audit fees: international evidence. Accounting and Business Research 52, 254–290.

Christensen, H.B., Hail, L., Leuz, C., 2016a. Capital-market effects of securities regulation: Prior conditions, implementation, and enforcement. The Review of Financial Studies 29, 2885–2924.

Christensen, H.B., Nikolaev, V.V., Wittenberg-Moerman, R., 2016b. Accounting information in financial contracting: The incomplete contract theory perspective. Journal of accounting research 54, 397–435.

Coase, R.H., 1960. The problem of social cost. The Journal of Law and Economics 3, 1–44.

Dedman, E., Lennox, C., 2009. Perceived competition, profitability and the withholding of information about sales and the cost of sales. Journal of Accounting and Economics 48, 210–230.

Dell, M., 2015. Trafficking networks and the mexican drug war. American Economic Review 105, 1738–1779.

Dell'Ariccia, G., Friedman, E., Marquez, R., 1999. Adverse selection as a barrier to entry in the banking industry. The RAND Journal of Economics , 515–534.

Demiroglu, C., Ozbas, O., Silva, R.C., Ulu, M.F., 2021. Do physiological and spiritual factors affect economic decisions? The Journal of Finance 76, 2481–2523.

Diamond, D.W., 1984. Financial intermediation and delegated monitoring. The review of economic studies 51, 393–414.

Donelson, D.C., Jennings, R., Mcinnis, J., 2017. Financial statement quality and debt contracting: Evidence from a survey of commercial lenders. Contemporary Accounting Research 34, 2051–2093.

Durnev, A., Mangen, C., 2020. The spillover effects of md&a disclosures for real investment: The role of industry competition. Journal of Accounting and Economics 70, 101299.

Duro, M., López-Espinosa, G., Mayordomo, S., Ormazabal, G., Rodríguez-Moreno, M., 2022. Enforcing mandatory reporting on private firms: the role of banks .

Frank, M.Z., Goyal, V.K., 2008. Trade-off and pecking order theories of debt. Handbook of empirical corporate finance , 135–202.

Frankel, R., Kim, B.H., Ma, T., Martin, X., 2020. Bank monitoring and financial reporting quality: The case of accounts receivable–based loans. Contemporary Accounting Research 37, 2120–2144.

Gelman, A., Imbens, G., 2019. Why high-order polynomials should not be used in regression discontinuity designs. Journal of Business & Economic Statistics 37, 447–456.

Glaeser, S., Omartian, J.D., 2022. Public firm presence, financial reporting, and the decline of us manufacturing. Journal of Accounting Research 60, 1085–1130.

Gow, I.D., Larcker, D.F., Reiss, P.C., 2016. Causal inference in accounting research. Journal of Accounting Research 54, 477–523.

Greenstone, M., Oyer, P., Vissing-Jorgensen, A., 2006. Mandated disclosure, stock returns, and the 1964 securities acts amendments. The Quarterly Journal of Economics 121, 399–460.

Gustafson, M.T., Ivanov, I.T., Meisenzahl, R.R., 2021. Bank monitoring: Evidence from syndicated loans. Journal of Financial Economics 139, 452–477.

Hahn, J., Todd, P., Van der Klaauw, W., 2001. Identification and estimation of treatment effects with a regression-discontinuity design. Econometrica 69, 201–209.

Hail, L., Lang, M., Leuz, C., 2020. Reproducibility in accounting research: Views of the research community. Journal of Accounting Research 58, 519–543.

Hale, G., Santos, J.A., 2009. Do banks price their informational monopoly? Journal of Financial Economics 93, 185–206.

Imbens, G.W., Angrist, J.D., 1994. Identification and estimation of local average treatment effects. Econometrica: journal of the Econometric Society , 467–475.

Imbens, G.W., Lemieux, T., 2008. Regression discontinuity designs: A guide to practice. Journal of econometrics 142, 615–635.

Ivashina, V., Laeven, L., Moral-Benito, E., 2022. Loan types and the bank lending channel. Journal of Monetary Economics 126, 171–187.

Jensen, M.C., Meckling, W.H., 1976. Theory of the firm: Managerial behavior, agency costs and ownership structure. Journal of financial economics 3, 305–360.

Kausar, A., Shroff, N., White, H., 2016. Real effects of the audit choice. Journal of Accounting and Economics 62, 157–181.

Kim, J., Valentine, K., 2021. The innovation consequences of mandatory patent disclosures. Journal of Accounting and Economics 71, 101381.

La Porta, R., Lopez-de Silanes, F., Shleifer, A., 2006. What works in securities laws? The journal of finance 61, 1–32.

La Porta, R., Lopez-de Silanes, F., Shleifer, A., Vishny, R.W., 1997. Legal determinants of external finance. The journal of finance 52, 1131–1150.

Lambert, R., Leuz, C., Verrecchia, R.E., 2007. Accounting information, disclosure, and the cost of capital. Journal of accounting research 45, 385–420.

Leuz, C., 2018. Evidence-based policymaking: promise, challenges and opportunities for accounting and financial markets research. Accounting and Business Research 48, 582–608.

Leuz, C., 2022. Towards a design-based approach to accounting research. Journal of Accounting and Economics 74, 101550.

Leuz, C., Wysocki, P.D., 2016. The economics of disclosure and financial reporting regulation: Evidence and suggestions for future research. Journal of accounting research 54, 525–622.

Lisowsky, P., Minnis, M., 2020. The silent majority: Private us firms and financial reporting choices. Journal of Accounting Research 58, 547–588.

Malenko, N., Shen, Y., 2016. The role of proxy advisory firms: Evidence from a regression-discontinuity design. The Review of Financial Studies 29, 3394–3427.

Martinez-Miera, D., Repullo, R., 2010. Does competition reduce the risk of bank failure? The Review of Financial Studies 23, 3638–3664.

McCrary, J., 2008. Manipulation of the running variable in the regression discontinuity design: A density test. Journal of econometrics 142, 698–714.

Mester, L.J., Nakamura, L.I., Renault, M., 2007. Transactions accounts and loan monitoring. The Review of Financial Studies 20, 529–556.

Minnis, M., 2011. The value of financial statement verification in debt financing: Evidence from private us firms. Journal of accounting research 49, 457–506.

Minnis, M., Shroff, N., 2017. Why regulate private firm disclosure and auditing? Accounting and Business Research 47, 473–502.

Minnis, M., Sutherland, A., 2017. Financial statements as monitoring mechanisms: Evidence from small commercial loans. Journal of Accounting Research 55, 197–233.

Minnis, M., Sutherland, A., Vetter, F., 2023. Financial statements not required. Available at SSRN 4408334 .

Myers, S.C., 1984. Capital structure puzzle. National Bureau of Economic Research Cambridge, Mass., USA .

Petersen, M.A., Rajan, R.G., 1995. The effect of credit market competition on lending relationships. The Quarterly Journal of Economics 110, 407–443.

Reardon, S.F., Robinson, J.P., 2012. Regression discontinuity designs with multiple rating-score variables. Journal of research on Educational Effectiveness 5, 83–104.

Roberts, M.R., Whited, T.M., 2013. Endogeneity in empirical corporate finance1, in: Handbook of the Economics of Finance. Elsevier. volume 2, pp. 493–572.

Roychowdhury, S., Shroff, N., Verdi, R.S., 2019. The effects of financial reporting and disclosure on corporate investment: A review. Journal of Accounting and Economics 68, 101246.

Sharpe, S.A., 1990. Asymmetric information, bank lending, and implicit contracts: A stylized model of customer relationships. The journal of finance 45, 1069–1087.

Shroff, N., Verdi, R.S., Yost, B.P., 2017. When does the peer information environment matter? Journal of Accounting and Economics 64, 183–214.

Shroff, N., Verdi, R.S., Yu, G., 2014. Information environment and the investment decisions of multinational corporations. The Accounting Review 89, 759–790.

Staiger, D., Stock, J.H., 1997. Instrumental variables regression with weak instruments. Econometrica 65, 557–586.

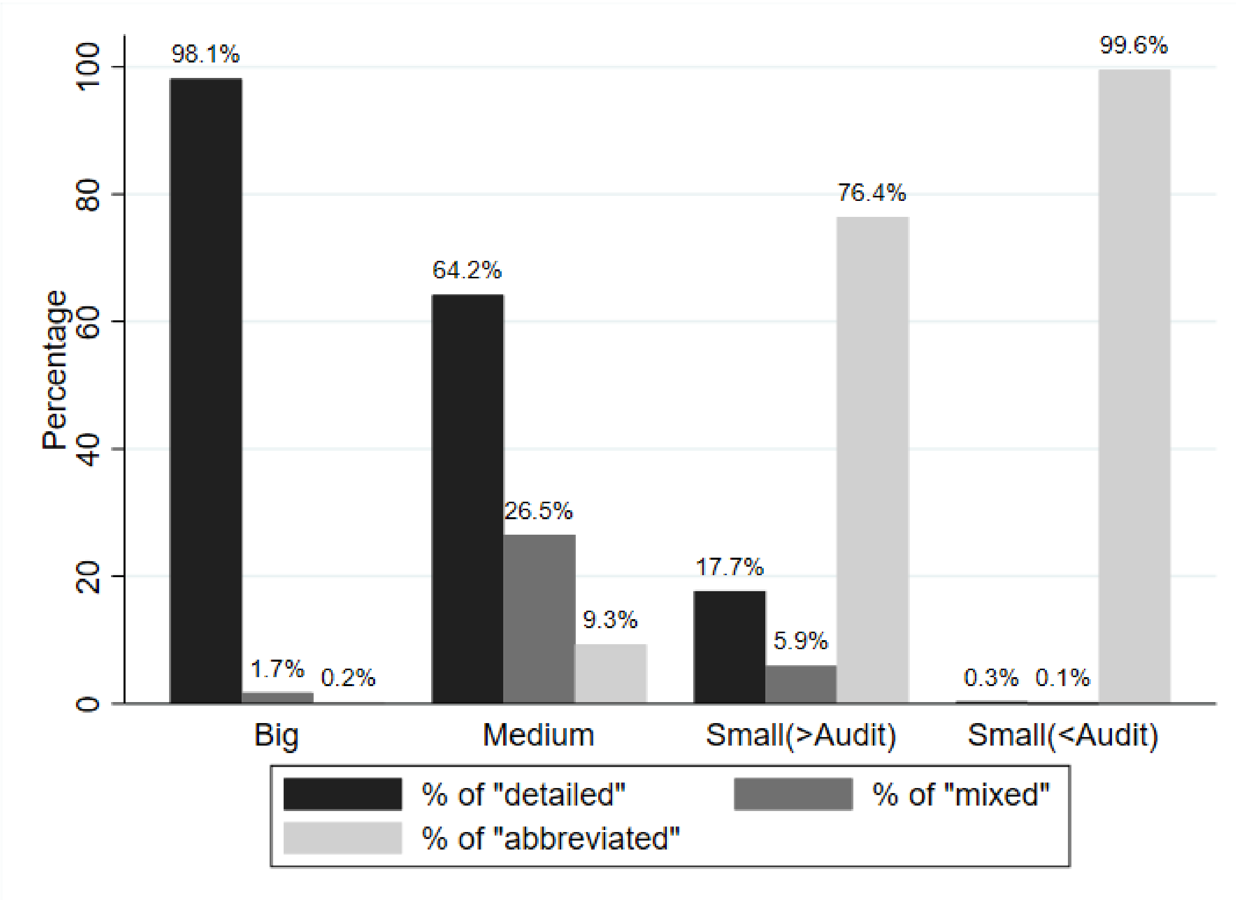
Stigler, G.J., 1964. Public regulation of the securities markets. The Journal of Business 37, 117–142.

Stiglitz, J.E., Weiss, A., 1981. Credit rationing in markets with imperfect information. The American economic review 71, 393–410.

Sutherland, A., 2018. Does credit reporting lead to a decline in relationship lending? evidence from information sharing technology. Journal of Accounting and Economics 66, 123–141.

Yang, Y.J., 2019. Real effects of disclosure regulation: Evidence from us import competition. Available at SSRN 3456743 .

Figure 1: Share of firms publicly disclosing detailed, mixed, and abbreviated models by size classification

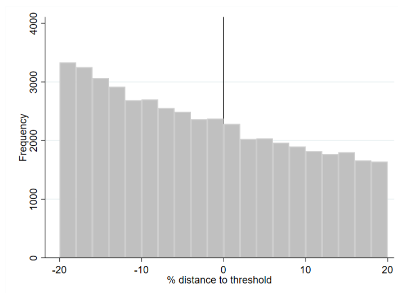


This figure presents the share of big, medium, small firms with mandated audited, and small firms without mandated audits reporting the detailed, mixed, and abbreviated models. The reporting and auditing requirements, and the size classification of firms are described in Table 1. The sample period is 2018-2019. Prior to 2018, the mixed and detailed models are grouped in the same category, preventing their separate identification.

Figure 2: Distribution of assets, sales, and employees around the regulatory thresholds over 2016-2019

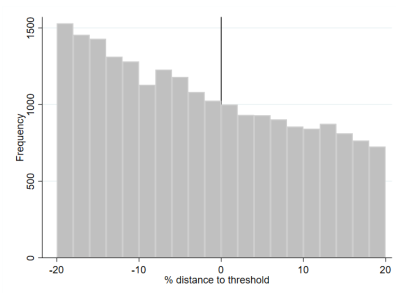
Reporting thresholds

(a) Assets = 4M



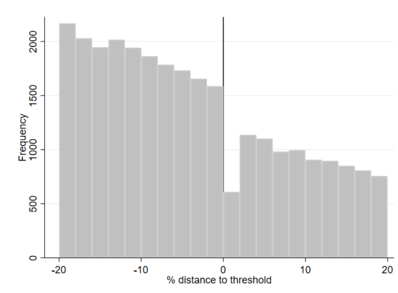
T = 0.89

(b) Sales = 8M



T = -0.41

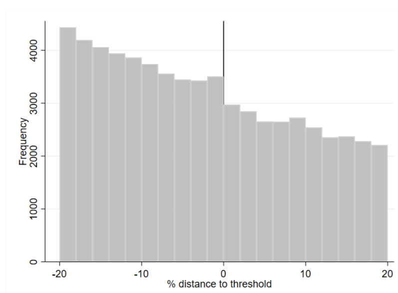
(c) Emp = 50



T = 9.32***

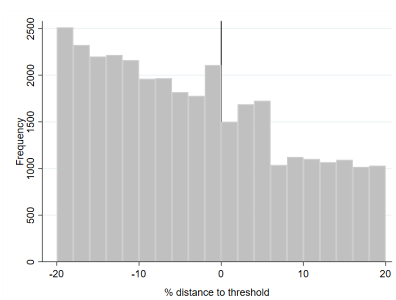
Auditing thresholds

(d) Assets = 2.85M



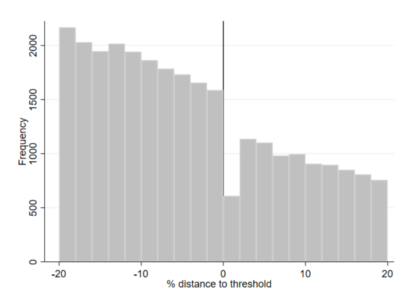
T = 4.22***

(e) Sales = 5.7M



T = 8.36***

(f) Emp = 50



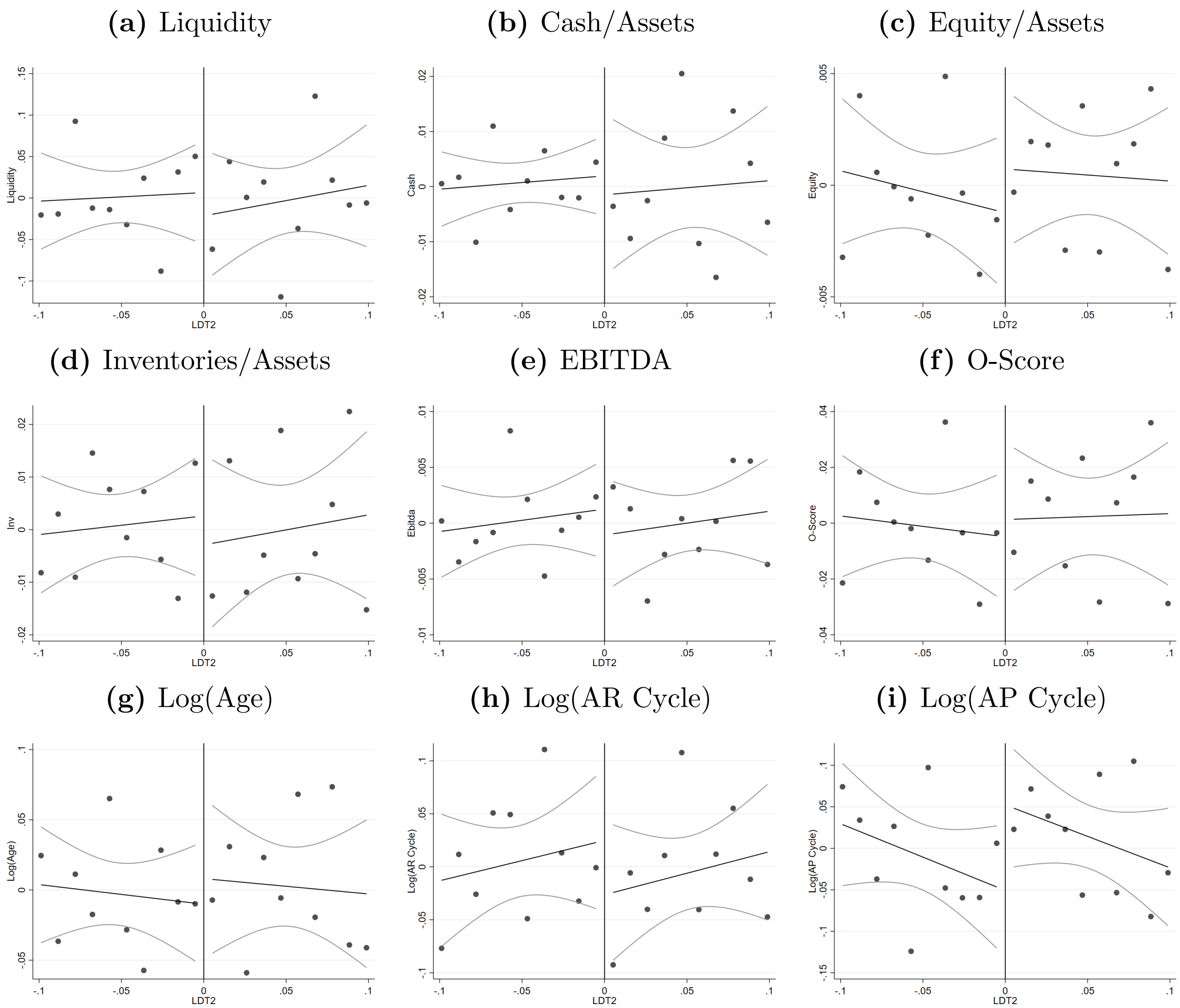
T = 9.32***

This figure presents the frequency distribution of the percentage distance of assets, sales, and employees scaled by the corresponding reporting and auditing regulatory threshold. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. Each bin has a width of 2% of the nominal value of the threshold as in Bernard et al. (2018). The regulatory reporting and auditing requirements are described in Table 1. To test the significance of each discontinuity I use the standardized difference statistic from Burgstahler and Chuk (2015). The statistic is estimated as follows:

$$T = \frac{n_i - 0.5(n_{i-1} + n_{i+1})}{\sqrt{1.5 n_i}}$$

Where n_i is the number of observations in bin i. T is normally distributed. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

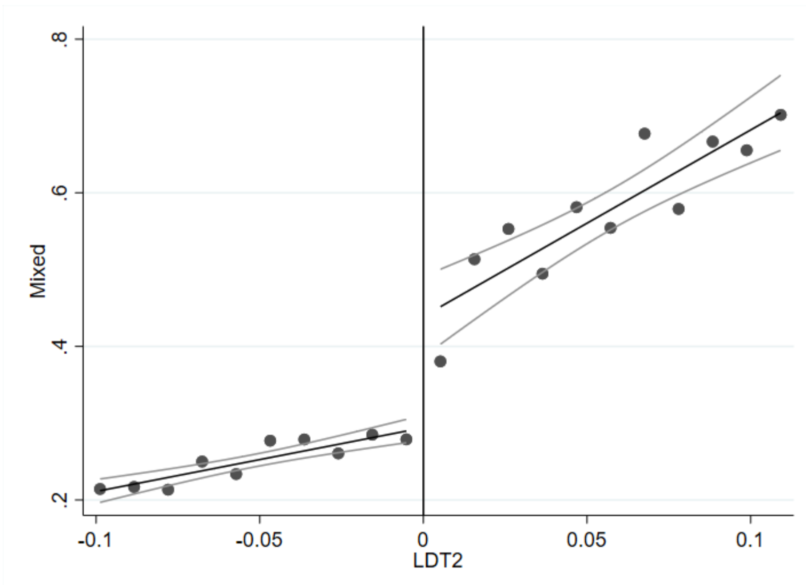
Figure 3: Balance of covariates



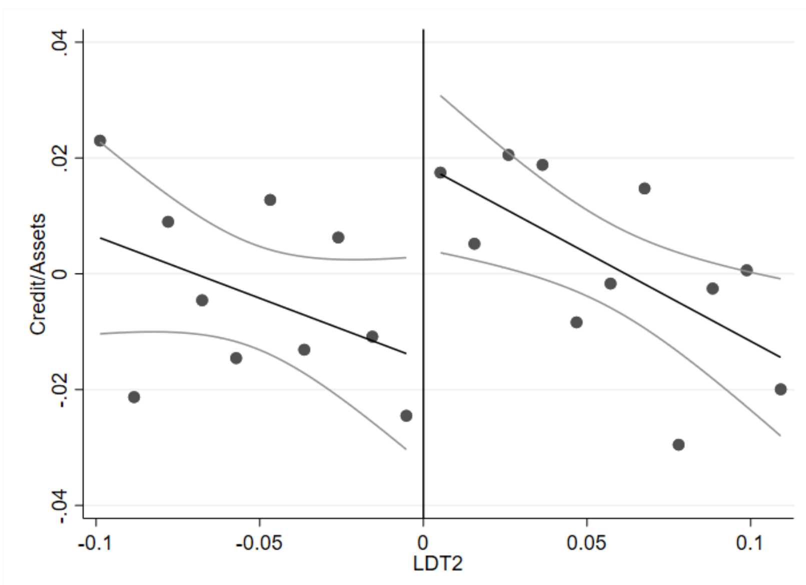
This figure plots the distribution of residualized firm-level covariates over the least distance to the second threshold ($LDT2$), in a 10.4% bandwidth around the cutoff. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. Residualized firm-level covariates are the residuals from a regression of the respective firm-level covariate as the dependent variable on all the controls and fixed effects from the main specification other than $Medium$, $LDT2$, and the respective interaction. Each dot represents the average value of the residualized firm-level covariate in equal-sized bins at each side of the cutoff. Dots to the left of 0 represent small firms, while dots to the right of 0 represent medium firms. The solid lines correspond to a linear fit generated separately at each side of the threshold, with the corresponding 90% confidence intervals.

Figure 4: Probability of disclosing more detailed financial statements and bank credit to assets

(a) First stage. Probability of disclosing more detailed financial statements



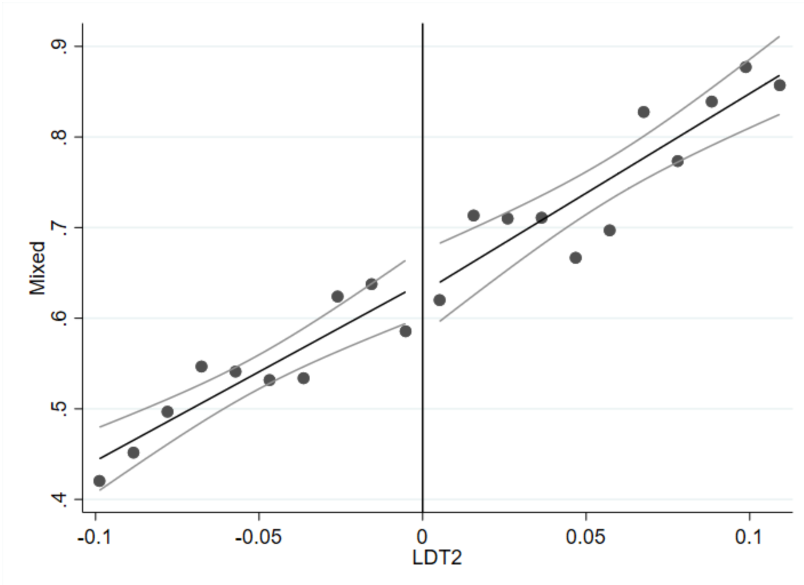
(b) Reduced form. Residualized bank credit to assets



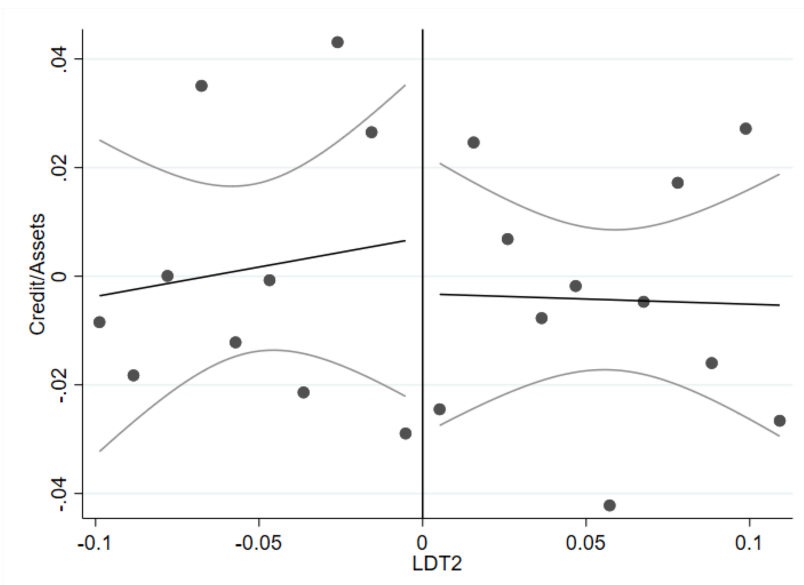
This figure plots the distribution of publicly disclosing more detailed financial statements ($Mixed$) and the residualized ratio of bank credit to total assets ($Credit/Assets$) over the least distance to the second threshold ($LDT2$), in panel (a) and panel (b), respectively, in a 10.4% bandwidth around the cutoff. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. Residualized firm-level bank credit to assets corresponds to the residuals from a regression of bank credit to assets as the dependent variable on all the controls and fixed effects from the main specification other than $Medium$, $LDT2$, and the respective interaction. Each dot represents the average value of the firm-level outcome in equal-sized bins at each side of the cutoff. Dots to the left of 0 represent small firms, while dots to the right of 0 represent medium firms. The solid lines correspond to a linear fit generated separately at each side of the threshold, with the corresponding 90% confidence intervals.

Figure 5: Placebo period 2012-2015

(a) First stage. Probability of disclosing more detailed financial statements



(b) Reduced form. Residualized bank credit to assets



This figure plots the distribution of publicly disclosing more detailed financial statements ($Mixed$) and the residualized ratio of bank credit to total assets ($Credit/Assets$) over the least distance to the second threshold ($LDT2$), in panel (a) and panel (b), respectively, in a 10.4% bandwidth around the cutoff. The sample period is 2012-2015, when the reporting thresholds described in Table 1 were not applicable. Residualized bank credit to assets corresponds to the residuals from a regression of bank credit to assets as the dependent variable on all the controls and fixed effects from the main specification other than $Medium$, $LDT2$, and the respective interaction. Each dot represents the average value of the firm-level outcome in equal-sized bins at each side of the cutoff. Dots to the left of 0 represent small firms, while dots to the right of 0 represent medium firms. The solid lines correspond to a linear fit generated separately at each side of the threshold, with the corresponding 90% confidence intervals.

Table 1: Size classification and reporting requirements

Panel A: Reporting requirements

Small		**Medium**		**Big**	
Assets < 4M		Assets < 11.4M		Rest	
Sales < 8M		Sales < 22.8M		Rest	
Emp < 50		Emp < 250		Rest	
"Abbreviated" model		**"Mixed" model**		**"Detailed" model**	
Bal. Sheet	Abbr.	Bal. Sheet	Det.	Bal. Sheet	Det.
Notes	Abbr.	Notes	Det.	Notes	Det.
Inc. Stmn.	Abbr.	Inc. Stmn.	Abbr.	Inc. Stmn.	Det.
C.F. Stmn.	-	C.F. Stmn.	-	C.F. Stmn.	Det.
Stmn. C.E.	-	Stmn. C.E.	-	Stmn. C.E.	Det.

Panel B: Auditing requirements

Exempted	**Required**
Assets < 2.85M	Rest
Sales < 5.7M	Rest
Emp < 50	Rest

This table describes the size-based reporting and auditing requirements for private limited liability firms in Spain after the regulatory change in 2017 (applying to fiscal years from 2016). Firms are classified as small, medium, big, or exempted from mandatory audits depending on whether they fall beneath two of three size-based regulatory thresholds related to total assets, sales, and employees for two consecutive years. "Bal. Sheet" stands for balance sheet, "Inc. Stmn." stands for income statement, "C.F. Stmn" stands for cash-flow statement, "Stmn. C.E." stands for statement of changes in equity, "Abbr." stands for abbreviated, and "Det." stands for detailed.

Table 2: Summary statistics and balance of covariates

Panel A: Summary statistics of the main variables

	Below Threshold (Small)			Above Threshold (Medium)		
	Mean	SD	50^{th}	Mean	SD	50^{th}
Mixed	0.250	0.433	0	0.559	0.497	1
Credit/Assets	0.240	0.256	0.172	0.258	0.272	0.179
Assets	5.848	2.800	4.877	6.375	2.857	5.380
Sales	7.918	1.853	7.567	8.361	2.008	8.333
Emp	33.84	18.41	32	38.50	21.11	37.55
LDT1	0.587	0.657	0.388	0.718	0.671	0.509
LDT2	-0.0534	0.0296	-0.0548	0.0498	0.0303	0.0487
LDT3	-0.405	0.232	-0.370	-0.359	0.249	-0.329
I^1	0.925	0.263	1	1	0	1
I^3	0	0	0	0.0519	0.222	0
Observations	2,140			1,657		

Panel B: Balance of covariates

	Mean (Small)	Mean (Medium)	Diff. of Means	T-stat	RD estimate	T-stat
Liquidity	2.300	2.282	0.018	0.287	-0.373	-0.614
Cash/Assets	0.170	0.170	0.001	0.139	-0.049	-0.848
Equity/Assets	0.482	0.484	-0.002	-0.227	0.015	0.673
Inventories/Assets	0.190	0.182	0.008	1.474	-0.059	-0.855
EBITDA	0.080	0.080	-0.001	-0.244	-0.009	-0.285
O-Score	-2.111	-2.143	0.032	0.562	0.032	0.216
Log(Age)	3.125	3.144	-0.019	-1.168	-0.018	-0.081
Log(AR Cycle)	3.938	3.881	0.057	1.744*	-0.500	-1.107
Log(AP Cycle)	3.891	3.865	0.026	0.700	0.599	1.108
$\widehat{Credit/Assets}$	0.246	0.250	-0.004	-0.898	-0.053	-0.738

This table presents the summary statistics and balancing of the sample of small and medium firms over 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. An optimal bandwidth of 10.4% around the binding threshold has been used to define the sample firms. Panel A displays the summary statistics of small and medium firms, respectively. Panel B presents balancing tests between the samples of small and medium firms. Columns 1 and 2 show the mean values of small and medium firms, respectively. Column 3 shows the difference in means across columns 1 and 2, and column 4 reports the t-statistic for the difference in means. Column 5 shows the regression discontinuity coefficient from the main specification using the baseline covariate as the dependent variable (and omitting it from the set of controls), and column 6 reports the corresponding t-statistic of the estimate. Table A1 of the Appendix provides a description of the variables.

Table 3: First stage. Effect of reporting regulation on the probability of disclosing the mixed or detailed model

	(1)	(2)	(3)	(4)	(5)
			Mixed		
	10.4%	8%	9%	11%	12%
Medium	0.138***	0.138***	0.138***	0.140***	0.142***
	(0.034)	(0.038)	(0.036)	(0.033)	(0.031)
LDT2	0.438	0.177	0.286	0.458	0.455
	(0.410)	(0.604)	(0.506)	(0.378)	(0.332)
LDT2*Medium	2.124***	2.722***	2.452***	2.005***	1.929***
	(0.670)	(0.982)	(0.829)	(0.616)	(0.540)
Observations	3,797	2,959	3,285	3,987	4,366
R-squared	0.161	0.153	0.155	0.163	0.168
Controls	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm
F-statistic	27.40	20.61	23.25	29.77	34.02

This table presents the first-stage estimates of the effect of reporting regulation on the probability of disclosing the mixed or detailed model. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variable, *Mixed*, is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. The main independent variable, *Medium*, is an indicator variable that equals one if the firm is classified as medium, and zero if it is classified as small. Column (1) presents the results for firms with size within a 10.4% optimal bandwidth around the binding regulatory threshold. Columns (2)-(5) employ the same specification using alternative bandwidths within 8%, 9%, 11%, and 12% the binding regulatory threshold, respectively. All specifications are estimated using a linear probability model and include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 4: Second stage. Effect of reporting regulation on private firms' bank credit

	(1)	(2)	(3)	(4)	(5)
			Credit/Assets		
	10.4%	8%	9%	11%	12%
$\widehat{Mixed}$	0.286**	0.309**	0.299**	0.281**	0.272**
	(0.129)	(0.148)	(0.139)	(0.123)	(0.115)
LDT2	-0.339	-0.411	-0.360	-0.352	-0.341
	(0.259)	(0.346)	(0.299)	(0.241)	(0.213)
LDT2*Medium	-0.731	-0.740	-0.769	-0.672	-0.636*
	(0.474)	(0.687)	(0.587)	(0.431)	(0.382)
Observations	3,797	2,959	3,285	3,987	4,366
R-squared	0.103	0.084	0.091	0.109	0.120
Controls	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm

This table presents the second-stage estimates of the effect of reporting regulation on private firms' bank credit. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variable, $Credit/Assets$, corresponds to the ratio of bank credit to total assets. The main independent variable, $\widehat{Mixed}$, corresponds to the predicted values of $Mixed$ from Equation (2), where $Mixed$ is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. The estimation is conducted using a 2SLS procedure, where $Mixed$ is the instrumented variable and $Medium$ is the instrument. Column (1) presents the results for firms with size within a 10.4% optimal bandwidth around the binding regulatory threshold. Columns (2)-(5) employ the same specification using alternative bandwidths within 8%, 9%, 11% and 12% the binding regulatory threshold, respectively. All specifications include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 5: Placebo period and DiD

	(1) RD 2012-2015	(2) RD 2016-2019	(3) DiD
Panel A: First stage: Dependent variable *Mixed*			
Medium	0.008	0.138***	0.008
	(0.041)	(0.034)	(0.040)
LDT2	1.188**	0.438	1.188**
	(0.528)	(0.410)	(0.526)
LDT2*Medium	0.290	2.124***	0.290
	(0.796)	(0.670)	(0.792)
Medium*Post			0.130**
			(0.053)
Post			-0.235
			(0.312)
Panel B: Reduced form: Dependent variable *Credit/Assets*			
Medium	-0.010	0.040**	-0.010
	(0.017)	(0.015)	(0.017)
LDT2	0.095	-0.213	0.095
	(0.221)	(0.189)	(0.220)
LDT2*Medium	-0.209	-0.123	-0.209
	(0.309)	(0.298)	(0.308)
Medium*Post			0.050**
			(0.023)
Post			0.231
			(0.161)
Observations	2,590	3,797	6,387
Controls	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes
Cluster	Firm	Firm	Firm

This table presents the results from the effect of reporting regulation on the propensity to disclose more detailed financial statements (1-stage) and the effect on the ratio of bank credit to assets (reduced form), in panels A and B, respectively. In panel A, the dependent variable is *Mixed*, which is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. In panel B, the dependent variable, *Credit/Assets*, corresponds to the ratio of bank credit to total assets. Column (1) displays the results of the RDD specification for the placebo period over 2012-2015. Column (2) displays the results of the RDD specification over the sample period used in the main analyses, 2016-2019. Column (3) reports the results from a DiD estimation over 2012-2019. Details of the DiD specification are provided in section C of the Appendix. *Post* is and indicator that equals one for fiscal years starting in 2016, and zero otherwise. All estimations are conducted for firms with size within a 10.4% bandwidth around the binding regulatory threshold. Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 6: Robustness. Coefficient of $\widehat{Mixed}$ for alternative regression specifications (2-stage)

	(1)	(2)	(3)	(4)	(5)
	Credit/Assets				
	10.4%	8%	9%	11%	12%
Linear	0.293*	0.314*	0.312*	0.284*	0.272*
	(0.160)	(0.190)	(0.177)	(0.152)	(0.141)
Linear+f(Size)	0.266*	0.287*	0.284*	0.260*	0.248*
	(0.146)	(0.173)	(0.161)	(0.139)	(0.130)
Linear+f(Size)+Controls	0.293**	0.304**	0.301**	0.287**	0.277**
	(0.129)	(0.147)	(0.139)	(0.123)	(0.115)
Linear+f(Size)+Controls+FE	0.286**	0.309**	0.299**	0.281**	0.272**
	(0.129)	(0.148)	(0.139)	(0.123)	(0.115)
Kernel: Uniform	0.250**	0.332**	0.264**	0.246**	0.227**
	(0.110)	(0.146)	(0.127)	(0.104)	(0.101)
Kernel: Epachenikov	0.284**	0.296**	0.293**	0.276**	0.264**
	(0.125)	(0.142)	(0.135)	(0.119)	(0.111)
No Cluster	0.286**	0.309**	0.299**	0.281**	0.272**
	(0.126)	(0.146)	(0.137)	(0.121)	(0.113)
Cluster: 3-Digit Zip Code	0.286**	0.309*	0.299**	0.281**	0.272**
	(0.137)	(0.166)	(0.152)	(0.130)	(0.120)
Cluster: 2-Digit Industry	0.286**	0.309**	0.299**	0.281**	0.272**
	(0.121)	(0.131)	(0.124)	(0.118)	(0.111)
Cluster: (1-Digit Ind)*(Year)	0.286**	0.309***	0.299**	0.281**	0.272**
	(0.126)	(0.118)	(0.118)	(0.126)	(0.122)
Winsor 2.5% Credit/Assets	0.238**	0.260**	0.251**	0.231**	0.222**
	(0.116)	(0.133)	(0.125)	(0.110)	(0.103)
All Loan Types	0.356**	0.428**	0.398**	0.346**	0.334**
	(0.167)	(0.200)	(0.185)	(0.159)	(0.148)

This table presents the second-stage estimates of the effect of reporting regulation on private firms' bank credit employing alternative regression specifications. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variable, $Credit/Assets$, corresponds to the ratio of bank credit to total assets. The main independent variable, $\widehat{Mixed}$, corresponds to the predicted values of $Mixed$ from Equation (2), where $Mixed$ is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. The estimation is conducted using a 2SLS procedure, where $Mixed$ is the instrumented variable and $Medium$ is the instrument. Column (1) presents the results for firms with size within a 10.4% optimal bandwidth around the binding regulatory threshold. Columns (2)-(5) employ the same specification using alternative bandwidths within 8%, 9%, 11% and 12% the binding regulatory threshold, respectively. All specifications include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 7: RDD specification using big and medium firms

	(1) 10.4%	(2) 8%	(3) 9%	(4) 11%	(5) 12%
Big	0.082*	0.116**	0.099**	0.079*	0.076*
	(0.045)	(0.052)	(0.048)	(0.044)	(0.043)
$\widehat{Detailed}$	0.186	-0.009	0.047	0.234	0.280
	(0.349)	(0.260)	(0.295)	(0.366)	(0.400)
Observations	1,011	794	890	1,012	1,012
Controls	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm
F-statistic	4.43	6.71	5.55	4.11	3.75

This table presents the first- and second-stage estimates from the main specification using the sample of big and medium firms around the size cutoff. The first row corresponds to the first-stage regression, while the second row corresponds to the second-stage regression. The dependent variable in the first-stage regression is *Detailed*, which is an indicator variable that equals one if the firm publicly discloses the detailed model, and zero if it publicly discloses the mixed or the abbreviated model. The dependent variable in the second-stage regression is $Credit/Assets$, which corresponds to the ratio of bank credit to assets. *Big* is an indicator variable that equals one if the firm is classified as big, and zero if it is classified as medium. $\widehat{Detailed}$, corresponds to the predicted values of *Detailed*. Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 8: Mechanism

	(1)	(2)	(3)
	Nbanks	Credit/Assets	
		<Median(HHI)	≥Median(HHI)
$\widehat{Mixed}$	3.418**	0.380**	0.174
	(1.635)	(0.178)	(0.190)
Observations	3,797	1,900	1,897
Controls	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes
Cluster	Firm	Firm	Firm

This table presents the second-stage estimates of the effect of reporting regulation on the number of banks a firm has a relationship with, in column (1), and of the effect of reporting regulation on private firms' bank credit on two subsamples split by the median HHI (0.145), in columns (2) and (3). The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. *Nbanks* is the number of different banks with outstanding debt with the firm. *Credit/Assets* corresponds to the ratio of bank credit to total assets. The main independent variable, $\widehat{Mixed}$, corresponds to the predicted values of *Mixed* from Equation (2), where *Mixed* is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. All specifications are estimated on a 10.4% around the binding regulatory threshold and include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 9: Effect of reporting regulation on alternative financing sources

	(1) Log(Assets)	(2) Equity/Assets	(3) Liab/Assets	(4) Cash/Assets
$\widehat{Mixed}$	0.155	-0.028	0.017	-0.038
	(0.132)	(0.043)	(0.082)	(0.058)
Observations	2,227	2,227	2,227	2,227
Controls	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm

This table presents the second-stage estimates of the effect of reporting regulation on alternative financing sources. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variables are the logarithm of the firm's assets in column (1), the ratio of equity capital over total assets in column (2), the ratio of total liabilities over total assets in column (3), the ratio of cash plus short-term investments over total assets column (4). The main independent variable, $\widehat{Mixed}$, corresponds to the predicted values of $Mixed$ from Equation (2), where $Mixed$ is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. All specifications are estimated on a 10.4% around the binding regulatory threshold and include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table 10: Effect of reporting regulation on balance sheet liabilities

	(1) LTcredit/Assets	(2) STcredit/Assets	(3) Ot.Liab/Assets	(4) AP/Assets
$\widehat{Mixed}$	0.108*	0.016	-0.086*	0.054
	(0.065)	(0.075)	(0.052)	(0.081)
Observations	2,227	2,227	2,227	2,227
Controls	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm

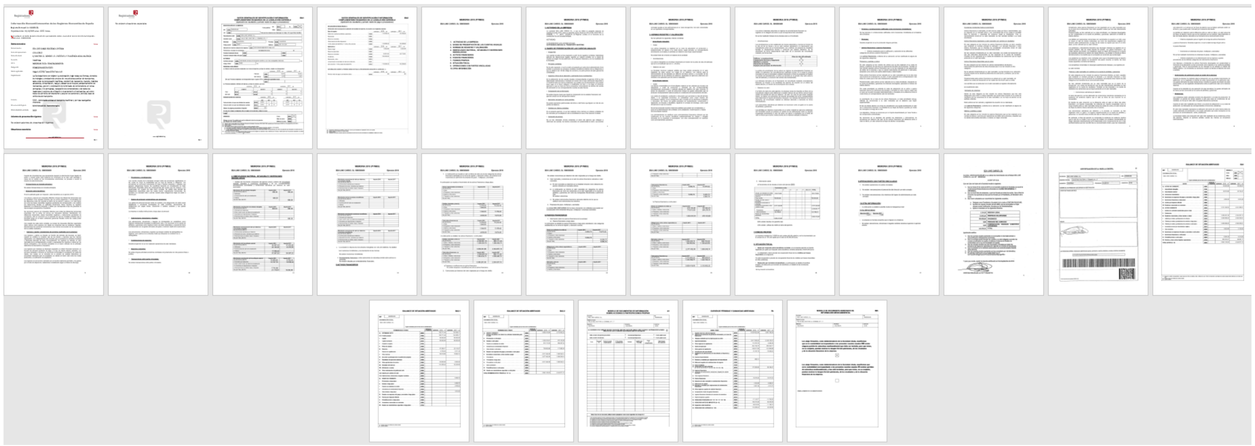
This table presents the second-stage estimates of the effect of reporting regulation on private firms' balance sheet liabilities. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variables are the ratio of long-term balance sheet debt over total assets in column (1), the ratio of short-term balance sheet debt over total assets in column (2), the ratio of other balance sheet liabilities over total assets in column (3), and the ratio of accounts payable over total assets in column (4). The main independent variable, $\widehat{Mixed}$, corresponds to the predicted values of $Mixed$ from Equation (2), where $Mixed$ is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. All specifications are estimated on a 10.4% around the binding regulatory threshold and include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Appendix

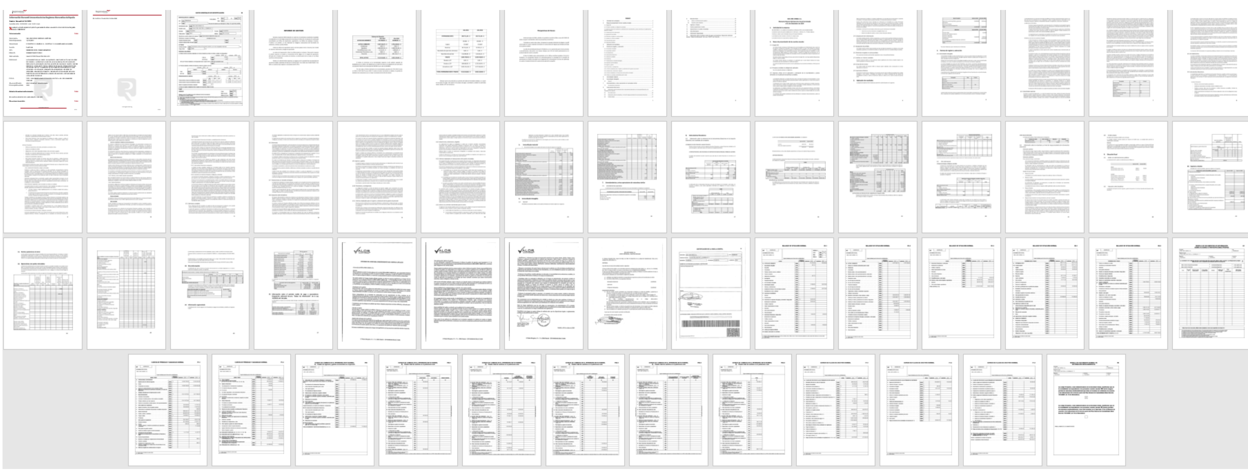
A Figures and Tables

Figure A1: Abbreviated and mixed financial statements

(a) Abbreviated



(b) Mixed

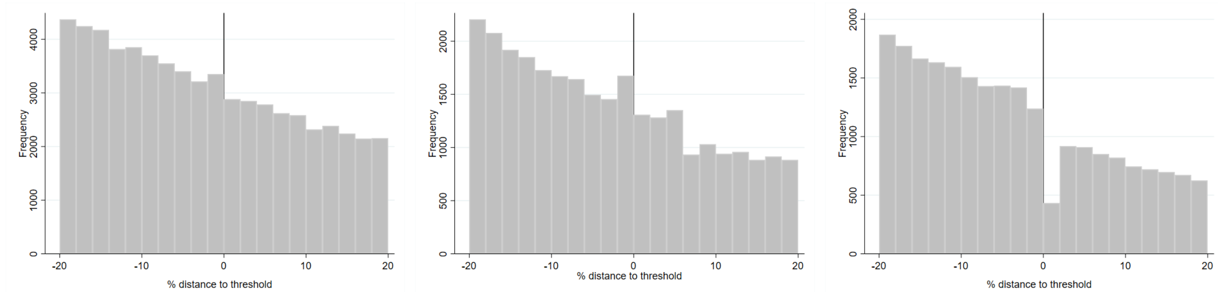


This figure presents the abbreviated financial statements of a firm classified as small in fiscal year 2018 (panel (a)), and the mixed financial statements of the same firm classified as medium in fiscal year 2019 (panel (b)). The figure illustrates the substantial increase in the public information disclosed. The number of pages submitted to the Commercial Registry in fiscal year 2018 is 29. The number of pages submitted to the Commercial Registry in fiscal year 2019 is 57.

Figure A2: Distribution of assets, sales, and employees around the regulatory thresholds over 2012-2015

Reporting/Auditing thresholds

(a) Assets = 2.85M　　**(b)** Sales = 5.7M　　**(c)** Emp = 50



T = 4.29***　　T = 5.87***　　T = -25.33***

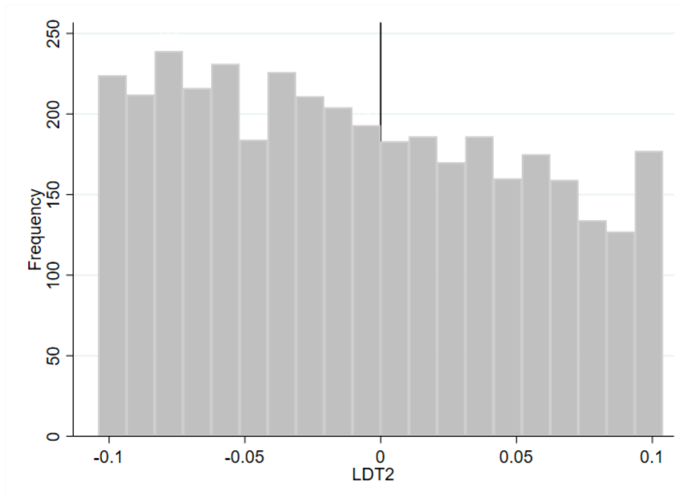
This figure presents the frequency distribution of the percentage distance of assets, sales, and employees scaled by the corresponding reporting and auditing regulatory threshold. The sample period is 2012-2015. Each bin has a width of 2% of the nominal value of the threshold as in Bernard et al. (2018). To test the significance of each discontinuity I use the standardized difference statistic from Burgstahler and Chuk (2015). The statistic is estimated as follows:

$$T = \frac{n_i - 0.5(n_{i-1} + n_{i+1})}{\sqrt{1.5n_i}}$$

Where n_i is the number of observations in bin i. T is normally distributed. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

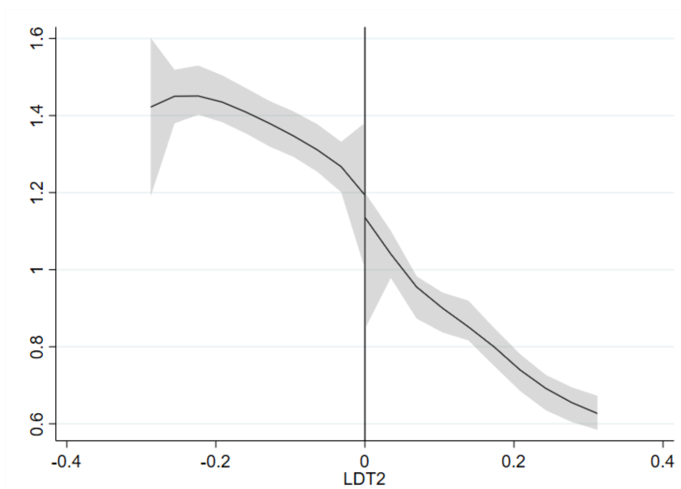
Figure A3: Distribution of the forcing variable and manipulation test

(a) Histogram of LDT2



T = -0.03

(b) Manipulation Test



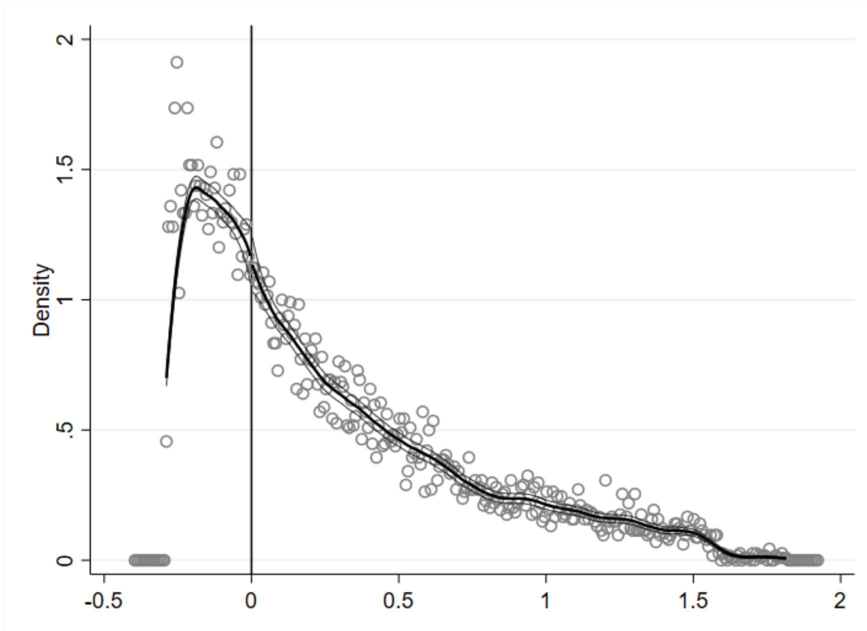
This figure presents the distribution of the least distance to the second threshold ($LDT2$) around the cutoff. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. Panel (a) plots the frequency distribution of LDT2 around the cutoff within the optimal bandwidth of 10.4%. To test the significance of the discontinuity at zero, I use the standardized difference statistic from Burgstahler and Chuk (2015). The statistic is estimated as follows:

$$T = \frac{n_i - 0.5(n_{i-1} + n_{i+1})}{\sqrt{1.5 n_i}}$$

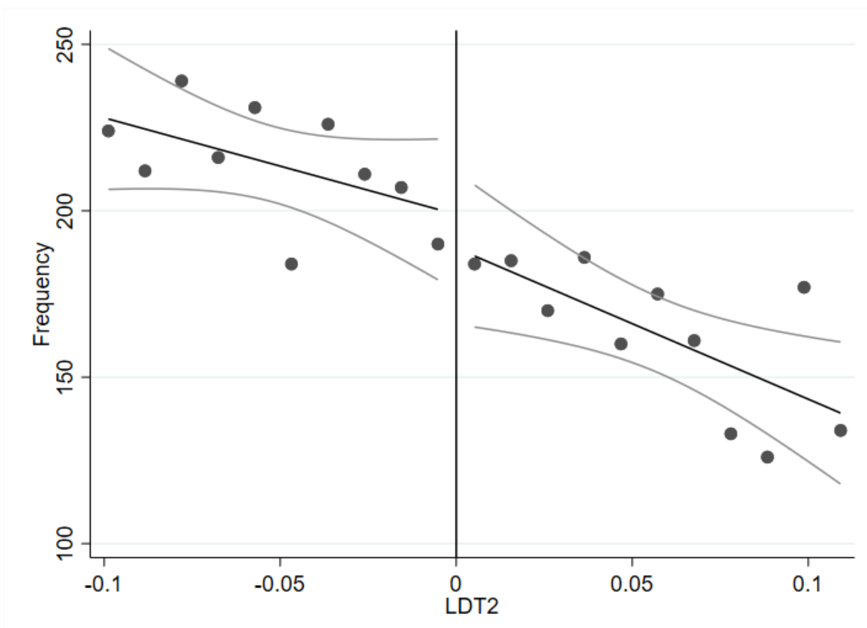
Where n_i is the number of observations in bin i. T is normally distributed. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively. Panel (b) shows the results of the manipulation test developed by Cattaneo et al. (2018). The solid lines are estimated using local polynomial density estimators at each side of the cutoff, with the corresponding 95% confidence intervals, testing for a discontinuity in the density at zero under the null hypothesis of no discontinuity. An optimal bandwidth of 10.4% around the cutoff is used for the estimation. The T-statistic from the test is -1.18 with a p-value of 0.24.

Figure A4: McCrary plots

(a) McCrary (2008) plot

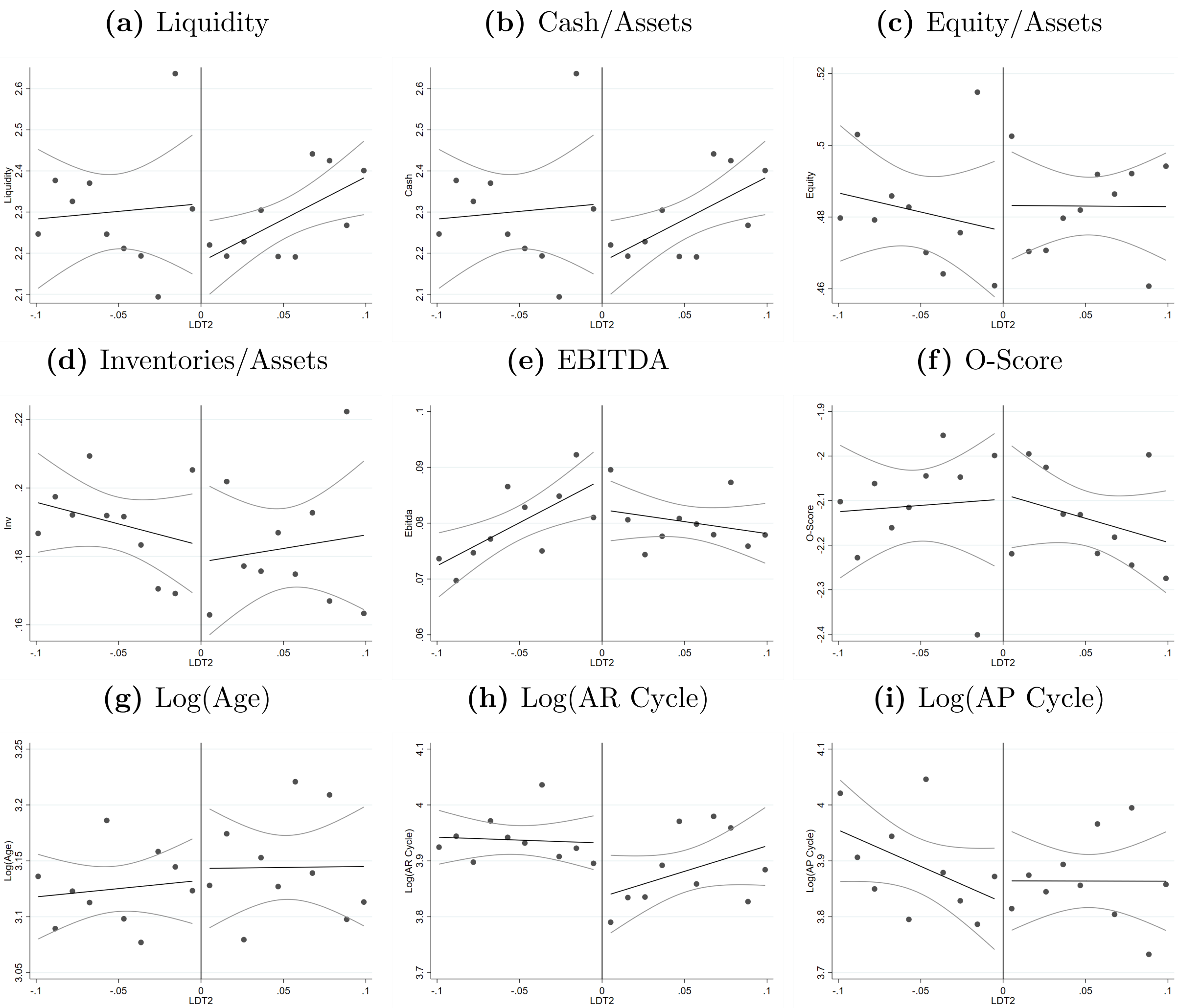


(b) Manual McCrary plot



This figure shows the distribution of the least distance to the second threshold (LDT2) around the cutoff. Panel (a) plots the results of the manipulation test developed by McCrary (2008). The solid line plots the estimated values from a local linear regression of density on LDT2, with separate estimations on either side of the cutoff. The optimal bandwidth used for the estimation is 10.4%. The log difference in height is -0.007, with a corresponding standard error of 0.07 (T-stat=-0.10). Panel (b) plots the frequency distribution around the cutoff within the optimal bandwidth of 10.4%. Each dot represents the number of observations in equal-sized bins at either side of the cutoff. The solid lines correspond to a linear fit generated separately at each side of the cutoff, with the corresponding 90% confidence intervals. A formal parametric regression shows that the log difference in the number of observations at the threshold is -0.042, with a corresponding standard error of 0.083 (T-stat=-0.50).

Figure A5: Balance of covariates



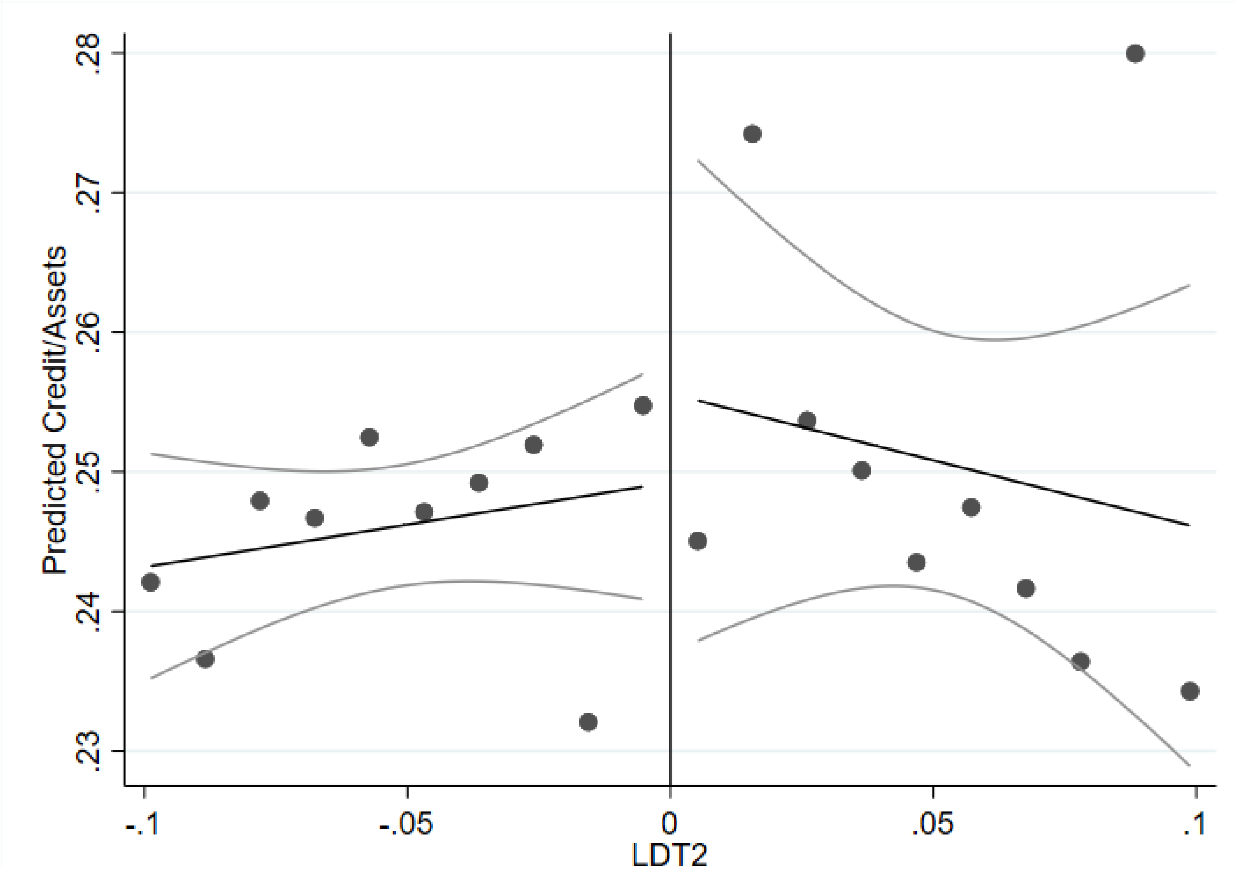
(a) Liquidity **(b)** Cash/Assets **(c)** Equity/Assets

(d) Inventories/Assets **(e)** EBITDA **(f)** O-Score

(g) Log(Age) **(h)** Log(AR Cycle) **(i)** Log(AP Cycle)

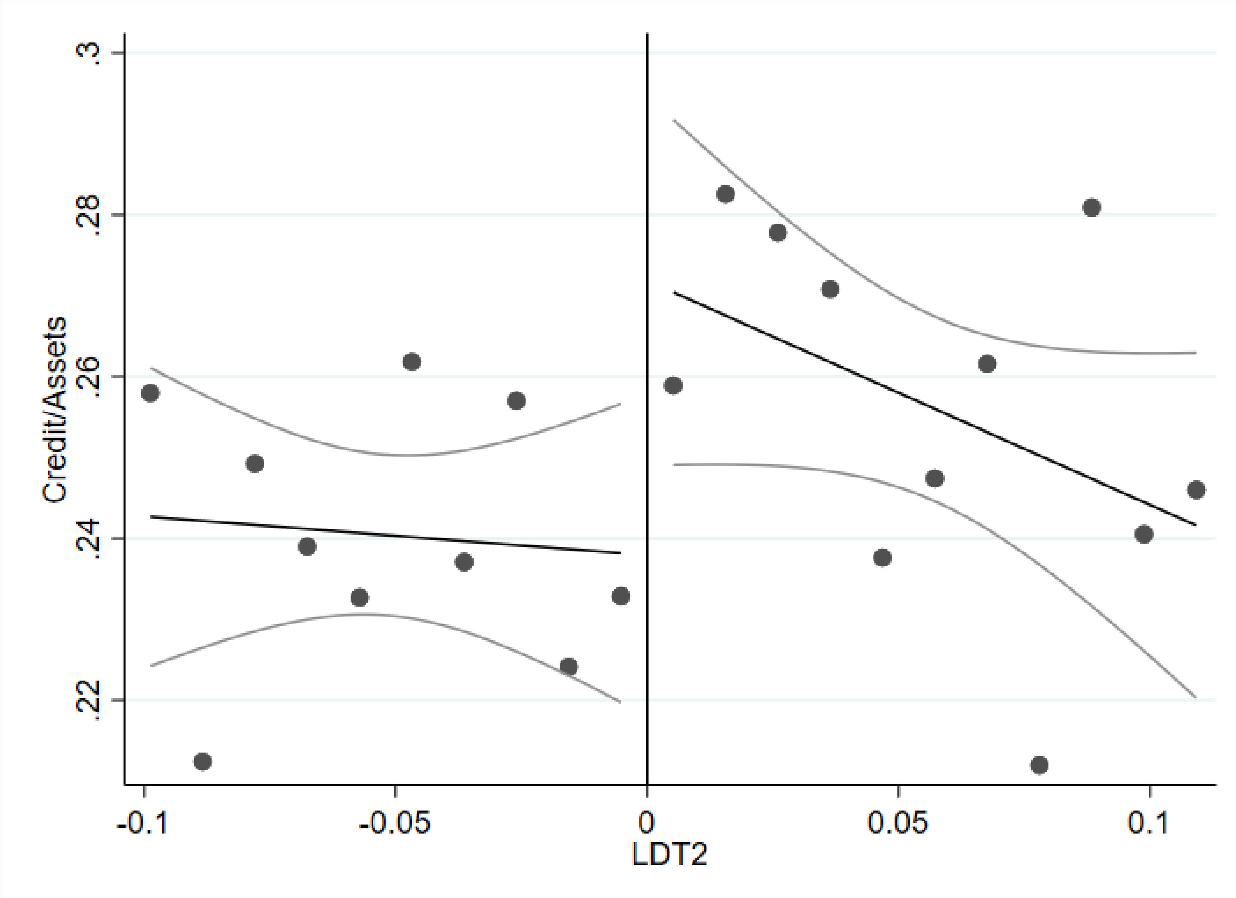
This figure plots the distribution of the firm-level covariates over the least distance to the second threshold ($LDT2$), in a 10.4% bandwidth around the cutoff. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. Each dot represents the average value of the firm-level covariate in equal-sized bins at each side of the cutoff. Dots to the left of 0 represent small firms, while dots to the right of 0 represent medium firms. The solid lines correspond to a linear fit generated separately at each side of the threshold, with the corresponding 90% confidence intervals.

Figure A6: Balance of predicted credit to assets



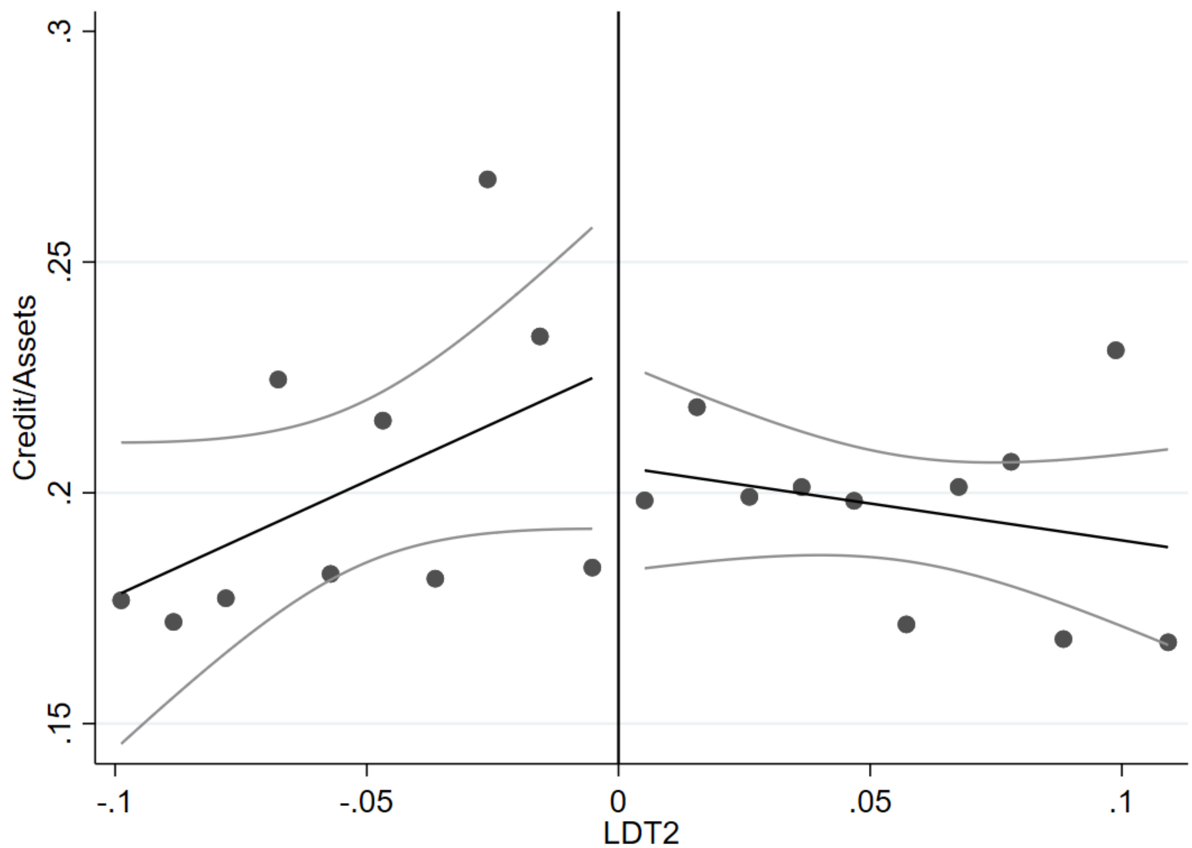
This figure plots the distribution of predicted bank credit to assets over the least distance to the second threshold ($LDT2$), in a 10.4% bandwidth around the cutoff. Credit to assets is predicted using the covariates described in Panel B of Table 2 and industry-year and region-year fixed effects. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. Each dot represents the average value of the firm-level covariate in equal-sized bins at each side of the cutoff. Dots to the left of 0 represent small firms, while dots to the right of 0 represent medium firms. The solid lines correspond to a linear fit generated separately at each side of the threshold, with the corresponding 90% confidence intervals.

Figure A7: Reduced form. Bank credit to Assets



This figure plots the distribution of the ratio of bank credit to total assets ($Credit/Assets$) over the least distance to the second threshold ($LDT2$) in a 10.4% bandwidth around the cutoff. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. Each dot represents the average value of bank credit over total assets in equal-sized bins at each side of the cutoff. Dots to the left of 0 represent small firms, while dots to the right of 0 represent medium firms. The solid lines correspond to a linear fit generated separately at each side of the threshold, with the corresponding 90% confidence intervals.

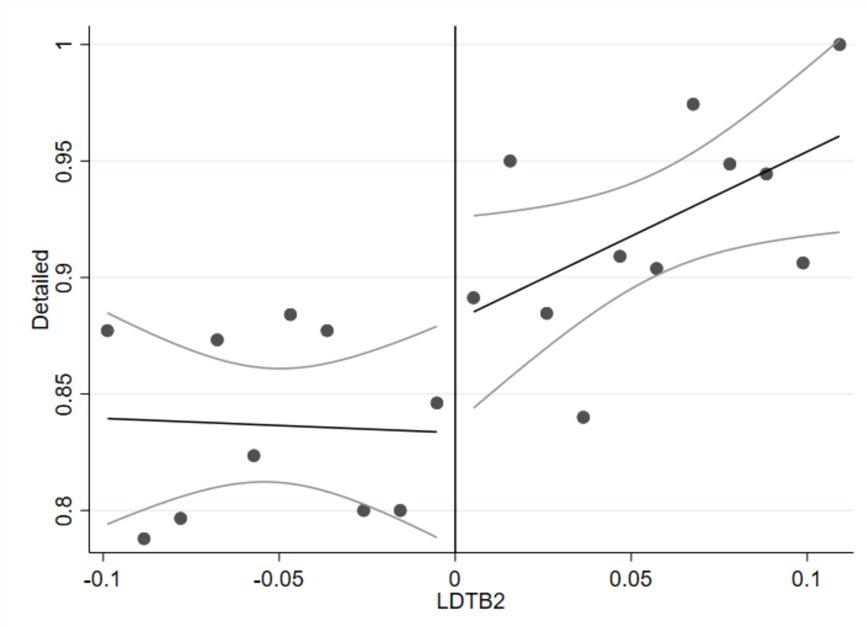
Figure A8: Reduced form. Bank credit to assets over 2012-2015



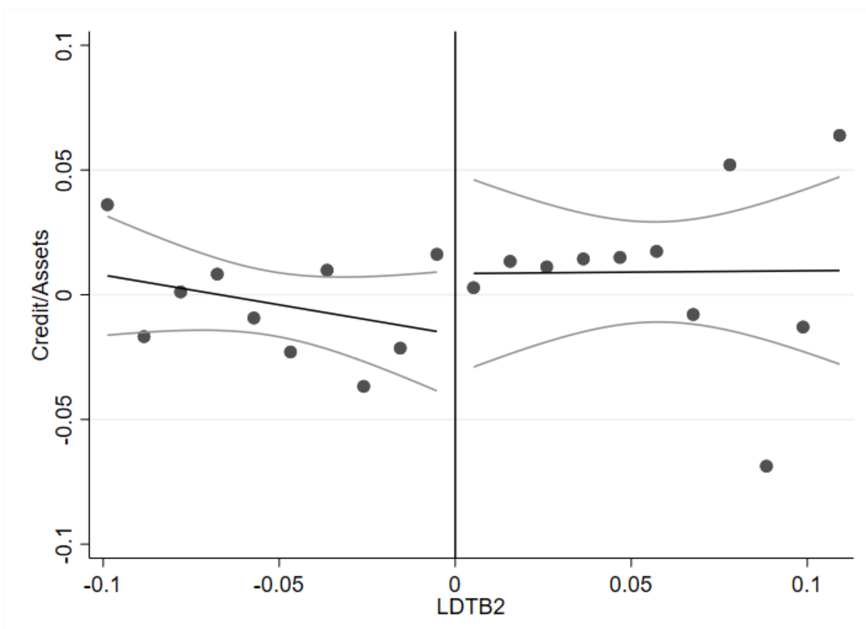
This figure plots the distribution of the ratio of bank credit to total assets ($Credit/Assets$) over the least distance to the second threshold ($LDT2$) in a 10.4% bandwidth around the cutoff. The sample period is 2012-2015, when the reporting thresholds described in Table 1 were not applicable. Each dot represents the average value of bank credit over total assets in equal-sized bins at each side of the cutoff. Dots to the left of 0 represent small firms, while dots to the right of 0 represent medium firms. The solid lines correspond to a linear fit generated separately at each side of the threshold, with the corresponding 90% confidence intervals.

Figure A9: RD plots using big and medium firms

(a) First stage. Probability of disclosing more detailed financial statements

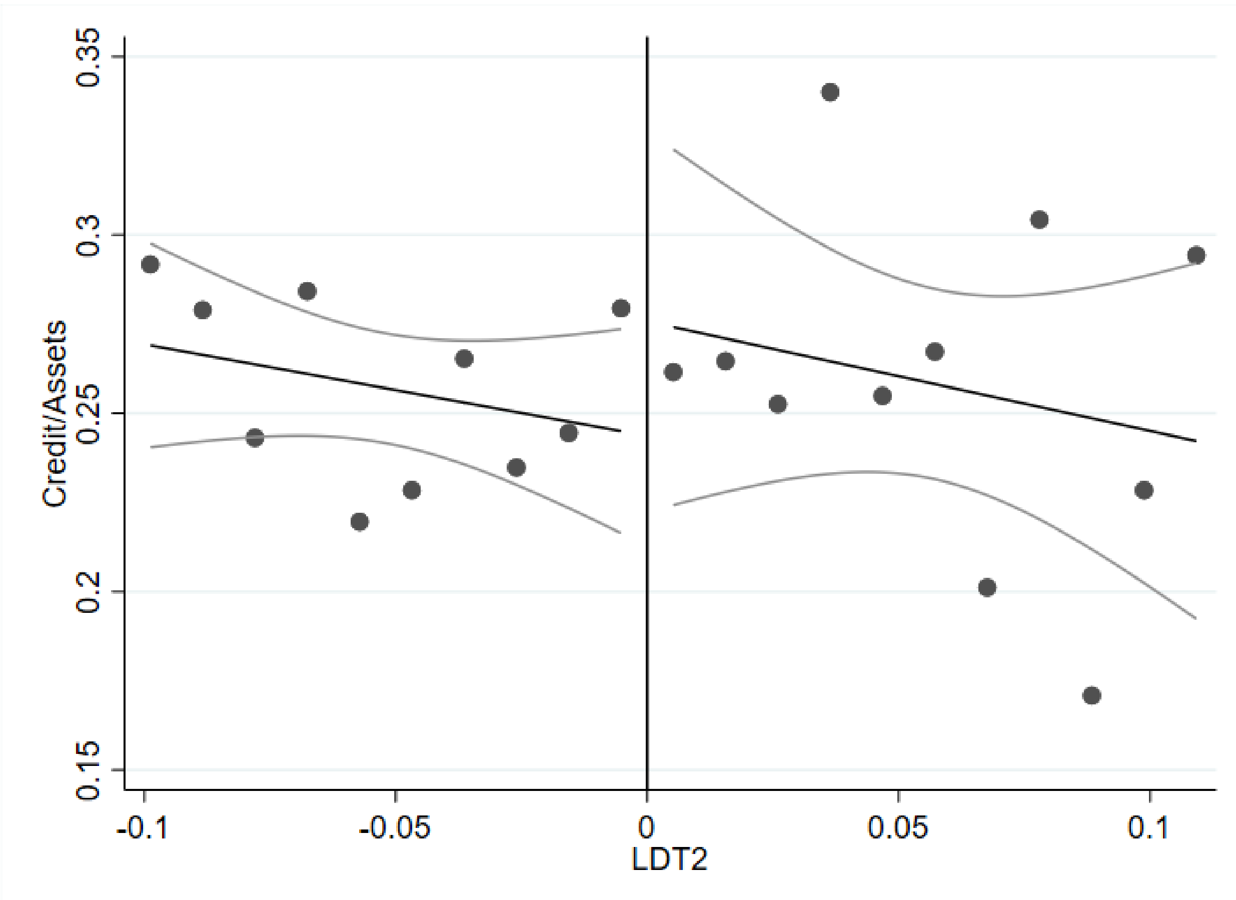


(b) Reduced form. Residualized credit to assets



This figure plots the distribution of big and medium firms of the propensity to publicly disclose detailed financial statements (*Detailed*) and the residualized ratio of bank credit to total assets ($Credit/Assets$) over the least distance to the second threshold ($LDT2$), in panel (a) and panel (b), respectively, in a 10.4% bandwidth around the cutoff. The sample period is 2016-2019. Residualized firm-level bank credit to assets corresponds to the residuals from a regression of bank credit to assets as the dependent variable on all the controls and fixed effects from the main specification other than Big, $LDT2$, and the respective interaction. Each dot represents the average value of the firm-level outcome in equal-sized bins at each side of the cutoff. Dots to the left of 0 represent small firms, while dots to the right of 0 represent medium firms. The solid lines correspond to a linear fit generated separately at each side of the threshold, with the corresponding 90% confidence intervals.

Figure A10: RD plot using big and medium firms. Reduced form. Credit to assets



This figure plots the distribution of big and medium firms of the ratio of bank credit to total assets ($Credit/Assets$) over the least distance to the second threshold ($LDT2$) in a 10.4% bandwidth around the cutoff. The sample period is 2016-2019. Each dot represents the average value of the firm-level outcome in equal-sized bins at each side of the cutoff. Dots to the left of 0 represent small firms, while dots to the right of 0 represent medium firms. The solid lines correspond to a linear fit generated separately at each side of the threshold, with the corresponding 90% confidence intervals.

Table A1: Variable definition

Variables	Definition
Dependent Variables	
$Credit/Assets$	The ratio of the firm's outstanding credit from assets-based and cash-flow-based loans reported in the CIR in December in $t+1$ over total assets in t.
$Mixed$	An indicator variable equal to one if the firm discloses detailed or mixed financial statements, and zero if it discloses abbreviated financial statements. Detailed, mixed, and abbreviated financial statements are defined in Table 1.
$Detailed$	An indicator variable equal to one if the firm discloses detailed financial statements, and equal to zero if the firm publicly discloses mixed or abbreviated financial statements. Detailed, mixed, and abbreviated financial statements are defined in Table 1.
$Log(Assets)$	The logarithm of one plus the firm's total assets in $t+1$.
$Equity/Assets$	The ratio of the balance sheet equity capital in $t+1$ over total assets in t.
$Liab/Assets$	The ratio of the balance sheet total liabilities in $t+1$ over total assets in t.
$Cash/Assets$	The ratio of the balance sheet cash and equivalents plus short-term investments in $t+1$ over total assets in t.
$LTcredit/Assets$	The ratio of the balance sheet long-term debt in $t+1$ over total assets in t.
$STcredit/Assets$	The ratio of the balance sheet short-term debt in $t+1$ over total assets in t.
$Ot.Liab/Assets$	The ratio of unspecified balance sheet liabilities in $t+1$ over total assets in t. These liabilities can include creditors for services rendered, advances from customers, remunerations pending payment, or public treasury creditors, among others. These liabilities do not include bank debt.
$AP/Assets$	The ratio of the balance sheet accounts payable in $t+1$ over total assets in t.
Independent Variables	
$Medium$	An indicator variable equal to one if the firm is classified as medium, and equal to zero if the firm is classified as small. The size classification of firms is presented in Table 1.
Big	An indicator variable equal to one if the firm is classified as big, and equal to zero if the firm is classified as medium. The size classification of firms is presented in Table 1.
$\widehat{Mixed}$	The predicted values of $Mixed$ from Equation (2).

$\widehat{Detailed}$	The predicted values of *Detailed* from Equation (2) replacing *Medium* for *Big*, and *Mixed* for *Detailed* in Equation (2).
$LDT1$	The highest value among the percentage distances to the three regulatory thresholds (assets, sales, employees), minus one.
$LDT2$	The second-highest value among the percentage distances to the three regulatory thresholds (assets, sales, employees), minus one.
$LDT3$	The third-highest value among the percentage distances to the three regulatory thresholds (assets, sales, employees), minus one.
I^1	An indicator variable equal to one if the firm exceeds at least one size-based regulatory threshold.
I^3	An indicator variable equal to one if the firm exceeds the three size-based regulatory thresholds.
Liquidity	The ratio of current assets in t over current liabilities in t.
Cash/Assets	The ratio of the balance sheet cash and equivalents plus short-term investments in t over total assets in t.
Equity/Assets	The ratio of the balance sheet equity capital in t over total assets in t.
Inventories/Assets	The ratio of the balance sheet inventories in t over total assets in t.
EBITDA	The ratio of EBITDA in t over total sales in t.
O-Score	= -1.32 - 0.407Log(Assets) + 6.03(Liab/Assets) - 1.43(Working Capital/Assets) + 0.076Liquidity - 1.72(1 if total liabilities>total assets, 0 otherwise) - 0.521($(netincome_t - netincome_{t-1})/(\|netincome_t\| + \|netincome_{t-1}\|)$) in t.
Log(Age)	The logarithm of one plus the firm's age in t.
Log(AR Cycle)	The logarithm of one plus the firm's average customer collection period in t.
Log(AP Cycle)	The logarithm of one plus the firm's average supplier payment period in t.
$\widehat{Credit/Assets}$	The predicted values of a regression of *Credit/Assets* on all the covariates from Panel B of Table 2.
Assets	The total assets of the firm in t in millions of euros.
Sales	The total sales of the firm in t in millions of euros.
Emp	The total number of employees of the firm in t.
Nbanks	The number of different banks with outstanding assets-based or cash-flow-based loans with the firm in $t + 1$.
HHI	The Herfindahl-Hirschman index measured as the sum of the squared bank market shares at the 2-digit zip code level in December of 2016.

Table A2: Reduced-form results

	(1)	(2)	(3)	(4)	(5)
			Credit/Assets		
Medium	0.038**	0.039**	0.038**	0.041***	0.039**
	(0.019)	(0.019)	(0.019)	(0.016)	(0.015)
LDT2	-0.205	-0.079	-0.072	-0.230	-0.206
	(0.180)	(0.229)	(0.233)	(0.190)	(0.189)
LDT2*Medium		-0.286	-0.256	-0.054	-0.131
		(0.355)	(0.361)	(0.301)	(0.298)
Observations	3,797	3,797	3,797	3,797	3,797
R-squared	0.002	0.003	0.003	0.292	0.336
Size Controls	No	No	Yes	Yes	Yes
Other Controls	No	No	No	Yes	Yes
Industry-year FE	No	No	No	No	Yes
Region-year FE	No	No	No	No	Yes
Cluster	Firm	Firm	Firm	Firm	Firm

This table presents the reduced-form estimates of the effect of reporting regulation on private firms' bank credit, within an optimal bandwidth of 10.4% around the cutoff. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variable is the ratio of bank credit to total assets $Credit/Assets$. The main independent variable is $Medium$, which equals one if the firm is classified as medium, and zero if it is classified as small. Table A1 provides a description of all the variables used in the models. Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table A3: Placebo tests using arbitrary cutoffs

	(1) -20%	(2) 20%	(3) 40%	(4) 60%	(5) 80%
Medium *(1-stage)*	0.008	0.020	0.005	0.006	0.001
	(0.026)	(0.026)	(0.017)	(0.013)	(0.011)
Medium *(Reduced-Form)*	0.002	-0.021	0.014	0.004	-0.014
	(0.015)	(0.017)	(0.019)	(0.023)	(0.023)
Observations	3,830	3,034	2,564	2,119	1,679
Controls	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm

This table presents the first-stage and reduced-form estimates from the main specification using arbitrary placebo cutoffs set at different values of $LDT2$. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The first row corresponds to the first-stage regression, while the second row corresponds to the reduced-form regression. In column (1) the value of the threshold is set at $LDT2 = -0.2 = -20\%$. Columns (2)-(5) set different values in increments of $0.2 = 20\%$ (except for the value of zero). The dependent variable in the first-stage regression is $Mixed$, which is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. The dependent variable in the reduced-form regression is $Credit/Assets$, which corresponds to the ratio of bank credit to total assets. $Medium$ is an indicator variable that equals one if the firm is classified as medium, and zero if it is classified as small. The different specifications use a 10.4% optimal bandwidth. All specifications include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table A4: Robustness. Coefficient of $Medium$ for alternative regression specifications (1-stage)

	(1)	(2)	(3)	(4)	(5)
			Mixed		
	10.4%	8%	9%	11%	12%
Linear	0.132***	0.126***	0.129***	0.134***	0.138***
	(0.034)	(0.038)	(0.036)	(0.033)	(0.031)
Linear+f(Size)	0.142***	0.136***	0.139***	0.145***	0.148***
	(0.034)	(0.038)	(0.036)	(0.033)	(0.031)
Linear+f(Size)+Controls	0.141***	0.139***	0.140***	0.143***	0.145***
	(0.033)	(0.037)	(0.035)	(0.032)	(0.030)
Linear+f(Size)+Controls+FE	0.138***	0.138***	0.138***	0.140***	0.142***
	(0.034)	(0.038)	(0.036)	(0.033)	(0.031)
Kernel: Uniform	0.151***	0.138***	0.143***	0.155***	0.152***
	0.031	0.035	0.034	0.030	0.029
Kernel: Epachenikov	0.143***	0.142***	0.143***	0.146***	0.149***
	0.033	0.037	0.035	0.032	0.031
No Cluster	0.138***	0.138***	0.138***	0.140***	0.142***
	(0.033)	(0.038)	(0.036)	(0.032)	(0.031)
Cluster: 3-Digit Zip Code	0.138***	0.138***	0.138***	0.140***	0.142***
	(0.035)	(0.041)	(0.039)	(0.034)	(0.032)
Cluster: 2-Digit Industry	0.138***	0.138***	0.138***	0.140***	0.142***
	(0.034)	(0.036)	(0.035)	(0.033)	(0.031)
Cluster: (1-Digit Ind)*(Year)	0.138***	0.138***	0.138***	0.140***	0.142***
	(0.035)	(0.038)	(0.037)	(0.034)	(0.033)
Winsor 2.5% Credit/Assets	0.138***	0.138***	0.138***	0.140***	0.142***
	(0.034)	(0.038)	(0.036)	(0.033)	(0.031)
All Loan Types	0.138***	0.138***	0.138***	0.140***	0.142***
	(0.034)	(0.038)	(0.036)	(0.033)	(0.031)

This table presents the first-stage estimates of the effect of reporting regulation on the probability of disclosing the mixed or detailed model employing alternative regression specifications. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variable, $Mixed$, corresponds to the ratio of bank credit to total assets. The main independent variable, $Medium$, equals one if the firm is classified as Medium, and zero if it is small. Column (1) presents the results for firms with size within a 10.4% optimal bandwidth around the binding regulatory threshold. Columns (2)-(5) employ the same specification using alternative bandwidths within 8%, 9%, 11%, and 12% the binding regulatory threshold, respectively. All specifications include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table A5: Robustness. Alternative polynomials and sample size

	(1) 20.8 Quadratic	(2) 20.8 Cubic	(3) 20.8 Quartic
Medium	0.137***	0.145***	0.119**
	(0.034)	(0.046)	(0.057)
$\widehat{Mixed}$	0.307**	0.287*	0.318
	(0.138)	(0.166)	(0.254)
Observations	7,361	7,361	7,361
Controls	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes
Cluster	Firm	Firm	Firm

This table presents the first- and second-stage estimates from the main specification fitting quadratic, cubic, and quartic polynomials on a 20.8% bandwidth around the threshold. The first row corresponds to the first-stage regression, while the second row corresponds to the second-stage regression. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variable in the first-stage regression is $Mixed$, which is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. The dependent variable in the second-stage regression is $Credit/Assets$, which corresponds to the ratio of bank credit to assets. $Medium$ is an indicator variable that equals one if the firm is classified as medium, and zero if it is classified as small. $\widehat{Mixed}$, corresponds to the predicted values of $Mixed$ from Equation (2). All specifications include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table A6: Robustness. Sample of firms with the employee-based threshold not binding

	(1) 10.4%	(2) 8%	(3) 9%	(4) 11%	(5) 12%
Medium	0.167***	0.169***	0.169***	0.168***	0.168***
	(0.037)	(0.043)	(0.040)	(0.036)	(0.034)
$\widehat{Mixed}$	0.197*	0.184	0.190	0.201*	0.204**
	(0.111)	(0.123)	(0.117)	(0.108)	(0.103)
Observations	3,098	2,410	2,697	3,267	3,575
Controls	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm

This table presents the first- and second-stage estimates from the main specification excluding the sample of firms whose binding threshold is the employee-based threshold. The first row corresponds to the first-stage regression, while the second row corresponds to the second-stage regression. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variable in the first-stage regression is $Mixed$, which is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. The dependent variable in the second-stage regression is $Credit/Assets$, which corresponds to the ratio of bank credit to assets. $Medium$ is an indicator variable that equals one if the firm is classified as medium, and zero if it is classified as small. $\widehat{Mixed}$, corresponds to the predicted values of $Mixed$ from Equation (2). Column (1) presents the results for firms with size within a 10.4% optimal bandwidth around the binding regulatory threshold. Columns (2)-(5) employ the same specification using alternative bandwidths within 8%, 9%, 11%, and 12% the binding regulatory threshold, respectively. All specifications include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table A7: Robustness. Sample of firms exceeding only one or two size-based thresholds

	(1) 10.4%	(2) 8%	(3) 9%	(4) 11%	(5) 12%
Medium	0.143***	0.143***	0.143***	0.145***	0.146***
	(0.034)	(0.039)	(0.037)	(0.033)	(0.031)
$\widehat{Mixed}$	0.276**	0.288**	0.279**	0.275**	0.273**
	(0.125)	(0.141)	(0.133)	(0.120)	(0.113)
Observations	3,551	2,814	3,105	3,718	4,035
Controls	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm

This table presents the first- and second-stage estimates from the main specification excluding the sample of firms exceeding either none or the three size-based thresholds. The first row corresponds to the first-stage regression, while the second row corresponds to the second-stage regression. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variable in the first-stage regression is $Mixed$, which is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. The dependent variable in the second-stage regression is $Credit/Assets$, which corresponds to the ratio of bank credit to assets. $Medium$ is an indicator variable that equals one if the firm is classified as medium, and zero if it is classified as small. $\widehat{Mixed}$, corresponds to the predicted values of $Mixed$ from Equation (2). Column (1) presents the results for firms with size within a 10.4% optimal bandwidth around the binding regulatory threshold. Columns (2)-(5) employ the same specification using alternative bandwidths within 8%, 9%, 11%, and 12% the binding regulatory threshold, respectively. All specifications include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table A8: Robustness. Different bandwidth at each side of the threshold

	(1)	(2)	(3)	(4)	(5)
			Credit/Assets		
Medium	0.104***	0.130***	0.139***	0.132***	0.130***
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
$\widehat{Mixed}$	0.274*	0.292*	0.266*	0.287**	0.278**
	(0.150)	(0.160)	(0.146)	(0.127)	(0.126)
Observations	4,316	4,316	4,316	4,316	4,316
R-squared	0.160	0.164	0.178	0.213	0.236
LDT2*Medium	No	Yes	Yes	Yes	Yes
Size Controls	No	No	Yes	Yes	Yes
Controls	No	No	No	Yes	Yes
Industry-year FE	No	No	No	No	Yes
Region-year FE	No	No	No	No	Yes
Cluster	Firm	Firm	Firm	Firm	Firm

This table presents the first- and second-stage estimates from the main specification using different optimal bandwidths at each side of the cutoff following Calonico et al. (2014). An optimal bandwidth of 10.3% is used to the left of the threshold. An optimal bandwidth of 14.2% is used to the right of the threshold. The first row corresponds to the first-stage regression, while the second row corresponds to the second-stage regression. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variable in the first-stage regression is $Mixed$, which is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. The dependent variable in the second-stage regression is $Credit/Assets$, which corresponds to the ratio of bank credit to assets. $Medium$ is an indicator variable that equals one if the firm is classified as medium, and zero if it is classified as small. $\widehat{Mixed}$, corresponds to the predicted values of $Mixed$ from Equation (2). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

Table A9: Robustness. Eliminate firm-years two years after being classified as medium for the first time

	(1) 10.4%	(2) 8%	(3) 9%	(4) 11%	(5) 12%
Medium	0.135***	0.136***	0.135***	0.136***	0.138***
	(0.035)	(0.039)	(0.037)	(0.034)	(0.032)
$\widehat{Mixed}$	0.321**	0.335**	0.328**	0.320**	0.314**
	(0.142)	(0.161)	(0.152)	(0.137)	(0.130)
Observations	3,475	2,713	3,008	3,647	3,988
R-squared	0.141	0.135	0.137	0.143	0.147
Controls	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes
Region-year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm

This table presents the first- and second-stage estimates from the main specification excluding the sample of firm-years two years after a firm is classified as medium for the first time. The first row corresponds to the first-stage regression, while the second row corresponds to the second-stage regression. The sample period is 2016-2019. Prior to 2016, the reporting and auditing requirements were bundled, preventing their separate identification. The dependent variable in the first-stage regression is $Mixed$, which is an indicator variable that equals one if the firm publicly discloses the mixed or detailed model, and zero if it publicly discloses the abbreviated model. The dependent variable in the second-stage regression is $Credit/Assets$, which corresponds to the ratio of bank credit to assets. $Medium$ is an indicator variable that equals one if the firm is classified as medium, and zero if it is classified as small. $\widehat{Mixed}$, corresponds to the predicted values of $Mixed$ from Equation (2). Column (1) presents the results for firms with size within a 10.4% optimal bandwidth around the binding regulatory threshold. Columns (2)-(5) employ the same specification using alternative bandwidths within 8%, 9%, 11%, and 12% the binding regulatory threshold, respectively. All specifications include firm-level controls as well as industry-year and region-year fixed effects (see Section 5.1 for details). Standard errors are clustered at the firm level. *, **, *** indicate statistical significance at the 1%, 5% and 10% levels, respectively.

B Illustratrative Example of IV and Fuzzy RD Estimation

This section provides a simple example that emphasizes how IV overcomes endogeneity problems present in OLS and illustrates how the IV estimates are derived from the subsample of firms that are sensitive to variation in the instrument.[1] Please note the strong correspondence between IV and fuzzy RDD (Hahn, Todd and Van der Klaauw, 2001).

First, for simplicity assume that the firm's total bank credit is the sum of the quality of the firm (μ), and the effect of disclosing more detailed financial statements, ($Mixed$ in my specific research design), such that:

$$Credit = \mu + \beta * Mixed \tag{B1}$$

Where μ is unobservable, and $Mixed$ is a dummy variable that is equal to one if the firm publicly discloses mixed or detailed financial statements, and zero if it publicly discloses abbreviated financial statements. I am interested in estimating β, which is the causal effect of disclosing more detailed financial statements on the firm's bank credit.

Assume also that the probability of disclosing more detailed financial statements is a function of the quality of the firm such that $Corr(Mixed, \mu) > 0$.[2] This creates the well-known endogeneity problem originating from the omitted variable bias present in standard OLS regressions. In particular, there exist three types of firms: (i) *Alwaystakers*. These are high-quality firms (μ_H) disclosing more detailed financial statements independently of reporting regulation. (ii) *Sensitive* firms. These are average-quality firms (μ_M) that disclose more detailed financial statements in the presence of a mandate, and that do not disclose more detailed financial statements in the absence of a mandate. (iii) *Nevertakers*. These are low-quality firms (μ_L) that choose not to disclose their financial statements irrespective of

[1] This example is based on Bernstein (2015).

[2] For simplicity, in this example, I assume that the firm's quality and the reporting regulation are the only determinants of the financial statements disclosure decision. Note that the literature identifies several determinants influencing this decision (e.g., Minnis and Shroff (2017)), such as the proprietary costs of disclosure.

the regulation. Suppose firms belong to each specific firm type with a probability of 1/3, and suppose that there is a 1/2 probability that each firm is regulated (i.e., being classified as small or medium).[3] A summary of the firm's outcomes is presented in the table below:

	Regulatory Classification	
Firm Type	Medium (1/2)	Small (1/2)
Alwaystaker (1/3)	$Mixed = 1$	$Mixed = 1$
	$\mu_H + \beta$	$\mu_H + \beta$
Sensitive (1/3)	$Mixed = 1$	$Mixed = 0$
	$\mu_M + \beta$	μ_M
Nevertakers (1/3)	$Mixed = 0$	$Mixed = 0$
	μ_L	μ_L

An OLS estimation compares the outcomes of firms disclosing more detailed financial statements ($Mixed = 1$) and abbreviated financial statements ($Mixed = 0$). Thus, β_{OLS} captures not only the effect of more detailed financial statements but also contains omitted variable bias:

$$\begin{aligned}\beta_{OLS} &= E[Credit|Mixed = 1] - E[Credit|Mixed = 0] = \\ &= [\frac{2}{3}(\mu_H + \beta) + \frac{1}{3}(\mu_M + \beta)] - [\frac{1}{3}(\mu_M) + \frac{2}{3}(\mu_L)] = \\ &= \beta + \frac{2}{3}(\mu_H - \mu_L) > \beta\end{aligned} \tag{B2}$$

Therefore, in this example, β_{OLS} overestimates the effect of disclosing more detailed financial statements on bank credit.

To overcome this omitted variable issue, the IV approach exploits quasi-exogenous variation in financial statements disclosure using the reporting regulatory framework. By comparing the bank credit outcomes of more and less regulated firms (i.e., medium and small

[3] In reality, whether a firm is regulated or not is not random. It is solely determined based on firm size (assets, sales, and employees). Controlling for firm size in the RD specification thus restores randomness, because it is the only variable determining assignment into treatment. Consequently, in this example, I assume that whether a firm is regulated or not is quasi-random.

firms), I recover an estimate equivalent to that coming from a reduced-form regression:

$$\beta_{RF} = E[Credit|Medium = 1] - E[Credit|Medium = 0] = \frac{1}{3}\beta \tag{B3}$$

The first-stage estimate captures the effect of the reporting regulation on the probability of disclosing more detailed financial statements, which is driven solely by sensitive firms:

$$\lambda_{FS} = E[Mixed|Medium = 1] - E[Mixed|Medium = 0] = \frac{1}{3} \tag{B4}$$

Thus, the IV estimate rescales the reduced-form estimate by the population that is affected by the instrument (i.e., sensitive firms):

$$\beta_{IV} = \frac{\beta_{RF}}{\lambda_{FS}} = \frac{E[Credit|Medium = 1] - E[Credit|Medium = 0]}{E[Mixed|Medium = 1] - E[Mixed|Medium = 0]} = \beta \tag{B5}$$

Note that β is precisely the difference in the bank credit outcomes of sensitive firms. Hence, this simple example illustrates that the IV estimates (and also the fuzzy RD estimates) exploit the variation in the outcomes of firms that are sensitive to the instrument (the reporting regulation in my setting) (Imbens and Angrist, 1994).

C Difference-in-Differences Specification

In this section, I describe the difference-in-differences specification used in column (3) of Table 5.

In May 2017, the Spanish government increased the reporting size-based regulatory thresholds to be classified as a medium-sized firm, applying to fiscal years from 2016 (included). For fiscal years before 2016, the thresholds to be classified as a medium firm are the following: (i) $Assets = 2.85M$, (ii) $Sales = 5.7M$, and (iii) $Emp = 50$. For fiscal years after 2016 (included), the thresholds are the following: : (i) $Assets = 4M$, (ii) $Sales = 8M$, and (iii) $Emp = 50$. Note that the thresholds requiring mandated financial statement audits remain unchanged.

This institutional framework allows the implementation of both a regression discontinuity design (RDD) and a difference-in-differences approach (DiD). I combine both approaches by estimating the following specification:

$$\begin{aligned} Outcome =& \delta_0 + \delta_1 Medium_{f,t} + \delta_2 Post_t + \delta_3 Medium_{f,t} * Post_t + \delta_4 LDT2_{f,t} \\ &+ \delta_5 LDT2_{f,t} * Medium_{f,t} + \delta_6 LDT2_{f,t} * Post_t \\ &+ \delta_7 LDT2_{f,t} * Medium_{f,t} * Post_t + \lambda_1 f(Size_{f,t}) + \lambda_2 f(Size_{f,t}) * Post_t \\ &+ \alpha_1 X_{f,t} + \alpha_2 X_{f,t} * Post_t + \lambda_{i,t} + \sigma_{d,t} + \epsilon_{f,t+1} \end{aligned} \tag{C1}$$

Where $Outcome$ is either the bank credit to assets ratio of firm f in $t+1$ ($\frac{Credit_{f,t+1}}{Assets_{f,t}}$), or an indicator variable equal to one if the firm f discloses a mixed or detailed model, and zero if it discloses an abbreviated model in t ($Mixed$); $Medium_{f,t}$ is an indicator variable equal to one if the firm is classified as medium, and zero if it is classified as small (according to the size-based thresholds defined after the reform); $Post_t$ is an indicator variable equal to one for fiscal years after 2016 (included), and zero otherwise; $LDT2$ is the least distance to the second threshold, described in section 5.1; $f(Size_{f,t})$ is a size control function that includes $LDT1_{f,t}$ and $LDT3_{f,t}$, which are the least distance to the first and third thresholds, respectively; $I^1_{f,t}$ and $I^3_{f,t}$, which are indicator variables equal to one if the firm exceeds at least one or three thresholds, respectively; and the interactions $LDT1_{f,t} * I^1_{f,t}$ and $LDT3_{f,t} * I^3_{f,t}$; and $X_{f,t}$ is a vector of firm-level controls that includes the current ratio (*Liquidity*), cash over total assets ($Cash/Assets$), equity over total assets ($Equity/Assets$), inventories over total assets ($Inventories/Assets$), EBITDA over sales ($EBITDA$), Ohlson score (*O-Score*), the logarithm of firm age (*Log*(*Age*)), the logarithm of the accounts receivables cycle (*Log*(*AR Cycle*)), and the logarithm of the accounts payables cycle (*Log*(*AP Cycle*)). $\lambda_{i,t}$ and $\sigma_{d,t}$ are industry-year and region-year fixed effects, respectively. Table A1 of the Appendix provides a description of the variables. The coefficient of interest is δ_3, which measures the changes in the outcomes of medium firms following the reform in the size-based thresholds, relative to small firms.

Equation (C1) is estimated using a triangular kernel, which assigns greater weights to observations close to the threshold, and a sample of firms within a 10.4% bandwidth at each side of the threshold. Standard errors are clustered at the firm level.

Note that δ_3 is equal to the difference between the estimates of an RDD specification conducted after 2016 (main analyses) and before 2016 (placebo period), as illustrated in Table 5.